Proceedings of the Institution of Electrical Engineers

Institution of Electrical Engineers

JOURNAL

OF THE

INSTITUTION OF ELECTRICAL ENGINEERS,

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

PUBLISHED UNDER THE SUPERVISION OF THE EDITING COMMITTEE

AND EDITED BY

G. C. LLOYD, SECRETARY.

VOL. 40. 1907-1908.

London:

E. AND F. N. SPON, LIMITED, 57, HAYMARKET, S.W.

New York:

SPON AND CHAMBERLAIN, 123, LIBERTY STREET.

1908.

438

The Institution is not, as a body, responsible for the opinions expressed by individual authors or speakers.

TABLE OF CONTENTS.

VOL. 40.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. 40. 1908. NO. 187.

Proceedings of the Four Hundred and Sixty-Second Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, November 14, 1907—Dr. R. T. GLAZEBROOK, F.R.S., President, in the chair.

The minutes of the Annual General Meeting held on May 24, 1907, were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members :—

James Pigg.

From the class of Associates to that of Associate Members :—

C. A. Baker. | H. L. King.
P. R. Stevenson.

From the class of Students to that of Associate Members :—

W. M. Booth. | R. A. Frank.
L. E. S. Jackson.

Donations to the *Library* were announced as having been received since the last meeting from C. Barus, The British Weights & Measures Association, The Board of Education, F. Broadbent, Messrs. E. E. Brooks & W. H. N. James, The Bulgarian Ministry of Commerce & Agriculture, L. Cohen, Messrs. A. Constable & Co., Ltd., F. H. Davies, L. Diemer-Hansen, H. Diesselhorst, The Engineering Standards Committee, Messrs. Gauthier-Villars, Sir John Gavey, A. Hands, W. Hibbert, Messrs. H. M. Hobart & A. G. Ellis, R. P. Howgrave-Graham, The Institution of Civil Engineers, Messrs. W. Jaeger & St. Lindeck, A. E. Kennelly, C. G. Lamb, R. Meldola, W. C. Mountain, The National Physical Laboratory, Major W. A. J. O'Meara, H.M. Patent Office Library, H. M. Sayers, G. Schultze, H. Schultze, T. Sewell, J. F. C. Snell, R. Sutton, J. J. Thomson, The Tudor Accumulator Company, Ltd., Vereinigung der Elektrizitätswerke, R. Wade, T. Wall, C. J. H. Woodbury; to the *Building Fund* from The Students' Committee, F. H. Webb; and to the *Benevolent Fund* from L. Birks, C. P. Sparks, A. M. Taylor, F. H. Webb, The Western Electric Company, C. E. Winter, J. H. Wolliscroft, to whom the thanks of the meeting were duly accorded.

The President (Dr. Glazebrook) then presented the Premiums and Scholarships referred to in the Annual Report for the year 1906–7.

The PRESIDENT: Gentlemen, my last duty as the occupant of this chair is to hand to my successor the trust which was conferred on me somewhere about a year ago. May I say on my own behalf that it has been a matter of great interest and great pleasure to me to occupy this chair for the year, that I have felt very deeply the trust and confidence that you have placed in me, and that I have been sincerely grateful for the help and support that I have had from all the members of the Institution. I had hoped that it would have been my high privilege—I can hardly use the phrase to introduce to you—but at any rate to induct into this chair as my successor the father of electrical science in England, Lord Kelvin. As you are aware, that has proved impossible. Lord Kelvin will now be our President, but in consequence of the serious illness of Lady Kelvin, he is unable to leave Scotland and to be with us at present. He has, however, written to me as follows:—

"NETHERHALL, LARGS.,
"*November* 9, 1907.

"DEAR GLAZEBROOK,—

"I wish I could be with you and our comrades of the Electrical Engineers on the 14th. Will you tell them how very sorry I am not to be present at the opening meeting of the Session. The work of Electrical Engineers throughout the world has grown marvellously from year to year, not only in practical importance, but also in profound scientific interest. From the time, in the middle of the nineteenth century, when

their profession had its beginning with the electric telegraph over land and under sea, workers of all grades of Electrical Engineering have kept closely in touch with purely scientific investigation, which has indeed been largely advanced by their labours. The work of the present Session of the Institution will surely be carried out on the lines which have made its meetings so interesting and valuable ever since 1871, when it came into existence as the Society of Telegraph Engineers. Give the members of the Institution my best wishes for all their work, both in the Institution and in carrying out its practical objects, and tell them that I hope to be with them before the end of the present Session, and that in the meantime I thank them warmly for their great kindness in allowing me to be their President.

"Yours very truly,

"KELVIN."

In Lord Kelvin's absence, it is my privilege, as prescribed by the Articles of the Institution, to ask our senior Vice-President, Mr. Charles P. Sparks, to take the chair, and I now have great pleasure in doing that.

The chair was then vacated by Dr. Glazebrook, and taken for the remainder of the meeting by Mr. C. P. Sparks, Vice-President.

The CHAIRMAN (Mr. Charles P. Sparks): I think our first action should be to pass a vote expressing our deep sense of regret at the unavoidable absence of our President. We have heard from our past President, Dr. Glazebrook, the reason why Lord Kelvin is not able to be with us to-night, and I now ask leave to move the following resolution: "That we, the members of the Institution assembled in meeting at the opening of the Session, do hereby express our deep sense of regret at the unavoidable absence of our President, Lord Kelvin, on account of the illness of Lady Kelvin. We also beg leave to tender our sincere sympathy with him in his anxiety, and our best wishes for her ladyship's speedy restoration to health."

The resolution was carried by acclamation.

Professor S. P. THOMPSON, F.R.S. (Past President): The duty of acting as the mouthpiece of the Institution on this occasion and giving voice to the gratitude that we all feel to the President who has just vacated the chair, is usually entrusted to one of the Past Presidents. I think, during the last fifteen years or so, on the majority of such occasions it has fallen to Sir William Preece to move this vote of thanks on behalf of the Institution. I regret Sir William Preece is not with us to-night once more in the name of the Institution to thank its Past President. We have had in Dr. Glazebrook during the last twelve months a very admirable President. He has never failed in the due discharge of the duties of the chair; he has presided with dignity and urbanity. We have all learned to appreciate him not only in his

capacity as President, but for the sake of his own personal qualities. We honour him as our Past President; we honoured him also when we elected him as President, because of the important office which he fulfils as Director of the National Physical Laboratory. He has presided over our business, he has been incessant in attending committees and Council, and we certainly cannot do less than convey to him our hearty thanks. The motion is, "That the best thanks of the Institution of Electrical Engineers be given to Dr. R. T. Glazebrook, F.R.S., for the very able manner in which he has filled the office of President during the past twelve months."

Mr. W. M. MORDEY: Professor Thompson in making this proposal has said everything that I wanted to say, and said it much better than I could say it. But I do not want merely to second the motion without at least expressing my own personal concurrence in everything that Dr. Thompson has said. In our past President we have a distinguished scientific man who, although not a professional engineer in the ordinary narrow sense, is an engineer in a very real sense. He is an expert in applying scientific knowledge to engineering purposes, being endowed at the same time with the important qualities of an excellent administrator and business man. He has, as Dr. Thompson says, been indefatigable in his duties—not only the duties that the members have seen him carrying out, and with regard to which they can judge for themselves, but in innumerable ways, in the Council room, and elsewhere, in which the President is called on to give his time to the work of the Institution. He has represented the Institution on all occasions with dignity and effectiveness, and he has endeared himself personally to all with whom he has had to work during his year of office. I have had very much pleasure indeed in seconding this proposal.

The resolution was carried by acclamation.

Dr. R. T. GLAZEBROOK: May I in a very few words thank you, Professor Thompson, Mr. Mordey, and the members of the Institution, for the extremely cordial way in which the vote of thanks to me has been proposed, and for the way in which it has been received. I shall value very greatly the recollection of the year that I have spent in this office. I shall value it for the experience and the knowledge I have gained myself; I shall value it more for the friendships that I feel I have made among the members of the Council, and I hope among all the members of the Institution. You, Professor Thompson, have been kind enough to speak in warm and cordial terms of what I have been able to do. A great part of that has depended on the fact that I have received such cordial assistance from the members of the Council and from the officers of the Institution; and without that any efforts of mine would have been of no avail.

Before I sit down I should like, with your permission, to refer to two events of the past year which are, I think, of some importance. In the first place, with this year a somewhat novel procedure as to dealing with papers to be read before the Institution is to be inaugu-

rated. Hitherto, the Local Sections of the Institution have acted without any very close co-operation amongst themselves, or without any very well organised endeavour to work together for the whole benefit of the Institution. That matter has been under careful thought and consideration during the past year, with the result that it appeared that our plan of procedure in dealing with papers might be modified so as to strengthen the Institution, and obtain for each member more of the value of the work of individual members. In the past, papers have been read at the Local Sections and then have been sent up to London, where they have come before the Papers Committee and the Council, and after discussion it has been decided whether or not they were to appear in the *Journal*. By the new arrangements that procedure is to be altered. The constitution of the Papers Committee has been changed, and it now consists of representatives of all the Local Sections as well as of representatives of the Council. Every paper which is to be read before the Institution or before one of the meetings of the Local Sections will come before that Committee before being read. The Committee will then make recommendations to the Council as to which of all these papers should be selected for reading and discussion at a general meeting in London. Those papers and all the other papers will be open to all the Local Sections for inclusion in their programmes, and an endeavour will be made to read them in the various centres as nearly as possible at the same time as they are read in London, so that the discussion of them will take place as nearly as possible simultaneously. By this means it will be possible to focus round any point of real interest or novelty all the talent of the members of the Institution, and it is hoped that in that way our work will be strengthened, that it will be made more useful. I am glad to recognise the way in which the Local Sections are coming forward and co-operating with the Council in that work. You will hear a little later in the evening the list of papers that is passed for the present Session, and then you will feel, I am sure, that this new scheme has begun under extremely good auspices, and is likely to lead to results of very real value.

There is one other modification of procedure which I have been allowed to notify. Arrangements have now been made that all papers which are to be read before the Institution shall be read in abstract, that the abstract shall be handed in to the Secretary ten days before the meeting, so that the President may have the opportunity of going through it and of realising exactly what the points are that are coming up for discussion, and thus arranging more easily, readily, and successfully than can be done now the order of the discussion. I trust you will find that those two points of procedure which have been altered during the past Session will tend to the advancement of the Institution.

Now, finally, let me again thank you for the confidence that you have placed in me as your President, and for the help you have given me in endeavouring to promote all the interests of the Institution.

The following paper was then read and discussed :—

THE DIELECTRIC STRENGTH OF INSULATING MATERIALS AND THE GRADING OF CABLES.

By ALEXANDER RUSSELL, M.A., Member.

(Paper received April 15th, revised Sept. 9th, and read in London Nov. 14th, 1907.)

1. *Introduction.*—In power transmission, whether by direct or alternating current, the saving in copper effected by using very high pressures has directed the attention of manufacturers to the construction of cables which will withstand pressures of 100 kilovolts and upwards. To design cables which will successfully withstand these pressures a knowledge of the electric stresses to which the various insulating materials round the core will be subjected under working conditions is essential as well as an accurate knowledge of the dielectric coefficients, dielectric strengths and resistivities of the insulating wrappings. In what follows the laws of disruptive dis-

charge are first discussed, next, the methods of measuring the dielectric strengths of gases, liquids, and solids, and finally, the electric stresses on the insulating materials of a single-core cable, with special reference to the grading of cables.

In Mr. O'Gorman's paper* valuable suggestions are made for the scientific grading of cables, and a useful table is given of the values obtained by various experimenters for the physical data of many of the ordinary insulating materials used in practice. Some of the numbers given, however, must be taken only as rough first approximations, as the temperature of the material, the shape of the testing electrodes, etc., are not stated.

In the classical paper† read by Mr. Jona to the International Congress at St. Louis in 1904 will be found many important theorems in connection with the manufacture of high-tension cables. In other papers ‡ he gives very valuable data on the dielectric strengths of oils, etc., and of liquid air. One of Mr. Jona's "graded" cables working at 25 kilovolts was shown by Messrs. Pirelli & Co. at the Paris Exhibition in 1900. Last year also, at the Milan Exhibition, his cables were shown working at 75 kilovolts.

2. *The Stress on the Dielectric.*—The best way of picturing what happens in a dielectric is by means of Faraday's tubes of force. We picture one end of one of these tubes anchored to a unit positive charge on the positive electrode and the other end anchored to a unit negative charge on the other electrode. By the resultant force at a point is meant the force with which a unit positive charge placed at the point would be urged if it could be placed there without disturbing the distribution elsewhere. It follows at once from the definition of potential that the resultant force at a point in a dielectric is equal to the rate at which the potential diminishes as we move along the line of force through the point. It is measured, therefore, by the potential gradient in the direction of the resultant force at the point, and this is the term that electricians customarily employ. It is also equal to 4π times the number of Faraday tubes which pass through unit area of the equipotential surface at the point. When regarded in this way it is generally called the electric intensity at the point. It has to be remembered when reading the literature of the subject that "the resultant electric force," "the potential gradient," and "the electric intensity" are all used to denote the resultant electric stress at a point.

3. *Dielectric Strength.*—The dielectric strength of an isotropic insulating material in a given physical condition is the maximum value of the electric stress which it can withstand without breaking down.

4. *Faraday's Criterion for Disruptive Discharge.*—In his "Experimental Researches in Electricity," vol. i., p. 436, Faraday states that "discharge probably occurs, not when all the particles have attained to a certain degree of tension, but when that particle which has been

* *Journal Institution of Electrical Engineers*, vol. 30. p. 608, 1901.
† *Transactions of the International Electrical Congress, St. Louis*, vol. 2, p. 550.
‡ *Atti della Associazione Elettrotecnica Italiana*, vol. 6, p. 396, and vol. 11, p. 47.

most affected has been exalted to the subverting or turning point." He considered it probable, therefore, that the disruptive discharge would ensue when the electric intensity at the point subjected to the maximum stress attains a definite value. It is now well known that part of an insulating material can break down without a disruptive discharge necessarily ensuing. For instance, when brush discharges occur from an electrode in air part of the air surrounding the electrode has become a true gaseous electrolyte, and its insulativity has therefore broken down. The air at the boundary of this electrolyte has not broken down because the electric stress to which it is subjected has not reached the "subverting" value.

It is not therefore safe to conclude that a disruptive discharge ensues the moment the maximum electric stress at any point of the dielectric between the two electrodes attains the breaking-down value for that dielectric. The breaking down of a portion of the dielectric may relieve the electric stress on the remainder. A disruptive discharge ensues only when the breaking down of part of the material leads to the electric stress on the remaining part being greater than it can withstand.

5. *The Maximum Electric Stress between Equal Spherical Electrodes.*—The easiest way of finding the dielectric strength of insulating materials is by finding the disruptive voltage between two equal spherical electrodes embedded in the material. In a previous paper I have shown* that, if the spheres be at a less distance apart than twice the diameter of either, a disruptive discharge will ensue the moment the maximum electric stress between the spheres equals the dielectric strength of the material. In order to calculate the maximum electric stress at the instant of discharge we must know the potential and size of each sphere and the distance between them.

Let a be the radius of each sphere and let x be the minimum distance between them. Let us first suppose that one sphere is at the potential V_1 and that the other is at zero potential. In this case the maximum electric stress, $R_{max.}$, between them is given by—

$$R_{max.} = (V_1/x)f_1,$$

where the values of f_1 can be found from Table II. A proof of this formula is given in my paper above referred to. In the important practical case when $V_1 = -V_2 = V/2$, where V_2 is the potential of the second sphere, we have—

$$R_{max.} = (V/x)f,$$

where f can be found from Table I.

In general, if V_1 and V_2 be the potentials of the two spherical electrodes, and V_1 be not numerically less than V_2, we have—

$$R_{max.} = \{(V_1 - V_2)/x\} f_1 + 2(V_2/x)(f_1 - f),$$

where f and f_1 are functions of x/a, the values of which can be found from Tables I. and II.

* *Phil. Mag.* (6), vol. 11, p. 258, 1906.

Hence by this formula the dielectric strength of the material can be calculated from the potentials of the electrodes at the instant of the disruptive discharge.

TABLE I.—VALUES OF f.

x/a.		x/a.	f.
0·0	1·000	2	1·770
0·1	1·034	3	2·214
0·2	1·068	4	2·677
0·3	1·102	5	3·151
0·4	1·137	6	3·632
0·5	1·173	7	4·117
0·6	1·208	8	4·604
0·7	1·245	9	5·095
0·8	1·283	10	5·586
0·9	1·321	100	50·51
1·0	1·359	1,000	500·5
1·5	1·559	10,000	5,000·5

TABLE II.—VALUES OF f_1.

x/a.	1.	x/a.	f_1.
0·0	1·000	2	2·339
0·1	1·034	3	3·252
0·2	1·068	4	4·201
0·3	1·106	5	5·167
0·4	1·150	6	6·143
0·5	1·199	7	7·125
0·6	1·253	8	8·111
0·7	1·313	9	9·100
0·8	1·378	10	10·091
0·9	1·446	100	100·0
1·0	1·517	1,000	1,000
1·5	1·909	10,000	10,000

6. *The Dielectric Strength of Air.*—In the following table the values of x and V are taken from Dr. Zenneck's work, "Elektromagnetische Schwingungen und Drahtlose Telegraphie," 1905, p. 1011. They are due to J. Algermissen, and are deduced from the average of the values obtained on different days under varying conditions. It has been assumed that the potentials of the electrodes are $+V/2$ and $-V/2$ respectively at the instant of discharge. As the results in the last column are very approximately constant the assumption is justified :—

TABLE III.

J. Algermissen. 5-cm. spheres ($a = 2·5$). x is measured in centimetres and V in kilovolts.

x.	x/a.	f (calc.).	V (obs.).	$R_{max.}$ (calc.).
2·0	0·80	1·283	58·2	37·3
2·2	0·88	1·312	62·8	37·5
2·4	0·96	1·342	67·0	37·5
2·6	1·04	1·374	70·8	37·4
2·8	1·12	1·406	74·4	37·4
3·0	1·20	1·437	78·0	37·4
3·2	1·28	1·469	81·3	37·3
3·4	1·36	1·500	84·7	37·4
3·6	1·44	1·533	88·0	37·5
3·8	1·52	1·566	91·2	37·6
4·0	1·60	1·599	94·2	37·7
4·2	1·68	1·632	97·2	37·5

In the above table $R_{max.}$ has been calculated by means of the formula—

$$R_{max.} = (V/x)f.$$

From the above results, and from many other experimental results obtained with both alternating and direct pressures, the author concludes that the dielectric strength of air under normal conditions is about 3·8 kilovolts per millimetre.

The practical constancy of the dielectric strength of air under ordinary atmospheric conditions is recognised in the Standardisation Rules (1907) of the A.I.E.E. For instance, in § 243, when discussing the value of the spark-gap safety-valve, it is stated that "a given setting of the spark-gap is a measure of one definite voltage, and, as its operation depends upon the maximum value of the voltage wave, it is independent of wave-form, and is a limit on the maximum stress to which the insulation is subjected. The spark-gap is not conveniently adapted for comparatively low voltages." The reason for the limitation given in the last sentence of the above quotation will be discussed in the next section.

In Appendix D of the American Rules, a table of the sparking distances in air between "opposed sharp needle-points" for sine-shaped voltage waves is given. It is stated, for example, that when they are 7·5 cm. apart the disruptive pressure is 45 kilovolts. It is interesting to notice that if we have two spherical electrodes, each 2 cm. in diameter, and if the distance between their centres be 7·5 cm., so that the minimum distance between the spheres is 5·5 cm., the disruptive voltage is 44 kilovolts, which is practically the same as that between two needle-points 7·5 cm. apart. As brush discharges from the needle-points occur at pressures much less than the disruptive

voltage, it will be seen that the ionised air round the points at the instant of the disruptive discharge is probably roughly spherical in shape and 1 cm. in radius. This is a good illustration of the following sentence in Faraday's "Experimental Researches," § 1499: "But, as has long been recognised, the small body is only a blunt end, and, electrically speaking, a point only a small ball; so that when a point or blunt end is throwing out its brushes into the air, it is acting exactly as the small balls have acted in the experiments already described, and by virtue of the same properties and relations."

7. *Failing Cases in Practice.*—The above formulæ cannot be applied in practice when the electrodes are at microscopic distances apart. G. M. Hobbs * has shown that when x is less than $3\ \mu$ (where $\mu = 10^{-6}$ metre) the sparking potentials are practically independent of the nature of the gas between the electrodes. They depend, however, on the metal of which the electrodes are made. When the electrodes are very close together, it has to be remembered that our assumption of an isotropic medium bounded by smooth rigid equipotential surfaces is no longer permissible. If the surfaces were magnified sufficiently they would be seen to be rough, and the dielectric surrounding the microscopic projections would probably be ionised. In these circumstances, therefore, accurate calculations would be difficult.

Hence in determining dielectric strengths it is necessary to have the electrodes at appreciable distances apart, and therefore high voltages must be used. It is not safe to calculate dielectric strengths from the observed disruptive voltages when the electrodes are less than a millimetre apart. When a maximum inaccuracy of more than 1 per cent. is not permissible, they should be at least half a centimetre apart.

It has also to be remembered that the formulæ for the maximum value of the electric stress on the medium between spherical electrodes have been obtained on the assumption that the Faraday tubes are in statical equilibrium. In the case of impulsive rushes of electricity, or with alternating pressures at exceedingly high frequencies, the disruptive voltages seem to be independent of the shape of the electrodes.

8. *Measuring the Dielectric Strength of Gases.*—The dielectric strength of a gas may be deduced from experiments on the sparking voltages between spherical electrodes. The containing vessel for the gas should be large with the spherical electrodes near the centre. The diameter of the supporting rods should be small compared with the diameter of the electrodes, and care should be taken that no conducting materials or insulating materials having dielectric coefficients different from the gas are in the immediate vicinity of the electrodes, otherwise the distribution of the Faraday tubes between the electrodes will be altered and our formulæ will not apply. It is usually best to earth the middle point of the secondary coil of the transformer, or the middle point

* *Phil. Mag.* [6], vol. 10, p. 617, 1905.

of the batteries used, so as to make the potentials of the electrodes equal and opposite at the instant of discharge.

If $E/2$ and $-E/2$ be the potentials of the electrodes, at the instant of discharge, when direct voltages are used, we have—

$$R_{max.} = (E/x)f,$$

where $R_{max.}$ is the dielectric strength of the gas, x the minimum distance between the electrodes, and f a number which can be obtained from Table I. The nearest points on the electrodes should not be closer than about half a centimetre, and their diameter should be about 5 cm. With air at atmospheric pressure a voltage slightly less than 20 kilovolts would be required when x was 0·5 cm.

When alternating pressures are used it is absolutely necessary to know the ratio of the maximum voltage E to the effective voltage V. Let this ratio, which is sometimes called the amplitude factor, be denoted by k, then our formula is—

$$R_{max.} = (k\,V/x)f.$$

Steinmetz's method* of putting the electrodes into nitrate of mercury and rubbing them with a clean cloth before and during the experiments is to be commended. This is especially necessary when the electrodes are only a small distance apart.

The pressure, temperature, and humidity of the gas must be given.

J. N. Collie and W. Ramsay† give interesting comparative values of the sparking potentials for various gases contained in glass tubes. The electrodes were of platinum with slightly rounded ends. Owing to the dielectric coefficient of the glass tube being different from that of the gas, and owing to the great electric stress at the electrodes causing excessive ionisation, absolute values cannot be found from their results, but the following table shows that the dielectric strengths of the gases differ considerably:—

Gas.	Sparking Distances in mms.
Oxygen	23
Air	33
Hydrogen	39
Argon	45·5
Helium ...	greater than 250

The applied voltage being the same in all the experiments, it is seen that the dielectric strength of helium is extraordinarily low compared with that of the other gases.

9. *Dielectric Strength of Liquids.*—The liquid to be tested is generally placed in a vertical glass cylinder about 2 in. in diameter. Spherical electrodes about half an inch in diameter are immersed in the liquid, and the distance between them is varied by means of

* *Transactions of American Institute of Electrical Engineers*, vol. 15, p. 281 (1898).

† *Proceedings of the Royal Society*, vol. 59, p. 257, 1896.

a micrometer screw. The formulæ for deducing the dielectric strength from the disruptive voltage are the same as for a gas.

The electrodes should not be less than 0·3 of a centimetre apart, and at this distance 40 or 50 kilovolts will be required to break down good insulating oils. In some cases when moisture is present much smaller voltages suffice.

In order to find the true dielectric strength of an oil, it is necessary to dry it thoroughly before the test. This can be done by letting hot air bubble up through it. It is inadvisable, however, to heat the oil above 100° C. as considerable discolouration often results and its physical state alters. When oils are dried in this way* perfectly consistent results can be obtained.

As a numerical example, let us suppose that the disruptive voltage for an oil between 1 cm. spherical electrodes, 0·3 of a centimetre apart, is 28 kilovolts, V_1 being equal to $-V_2$, and the amplitude factor being 1·5. By Table I. we get—

$$R_{max.} = (1·5 \times 28/0·3) \times 1·21$$
$$= 168 \text{ kilovolts per centimetre.}$$

10. *Dielectric Strength of Isotropic Solids.*—If the spherical electrodes can be entirely embedded in the insulating material then we can proceed as for liquids and gases, the same formulæ being employed.

The method frequently adopted of putting thin sheets of the insulating material between metal electrodes in air is of doubtful value. As the voltage is increased the air surrounding the electrodes is broken down long before the disruptive voltage is reached. The insulating material heats excessively, and the maximum electric stress to which it is subjected cannot be calculated as the temperature is rarely uniform throughout, and the resistivity of the medium and the dielectric coefficient vary with the temperature. Results obtained by neglecting the variations of the electric stress due to temperature are useful only when all the conditions of the experiment are mentioned.

11. *Dielectric Strength of Æolotropic Solids.*—When the insulating material is æolotropic the calculation of the electric stresses is very difficult. They vary with the dielectric coefficients and the resistivities of the various constituents, and, as has just been mentioned, these quantities vary rapidly with the temperature. Accurate measurements of the mean dielectric strength are therefore in many cases almost impossible.

It is the opinion of many of the engineers connected with manufacturing companies that the testing pressures sometimes specified by consulting engineers are too high, and that the times for which they are to be applied are too long. Most insulating materials are composed of organic matters, and are therefore not quite isotropic, and so the effect of applying an excessive pressure to a cable or a piece of electrical apparatus for a considerable time is often to carbonise part

* R. B. Treat, *Electrical World*, vol. 49, p. 441 (1907).

of the dielectric. This permanently weakens it mechanically, and may shorten its life considerably.

Mr. J. F. Watson, of Callender's Cable Company, has informed me that he has often noticed when repairing extra high pressure cables that a partial destruction of the wall of insulation has occurred at places quite remote from the fault. This has probably been caused in some instances by "extravagant testing pressures applied for excessive periods." E. Jona in his paper above has also noticed similar phenomena. C. E. Skinner * states that "long-continued tests are liable to produce incipient burning at points within the insulation," and so he considers them very inadvisable for dynamos, etc.

12. *Stresses in a Concentric Cable having an Isotropic Dielectric.*— Let the outer radius of the inner conductor be a and the inner radius of the outer conductor be b. Then the electric stress R at a point in the dielectric distant x from the axis is given by †—

$$R = V/\{x \log_\epsilon (b/a)\},$$

where V is the P.D. between the inner and outer conductors. The value of R is independent of the absolute potentials of the two conductors, and it obviously has its maximum $R_{max.}$ when $x = a$. Hence—

$$R_{max.} = V/\{a \log_\epsilon (b/a)\}.$$

If V and b remain constant—

$$\frac{d}{d\,a} R_{max.} = \frac{V}{\{a \log_\epsilon (b/a)\}^2}\{1 - \log_\epsilon (b/a)\}.$$

Hence if a be less than b/ϵ where ϵ is the base of Naperian logarithms, $R_{max.}$ will diminish as a increases. In this case we see that the breaking down of the dielectric round the inner core actually diminishes the maximum stress to which the dielectric is subjected. It is only when the radius of the charred dielectric gets greater than b/ϵ that a disruptive discharge ensues.

Jona, in his paper to the St. Louis Congress (see § 1), describes an experiment on the disruptive voltages of two single-core cables of very different diameters, but each wound with the same thickness (1·4 cm.) of paper insulation. The core of one consisted of a thin wire 0·1 cm. in diameter, while the other was a copper cylinder 2·9 cm. in diameter. The former broke down at 40 kilovolts, and the latter at from 75–80 kilovolts. The former also got exceedingly hot after being subjected to 30 kilovolts for an hour, whilst the latter was still cold after 50 kilovolts had been applied for the same time. If we calculate the maximum electric stress on the dielectric surrounding the thin wire,

* "Insulation Testing: Apparatus and Methods." *National Electric Light Association* (June, 1905).

† A. Russell, "Alternating Currents," vol. 1, p. 95.

on the assumption that no part of it is broken down before the disruptive discharge ensues, we get—

$$R_{max.} = \frac{40}{0{\cdot}05 \log_\epsilon (1{\cdot}45/0{\cdot}05)}$$
$$= 238 \text{ kilovolts per centimetre.}$$

Similarly, the experimental results with the thick cable make $R_{max.}$ lie between 76·5 and 81·6 kilovolts per centimetre. This experiment is quoted by Jona to show that the ordinary formula cannot be applied when b/a is large.

If, however, we assume that the disruptive discharge does not occur until the outer radius of the charred dielectric becomes equal to b/ϵ, the experiment on the thin wire gives us—

$$R_{max.} = \frac{40 \times 2{\cdot}718}{1{\cdot}45}$$
$$= 75 \text{ kilovolts per centimetre, nearly,}$$

which, being in substantial agreement with the results given by the test on the thick cable, is a striking confirmation of the theory outlined above.

13. *Suitable Dimensions for a Concentric Main.*—Let us suppose that the maximum working voltage V, the density of the current in the inner conductor and the maximum permissible stress to which the dielectric may be subjected, are fixed. Let us first suppose that the inner cylindrical conductor is solid and that its radius is a. If, then, V/d be the maximum permissible stress, we have—

$$\frac{V}{a \log_\epsilon (b/a)} = \frac{V}{d},$$

and thus—

$$b = a\, \epsilon^{d/a}.$$

Hence also—

$$\frac{d\,b}{d\,a} = \epsilon^{d/a}\left(1 - \frac{d}{a}\right).$$

If, therefore, a be greater than d, $d\,b/d\,a$ is positive, and therefore b increases as a increases, but if a be less than d, b diminishes as a increases. In the latter case it would obviously be advantageous to make the inner conductor hollow, its section remaining constant, so as to increase the value of a and diminish the value of b. The quantity of armouring and insulating material used would be diminished by this procedure. We conclude, therefore, that if a solid inner conductor of the required cross-section would have a radius less than d, the inner conductor should be made hollow and its outer radius should not be less than d. In some cases it would be advantageous to make the inner conductor of aluminium.

Although the inner radius of the outer conductor begins to increase when a gets greater than d, the following reasoning shows that the

quantity of the dielectric required diminishes until a gets greater than 1·25 d.

Using the same notation, the area of the cross-section of the dielectric of the cable is $\pi(b^2 - a^2)$, and we have to find the value of a that makes $a^2(\epsilon^{2d/a} - 1)$ a minimum. Differentiating with respect to a and equating to zero, we get—

$$\epsilon^{2d/a} = a/(a-d).$$

Let $a = nd$, then—

$$2/n = \log_\epsilon n - \log_\epsilon (n-1).$$

By trial we find that $n = 1·2544$ ... satisfies this equation, and hence, when n has this value the quantity of insulating material required is a minimum. In this case $a = 1·254d$, $b = 2·784d$, and $b = 2·22\,a$. As the saving of insulating material effected by increasing a from d to 1·25 d is only about 3 per cent. it is of little importance compared with the increased cost of the armouring.

We conclude, therefore, that high-pressure concentric cables, having isotropic dielectrics, for use at a maximum voltage V should be constructed so that $b = a\,\epsilon^{d/a}$, where V/d is the maximum permissible working stress to which the dielectric may be subjected, and a should never be made less than d.

14. *Effect of the Temperature Gradient on the Electric Stress.*—When a concentric main is carrying a current, the temperature of the dielectric is not uniform owing to the heat generated in the inner conductor. If the dielectric is isotropic, the temperature at any point after the flow of heat has become steady can be readily written down, if we assume that the thermal conductivity k of the dielectric remains approximately constant over the range of working temperatures.

If θ be the temperature of all points at a distance r from the axis of the main, we have, since the heat entering per second an elementary cylinder of the dielectric, coaxial with the main, must equal the heat leaving it—

$$\frac{d}{dr}\left(2\pi r k \frac{d\theta}{dr}\right) = 0,$$

neglecting the flow of heat near the ends parallel to the length.

Hence—

$$\frac{d\theta}{dr} = -\frac{A}{r},$$

where A is a constant. We have, therefore—

$$\theta = \theta_2 + A \log (b/r),$$

where θ_2 is the temperature of the outer conductor, the inner radius of which is b.

Let us suppose that the inner conductor, supposed of copper, is

solid and of radius a and that i is the current density in it. Then if σ be the resistivity of the copper in ohms, we have—

$$-4{\cdot}2 \times 2\pi a k \left(\frac{d\theta}{dr}\right)_{r=a} = (\pi a^2 i)^2 \frac{\sigma}{\pi a^2},$$

and thus—

$$A = a^2 i^2 \sigma/(8{\cdot}4\, k).$$

Hence—

$$\theta = \theta_2 + (a^2 i^2 \sigma/8{\cdot}4\, k) \log_e (b/r),$$

and—

$$\theta_1 - \theta_2 = (a^2 i^2 \sigma/8{\cdot}4\, k) \log_e (b/a),$$

where θ_1 is the temperature of the surface of the inner conductor.

We have assumed above that the thermal conductivity k of the dielectric does not vary appreciably with the temperatures likely to occur in practice. C. H. Lees * has proved that this assumption is permissible for paraffin wax, glycerine, and various other insulating materials. There appears to be a slight tendency, however, towards lower conductivity as the temperature increases. G. F. C. Searle † has devised an exceedingly simple method of determining the thermal conductivity of indiarubber, the value of which he finds to equal 0·0004 nearly.

To illustrate the values of $\theta_1 - \theta_2$ likely to occur in practice, let us suppose that $b = 1{\cdot}649$ cm., and $a = 1$ cm. Let us also suppose that the current density i is 150 amperes per sq. cm., that $\sigma = 1{\cdot}8 \times 10^{-6}$, and that $k = 0{\cdot}0006$. I have no trustworthy data with reference to the conductivities of the dielectrics used in actual cables, and so I take the value of k for paraffin wax, which has been found accurately by Lees. Substituting in the formula, we get—

$$\begin{aligned}\theta_1 - \theta_2 &= \frac{(150)^2\, 1{\cdot}8 \times 10^{-6}}{8{\cdot}4 \times 0{\cdot}0006} \times \tfrac{1}{2} \\ &= 4^\circ \text{C. nearly.}\end{aligned}$$

It is easy to see from the formula for $\theta_1 - \theta_2$ that for a given value of b and for a given current density, the difference of temperature between the inner and outer conductors is a maximum when

$$a = b/\sqrt{\epsilon} = b/1{\cdot}649 = 0{\cdot}6065\, b,$$

which is the case we considered. We see, therefore, that the difference of temperature between the inner and outer conductors is probably not greater than 10 deg. in the most unfavourable circumstances.

It is known that the dielectric coefficient and the electric resistivity of an insulating material vary rapidly with the temperature. Jona, in his paper already quoted, mentions a case where a rise of temperature of 20° C. made the insulation resistance of a paper insulated cable fall

* *Phil. Trans.*, vol. 204 A, p. 433, 1905.
† *Proceedings of Cambridge Philosophical Society*, vol. 14 (2), p. 190, 1907.

to one-thirtieth of its original value, and even more striking instances could be given. The question of the variation of the dielectric coefficients of dry paper and of solid cellulose has been investigated very thoroughly by A. Campbell.* The following table for oven-dried cellulose is taken from his paper :—

Temperature Centigrade.	Dielectric Coefficient.	Resistivity 10^6 Megohm-cm.
20	6·7	—
25	—	1,600
30	6·8	900
40	7·0	330
50	7·2	125
60	7·3	40
65	—	20
70	7·5	—

Let us suppose that a steady pressure E is applied across the inner and outer conductors of a concentric main having an isotropic dielectric. The momentary stresses set up initially are the same as if the resistivity were infinite. Now imagine that the dielectric is split up into an infinite number of concentric cylindrical tubes, the material of each tube being at the same temperature. Since these tubes form condensers in series between the conductors, the quantity of electricity per unit length in each condenser will be the same, and thus—

$$\lambda \frac{2\pi r}{4\pi}\frac{dv}{dr} = \text{constant},$$

where λ is the value of the dielectric coefficient at a distance r from the axis and v is the potential at the same distance.

Hence—

$$-\frac{dv}{dr} = \frac{A}{\lambda r},$$

where A is a constant. Now λ diminishes as the temperature diminishes, it therefore diminishes as r increases. We see, therefore, that the effect of λ varying with the temperature is to make the electric stress on the dielectric more uniform.

* *Proceedings of the Royal Society*, vol. 78 A, p. 196, 1906.

If we assume that λ varies with temperature according to the linear law, we may write—

$$\begin{aligned}\lambda &= \lambda_0\left\{1 + a(\theta - \theta_2)\right\}\\ &= \lambda_0\left\{1 + B\log_\epsilon(b/r)\right\},\end{aligned}$$

where $B = a\,a^2\,i^2\,\sigma/8{\cdot}4\,k$. It readily follows that the electric stress is a minimum where—

$$r = b\,\epsilon^{(1 - B)/B}.$$

In practice, B is very small compared with unity, and hence the electric stress diminishes as we pass from the inner to the outer conductor.

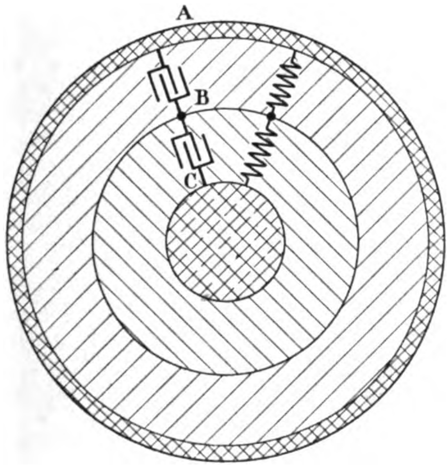


FIG. I.

Let us now suppose that the direct pressure E has been applied sufficiently long to make the electric stresses and the leakage currents assume their steady values. In this case by Ohm's law—

$$dv\Big/\left\{\rho\frac{dr}{2\pi r}\right\} = \text{constant},$$

where ρ is the resistivity in ohms of the dielectric.

Hence—

$$\frac{r}{\rho}\frac{dv}{dr} = -A',$$

or—

$$-\frac{dv}{dr} = \frac{\rho A'}{r},$$

where A' is a constant.

Now ρ increases as the temperature diminishes and therefore as r

increases. The variation of ρ, therefore, due to a slight temperature gradient in the dielectric tends again to make the stress more uniform. But if there be a drop of 10° C. between the inner and outer conductors the electric stresses on the outer layers of the dielectric when the cable is loaded may be much greater than on the inner layers.

It will be seen that the effect of the temperature gradient is to turn the isotropic dielectric into a composite dielectric, and hence it may show the phenomenon of residual charge.

15. *Concentric Main with Composite Dielectric.*—To simplify the formula, let us suppose that the dielectric consists of two layers of different isotropic insulating materials at the same temperature throughout. Let ρ_2, λ_2 be the resistivity in ohms and the dielectric coefficient of the inner layer next the inner conductor, and let ρ_1, λ_1 be the corresponding quantities for the outer layer. Let r be the radius of the cylindrical boundary B between the two wrappings (see Fig. 1). Then if R_1, R_2 be the resistances per unit length to the flow of leakage electric currents across them, and K_1, K_2 be the capacities in farads per unit length of the cylindrical tubes formed by the wrappings, we have*—

$$R_1 = \frac{\rho_1}{2\pi}\log_e\frac{b}{r}; \qquad R_2 = \frac{\rho_2}{2\pi}\log_e\frac{r}{a};$$

$$K_1 = \frac{\lambda_1}{2\log_e(b/r)} \times \frac{1}{9\times 10^{11}}; \quad K_2 = \frac{\lambda_2}{2\log_e(r/a)} \times \frac{1}{9\times 10^{11}}.$$

Now the leakage current i_R across an isotropic dielectric is in phase with the P.D. applied at its boundaries, and the capacity current i_K is 90° in advance of this P.D. If v', v, and v'' denote the instantaneous values of the potential of the outer conductor, the boundary between the two media, and the inner conductor respectively, we have—

$$i'_R = \frac{v'-v}{R_1}; \qquad i''_R = \frac{v-v''}{R_2};$$

$$i'_K = K_1\frac{d}{dt}(v'-v); \quad i''_K = K_2\frac{d}{dt}(v-v''),$$

where i'_R, i''_R denote the leakage currents, and i'_K, i''_K the capacity currents in the media between A and B, and between B and C respectively. We also have—

$$i'_R + i'_K = i''_R + i''_K = i,$$

since the sum of the leakage and capacity (displacement) currents in each medium must equal the total current flowing across the dielectric.

Let V_1, V_2 denote the effective values of $v'-v$ and of $v-v''$, and let ϕ_1 and ϕ_2 denote respectively the phase-difference between V_1 and V_2, and the effective value of i. Then if f be the frequency, and $\omega = 2\pi f$, we have—

$$\left.\begin{aligned} \tan\phi_1 &= \omega K_1 R_1 = f\lambda_1\rho_1/(18\times 10^{11}), \\ \text{and—}\quad \tan\phi_2 &= \omega K_2 R_2 = f\lambda_2\rho_2/(18\times 10^{11}). \end{aligned}\right\} \quad \dots\dots \quad \text{(A)}$$

* A. Russell, "Alternating Currents," vol. 2, p. 458.

If, therefore, $\lambda_1 \rho_1 = \lambda_2 \rho_2$, V_1 and V_2 are in phase with one another, and thus—

$$V = V_1 + V_2,$$

where V is the effective value of the applied P.D. In general, however, $\lambda_1 \rho_1$ is not equal to $\lambda_2 \rho_2$, and therefore $V_1 + V_2$ must be greater than V.

Now by reciprocating the well-known formula *—

$$\frac{A_1}{A} = \frac{\left\{R_2^2 + L_2^2 \omega^2\right\}^{1/2}}{\left\{(R_1 + R_2)^2 + (L_1 + L_2)^2 \omega^2\right\}^{1/2}},$$

for the currents in a divided circuit, we get—

$$\frac{V_1}{V} = \frac{\left\{1/R_2^2 + K_2^2 \omega^2\right\}^{1/2}}{\left\{(1/R_1 + 1/R_2)^2 + (K_1 + K_2)^2 \omega^2\right\}^{1/2}} \quad . \quad . \quad . \quad . \quad (1),$$

the formula for the voltages across leaky condensers in series.

By differentiating this expression with respect to ω it is easy to see that V_1 increases with ω when $\lambda_2 \rho_2$ is greater than $\lambda_1 \rho_1$. In this case the electric stresses in the medium next the outer conductor increase as the frequency increases, and the stresses in the inner medium diminish. If $\lambda_2 \rho_2$ were less than $\lambda_1 \rho_1$ the converse effects would take place.

We see from (1) that, when $\lambda_2 \rho_2$ is greater than $\lambda_1 \rho_1$, V_1/V has its mimimum value when ω is zero, that is, with steady pressures, and its maximum value when ω is infinite, that is, with an alternating voltage of very high frequency.

16. *Numerical Example.*—Let us assume that the radius of the inner conductor is 1 cm. ($a = 1$), the radius of the boundary 1·5 cm. ($r = 1·5$), and the inner radius of the outer conductor 2·25 cm. ($b = 2·25$). Let us also assume that for the outer jute wrapping, $\rho_1 = 10^{12}$, $\lambda_1 = 2$, and that for the vulcanised rubber inner wrapping, $\rho_2 = 10^{16}$, $\lambda_2 = 4$. If the direct voltage applied to the conductors be 30,000, then, putting $\omega = 0$ in (1), we find that—

$$V_1 = 0 \text{ and } V_2 = 30{,}000, \text{ very approximately.}$$

Thus practically all the electric stress comes on the rubber.

Let us now suppose that an alternating pressure of very high frequency is applied between the conductors. In this case, putting ω equal to infinity in (1), we get—

$$\frac{V_1}{V} = \frac{\lambda_2 \log_e (b/r)}{\lambda_1 \log_e (r/a) + \lambda_2 \log_e (b/r)} = \frac{2}{3},$$

and thus V_1 is 20,000 volts and V_2 is 10,000. Hence, as the frequency increases from 0 to infinity, V_1 increases from 0 to 20,000, and V diminishes from 30,000 to 10,000 volts.

From (A) we see that—

$$\tan \phi_1 = 10 f/9 \,; \text{ and } \tan \phi_2 = 2 \times 10^5 f/9.$$

* A. Russell, "Alternating Currents," vol. 1, p. 166.

Hence, at the frequencies used in practice, the error made by assuming that ϕ_1 and ϕ_2 are both 90° is small. It readily follows from (1) and (A that—

$$\frac{V_1}{V_2}=\frac{R_1 \cos \phi_1}{R_2 \cos \phi_2},$$

and thus, if f be greater than 9, V_1/V_2 would be equal to 2 very approximately.

In practice, therefore, we see that in the case considered the maximum pressure across the outer layer with alternating pressures may be very much larger than when a direct pressure is applied between the conductors, the value of which equals the maximum value of the alternating pressure between the conductors. On the other hand, the electric stresses on the inner dielectric may be much less with the alternating pressures.

17. *The Electric Stresses with Direct and Alternating Pressures.*—The preceding example illustrates that the electric stresses to which the insulating materials may be subjected with direct and alternating pressures are sometimes quite different. Whether the cables break down sooner with direct or alternating pressures depends on the relative values of the dielectric strengths of the materials which are subjected to the greatest stresses in the two cases. Unless we know the physical constants and the dimensions of the various wrappings, it is impossible to say whether the cable will break down more readily with direct or with alternating pressures of the same maximum value.

In the general case, if—

$$\rho_1 \lambda_1 = \rho_2 \lambda_2 = \rho_3 \lambda_3 = \dots,$$

the stresses with direct pressure will be the same as with alternating of the same maximum value. Even, however, if this relation between the coefficients were approximately true at the start, it would soon cease to be true owing to the heating of the dielectric. Hence the electric stresses will, in the majority of cases, be different with direct and alternating pressures.

At the moment of switching on the direct pressure the distribution of the electric stresses depends on the dielectric coefficients of the wrappings, but after a few seconds the distribution depends on the resistivities.

With alternating pressures, as I have previously pointed out the mere fact that the pressures across the various layers are out of phase with one another may unduly increase the pressure across a particular layer. It is to be noticed also that at the moment of switching on, if the cable is charged, an excessive stress may be thrown on the dielectric. For this reason, especially when testing cables, care should be taken to discharge them before closing the switch.

18. *The Grading of Cables.*—Let us first consider the grading of cables for use on alternating-current circuits. As early as 1898, Jona had constructed single-core cables, the insulation wrappings of which were arranged so that those nearer the core had greater dielectric

coefficients than those more remote. The layers next the core were generally of rubber, and round them were wound layers of paper or jute having smaller dielectric coefficients. The more costly rubber insulation was thus concentrated where its high dielectric coefficient partially relieved the excessive electric stress, and its great dielectric strength enabled it to withstand easily this diminished stress. The value generally accepted for the dielectric strength of pure vulcanised para is 15–20 effective alternating kilovolts per millimetre, or 20–30 direct kilovolts per millimetre.

According to Jona, the value of the dielectric coefficient λ of pure vulcanised rubber is 3. We can increase the value of λ without appreciably weakening the dielectric strength by "loading" it with certain materials. The following data, taken from Jona's St. Louis paper illustrate that λ can easily be varied throughout wide limits—

	λ
58 per cent. para, 26 per cent. talc, 14 per cent. oxide of zinc, 2 per cent. sulphur	4–4·2
64 per cent. para, 16 per cent. talc, 8 per cent. sulphur, 8 per cent. minium, 4 per cent. oxide of zinc	5
55 per cent. para, 22·2 per cent. talc, 22·2 per cent. sulphur ...	6·1

I am indebted to Mr. Jona for a sample of one of his graded cables. This cable was tested with a pressure of 150 kilovolts at the Milan Exhibition without being appreciably affected by the test. The pressure was measured by the ingenious high-tension electrostatic voltmeter devised by Mr. Jona, which seems most useful for measuring these high pressures.

In the sample of the Jona graded cable which I have received, the conductor consists of nineteen strands of copper wire, the diameter of each of which is 3·3 mm. The cross-section of the copper is therefore 162 mm.^{2}. Round this, for reasons explained later, is a close-fitting lead tube, the outer diameter of which is 18 mm. The insulation is built up as follows:—

	Thickness in mm.	λ
First layer. Rubber	2·5 ...	6·1
Second layer. Rubber	2·3 ...	4·7
Third layer. Rubber	4·5 ...	4·2
Fourth layer. Impreg. paper	5·2 ...	4

The total thickness of the insulation is therefore 14·5 mm. ($b/a = 2·61$), and the cable is lead-covered.

If R, R′, R″, and R‴ be the maximum electric stresses on the four layers when the applied pressure is 150 kilovolts, we find by the formulæ given in Appendix A, $R = 124$, $R' = 132$, $R'' = 123$, $R''' = 97·4$ kilovolts per centimetre. If a dielectric of homogeneous substance had been used, the maximum electric stress would have been 174 kilovolts. Hence the grading has reduced the maximum electric stress by about 24 per cent. If air had been the dielectric, a disruptive discharge would have ensued at 23 kilovolts.

O'Gorman, in his paper before referred to, has shown that to get uniform electric stress we must have at all points in the dielectric

$$\lambda\, r = \text{constant},$$

where λ is the dielectric coefficient and r the distance of the point from the axis of the cable. He suggests that by suitably "loading" paper insulation an approximation to this ideal cable might be constructed.

In Appendix A, the formulæ for the grading of direct-current cables are given. When applying direct voltage suddenly to a cable, it has to be remembered that the value of the stresses initially may be quite different from their value after the steady state is reached. In a cable, the dielectric of which consists of different insulating wrappings, the relative values of the voltages between each of these wrappings will initially be inversely as the capacities of the condensers formed by the inner and outer layers of these wrappings, but when the steady state is reached these ratios are as the resistances offered by the wrappings to a radial flow across them, and hence the stresses may be quite different in the two cases. It is therefore advisable to apply the direct pressure to a cable gradually.

19. *The British Standard Radial Thicknesses for Jute or Paper Dielectrics.*—The nominal area of the cross-sections of the conductors and the radial thicknesses ($b - a$) of the dielectric for concentric cables given in the following table are taken from Report No. 7 issued by the Engineering Standards Committee in August, 1904 (p. 8):—

		660 Volts.		11,000 Volts.	
S.	a.	$b - a$.	R_m.	$b - a$.	R_m.
Sq. In.	In.	In.	K.V. per mm.	In.	K.V. per mm.
0·025	0·089	0·08	0·64	0·35	4·3
0·050	0·126	0·08	0·59	0·35	3·7
0·075	0·155	0·08	0·57	0·35	3·3
0·100	0·178	0·09	0·50	0·36	3·1
0·125	0·199	0·09	0·49	0·36	3·0
0·150	0·219	0·09	0·49	0·36	2·9
0·200	0·253	0·09	0·48	0·36	2·7
0·250	0·282	0·10	0·43	0·37	2·6

In this table S represents the cross-sectional area, a the radius of the cylindrical conductor whose cross-sectional area is S, $b-a$ the thickness of the dielectric given by the E.S.C., and R_m the maximum working electric stress when the amplitude factor of the applied alternating pressure is $\sqrt{2}$.

It will be seen that the electric stresses on the dielectric are very different in the high-pressure cable from what they are in the low-pressure cable, and the dielectrics in cables of different sizes are subjected to appreciably different stresses.

In the first five of the high-pressure cables the dielectric surrounding the high-pressure conductor will begin to be broken down before the disruptive discharge takes place, because in these cables the ratio of b/a is greater than ϵ (2·718). The specified thicknesses, therefore, are not economical. Take, for instance, the main in which the nominal cross-sectional area of the conductor is 0·025 sq. in. With a solid cylindrical conductor a equals 0·089 in., and b is, therefo , equal to $0·35+0·089=0·439$ in. Thus $b/a=4·92$. If we make the inner conductor hollow and $a=0·142$ in., $b=0·3865$ in., we get the same maximum stress on the dielectric, but its thickness has been reduced by 33 per cent. and the outer radius by 12 per cent. As the armouring, etc., would also be substantially reduced, the cable would be less costly. If we merely kept $b=0·439$ in., but increased a to 0·1616, so that $b/a=\epsilon$ nearly, then the carrying capacity of the cable would be nearly quadrupled, the thickness of the dielectric diminished 20 per cent., and the maximum electric stress would have been reduced to 3·8 kilovolts per millimetre.

The fact that the dielectrics of cables are not quite isotropic is sometimes advanced as a reason for making the radius of the inner conductor smaller than the value indicated by theory. This practice, however, is founded on a misapprehension, as the effect of diminishing the radius is to increase the electric stress, and there is no reason why dielectrics of heterogeneous substance should be subjected to greater stresses than those of homogeneous substance. The want of isotropy may possibly be a reason for increasing the diameter of the inner conductor, the thickness of the insulating covering remaining the same.

In order to simplify the formulæ for the electric stress, we have assumed that the inner conductor is a smooth cylinder. In practice the inner conductor is nearly always stranded, and it is necessary therefore to consider the effect of the stranding. Owing to the greater curvature of the surface of the strands, we can see, from first principles, that the effect will be to increase the maximum stress. Jona found experimentally that the brush discharges from solid wires and stranded or braided wires having the same external size begin at practically the same voltages. Hence we may infer that the stranding of the conductor does not much affect the dielectric strength of the cable. It is important, however, to be able to calculate the stress exactly, and this can be done by means of a formula due to Professor Levi-Civita (see Jona's paper before quoted). The formula is given in terms of Gauss's hypergeo-

metric series, but Jona has computed these series for useful values of the variables, so that approximate solutions can be readily obtained. The results show that the effect of the stranding is generally to increase the maximum stress on the inner dielectric by about 20 per cent. It is worth while, therefore, to prevent this increase in the stress on the inner wrapping by making the surface of the conductor smooth. This can be done by covering, as Jona does, the inner conductor with a thin lead tube. For extra high-pressure cables the gain in the strength is well worth the slight increase in the cost of the cable.

20. *Conclusions.*—(1) When part of the dielectric under stress breaks down, a disruptive discharge ensues only when the effect of this partial breakdown is to increase the electric stress on the remaining portion.

(2) The dielectric strength of air under given conditions can be found accurately by finding the disruptive voltages between spherical electrodes at distances greater than 0·5 of a centimetre apart. Under normal conditions it is about 3·8 kilovolts per millimetre.

(3) The dielectric strength of other gases can be found in a similar way experimentally by the help of the tables given in § 5. Helium and probably neon break down under comparatively weak electric stresses.

(4) The dielectric strength of oils can be found by noticing the disruptive voltages between spherical electrodes immersed in them, provided that the distance apart is greater than 0·3 of a centimetre. An excellent way of drying oils is by letting heated air bubble through them.

(5) In finding the dielectric strength of solids it is advisable, when possible, to embed the spherical electrodes in the material under test.

(6) High-pressure concentric cables, having an isotropic dielectric, for a maximum working pressure V should be constructed so that—

$$b = a\,\varepsilon^{d/a};$$

where V/d is the maximum permissible working stress to which the dielectric may be subjected, b is the inner radius of the outer conductor, and a is the outer radius of the inner conductor. The smallest permissible value of a is d. When the core is stranded it should be encased in a thin lead tube.

(7) The effect of the temperature gradient in the dielectric of a concentric main, when working, is often to make the electric stress between the two conductors more uniform. Jona's experiments indicate that the dielectric strengths of paper insulated cables do not vary much when the range of temperature does not exceed 60° C. They are probably slightly less at the high temperatures. C. E. Skinner's experiments (above referred to) on glass, treated cloth, mica, etc., show that the dielectric strengths of many insulating materials in the solid form diminish as the temperature rises.

(8) With a composite dielectric subjected to alternating pressures, the P.D.'s across the layers are usually out of phase with one another. It is only in a limited number of cases, however, that the

increase of the stress due to this cause has to be considered, as the leakage currents are usually negligibly small in comparison with the capacity currents.

(9) The effects of alternating and direct pressures in producing stresses in the dielectric are sometimes quite different.

(10) High-pressure cables for alternating- or direct-current circuits should be graded so as to make the maximum electric stress on the dielectric as small as possible, and stranded conductors should be encased in thin lead tubes.

In conclusion, the author has pleasure in acknowledging his personal indebtedness to Mr. Jona, the engineer to Messrs. Pirelli & Co., of Milan, to Mr. C. E. Skinner, of the Westinghouse Company, and to his old pupil, Mr. J. F. Watson, of Callender's Cable Company, for much of the information given in this paper.

APPENDIX A.

Formulæ for the Grading of Single-core Cables.

1. *For Alternating Pressures.*—We shall first make the supposition that all the insulating wrappings used have the same dielectric strength, and that the maximum and minimum stresses to which they are subjected, when working, are to be the same for them all. We shall also suppose that the leakage current across the dielectric can be neglected in comparison with the capacity current. Let us suppose that there are n insulating wrappings the inner radii of which are $a, r_2, r_3, \dots r_n$, respectively, where a is the outer radius of the lead tube encasing the inner core and let b equal the inner radius of the lead sheath. Since the ratio of the maximum to the minimum electric stress is to be the same in all the wrappings we must have—

$$\frac{r_2}{a} = \frac{r_3}{r_2} = \dots = \frac{b}{r_n}.$$

We see, therefore, that the radii should be in geometrical progression, the common ratio being $(b/a)^{1/n}$. The thicknesses of the layers also form a geometrical progression having the same ratio $(b/a)^{1/n}$.

Let $V_1, V_2, \dots V_{n+1}$, be the potentials of points at distances $a, r_2, \dots b$ from the axis of the cable. Then since the layers form n condensers in series the potential difference across a layer will be inversely proportional to the capacity of the layer, and thus we have—

$$\frac{V_1 - V_2}{(1/\lambda_1)\log_e(r_2/a)} = \frac{V_2 - V_3}{(1/\lambda_2)\log_e(r_3/r_2)} = \dots = \frac{V_n - V_{n+1}}{(1/\lambda_n)\log_e(b/r_n)}.$$

Hence, since the P.D.'s are all in phase, each of these ratios equals $V/\left\{\Sigma(1/\lambda_m)\log_e(r_{m+1}/r_m)\right\}$, where V is the voltage applied between the core and the sheath.

If R_m denote the maximum electric stress on the mth layer, we have—

$$R_m = \frac{V_m - V_{m+1}}{r_m \log_e (r_{m+1}/r_m)}$$
$$= (V/\lambda_m r_m)/\Sigma(1/\lambda_m) \log_e (r_{m+1}/r_m).$$

Now, since the maximum stress on every layer is to be the same, we must arrange so that—

$$\lambda_1 a = \lambda_2 r_2 = \ldots = \lambda_n r_n.$$

Therefore—

$$\frac{\lambda_1}{\lambda_2} = \frac{\lambda_2}{\lambda_3} = \ldots = \frac{\lambda_{n-1}}{\lambda_n} = \left(\frac{b}{a}\right)^{1/n}.$$

Hence $\lambda_1, \lambda_2, \ldots \lambda_n$, are the terms of a geometrical progression whose common ratio is $(b/a)^{1/n}$.

If $R_{max.}$ denote the maximum electric stress in the graded cable, we have—

$$R_{max.} = \frac{V}{a}\Big/\left(1 + \frac{\lambda_1}{\lambda_2} + \frac{\lambda_1}{\lambda_3} + \ldots\right) \log (b/a)^{1/n}.$$
$$= \frac{V}{a}\Big/ \frac{b/a - 1}{n\left\{(b/a)^{1/n} - 1\right\}} \log (b/a).$$
$$= R'_{max.} \frac{n\left\{(b/a)^{1/n} - 1\right\}}{b/a - 1},$$

where $R'_{max.}$ stands for $V/a \log (b/a)$ the maximum stress in a cable of the given dimensions with an isotropic dielectric. If $R_{min.}$ denote the minimum electric stress in the dielectric of the graded cable, we have—

$$R_{min.} = R_{max.} (a/b)^{1/n}.$$

In the ideal cable n would be infinite, and thus the stress would be the same at all points, and would equal $V/(b-a)$.

The capacity of a single-core cable, with isotropic dielectric, per unit length equals $\lambda/\left\{2 \log_e (b/a)\right\}$. The capacity of the graded cable equals $\lambda_1 n \left\{(b/a)^{1/n} - 1\right\}/\left\{(b/a - 1) 2 \log_e (b/a)\right\}$. When n is infinite this equals $\lambda_1 a/\left\{2(b-a)\right\}$. If λ be the dielectric coefficient of the cable with the isotropic dielectric, and $\lambda_{max.}$ be the dielectric coefficient of the inner coating of a graded cable having n layers, the capacities of the cables will be equal if—

$$\lambda_{max.} = \lambda (b/a - 1)/n \left\{(b/a)^{1/n} - 1\right\}.$$

If the value of $\lambda_{max.}$ be less than this, the capacity of the graded cable will be the smaller. For example, if there are 4 layers and b/a equals 3, we find that the capacities are equal when $\lambda_{max.} = 1{\cdot}58\,\lambda$. In this case the minimum value of λ in the graded cable is $0{\cdot}69\,\lambda$.

To illustrate how the value of the maximum electric stress diminishes as the number of wrappings is increased we shall work out a few numerical examples.

(i). Two wrappings ($n = 2$)—

b/a	2	3	4	5
$R_{max.}/R_{min.}$ graded dielectric...	1·414	1·732	2	2·236
$R'_{max.}/R'_{min.}$ isotropic dielectric	2	3	4	5
$R_{max.}/R'_{max.}$	0·828	0·732	0·667	0·618
Per cent. increase of the permissible voltage due to grading	21	37	50	62

(ii). Three wrappings ($n = 3$)—

b/a	2	3	4	5
$R_{max.}/R_{min.}$ graded dielectric...	1·260	1·442	1·587	1·710
$R'_{max.}/R'_{min.}$ isotropic dielectric	2	3	4	5
$R_{max.}/R'_{max.}$	0·780	0·663	0·587	0·532
Per cent. increase of the permissible voltage due to grading	28	51	70	88

(iii). Four wrappings ($n = 4$)—

b/a	2	3	4	5
$R_{max.}/R_{min.}$ graded dielectric...	1·189	1·316	1·414	1·495
$R'_{max.}/R'_{min.}$ isotropic dielectric	2	3	4	5
$R_{max.}/R'_{max.}$	0·756	0·632	0·552	0·495
Per cent. increase of the permissible voltage due to grading	32	58	81	102

(iv.) Ideal uniformly graded cable (n = infinity)—

b/a	2	3	4	5
$R_{max.}/R'_{max.}$	0·693	0·549	0·462	0·402
Per cent. increase of the permissible voltage due to grading	44	82	116	149

We have assumed above that the dielectric strengths of all the insulating wrappings are the same. If, however, the dielectric strengths are known accurately and are not all the same, another solution may be preferable. If R_m be the safe working stress for the mth layer, we have—

$$R_m = (V/\lambda_m r_m)/\Sigma(1/\lambda_m)\log_e(r_{m+1}/r_m).$$

Since it is advisable to make the ratio $R_{max.}/R_{min.}$ the same for all layers, the ratio r_{m+1}/r_m will be constant, and as before $r_2, r_3, \ldots r_n$ will be the $n-1$ geometrical means between a and b.

The above equation shows that we must have—

$$\lambda_1 a R_1 = \lambda_2 r_2 R_2 = \ldots = \lambda_n r_n R_n.$$

Since $a, r_2, \ldots r_n$ are in an ascending order of magnitude, $\lambda_1 R_1, \lambda_2 R_2, \ldots \lambda_n R_n$ must be in a descending order. We see, therefore, that it is necessary to put the wrapping whose constants are (λ, R), over the wrapping whose constants are (λ', R'), if λR be greater than $\lambda' R'$, even although λ' may be less than λ.

When, however, the main object we have in view is to make, at all costs, the factor of safety of the cable as high as possible, it is, in general, advisable to put the insulating material having the greatest dielectric strength in contact with the core, and if possible grade the dielectric by using outer layers having smaller dielectric coefficients.

2. *With Direct Pressures.*—Let the leakage current flowing across the dielectric be C, then, using the same notation as before, we have—

$$C = \frac{V_1 - V_2}{(\rho_1/2\pi)\log_\epsilon(r_2/a)} = \frac{V_2 - V_3}{(\rho_2/2\pi)\log_\epsilon(r_3/r_2)} = \ldots = \frac{V_n - V_{n+1}}{(\rho_n/2\pi)\log_\epsilon(b/r_n)},$$

and therefore—

$$C = V/\Sigma(\rho_m/2\pi)\log^\epsilon(r_{m+1}/r_m).$$

Also for the mth layer—

$$\begin{aligned} R_m &= -\frac{dv}{dr} \\ &= \frac{\rho_m}{2\pi r_m} \cdot C \\ &= (\rho_m V/r_m)/\Sigma\left\{\rho_m \log_\epsilon(r_{m+1}/r_m)\right\}. \end{aligned}$$

For reasons stated above, we choose the radii of the boundaries between the wrappings so that they are the $n-1$ geometric means between a and b. Hence, if the factor of safety is to be the same for all the layers, we must have—

$$r_m R_m/\rho_m = \text{constant}.$$

If the dielectric strengths are all equal, this simplifies to—

$$r_m/\rho_m = \text{constant}.$$

These results can be discussed in the same way as the corresponding results for alternating pressures in (A. 1). As the resistivities of the materials used for the dielectric vary rapidly with the temperature, the electric stresses on the various materials at different temperatures will have to be considered.

APPENDIX B.

The Thermal Conductance of the Dielectric.

We shall define the thermal conductance of the dielectric of a single-core cable as the ratio of the flow of heat per second across the dielectric, in gramme-Centigrade units, to the difference in temperature (Cent.) between the outer boundary of the core and the inner boundary of the lead sheath, when the flow of heat has attained its steady state. As the thermal conductivities of insulating materials are very small compared with those of metals, we can assume, without appreciable error, that the metals are at uniform temperatures. If a and b be the inner and outer radii of the dielectric (supposed isotropic) we have (*see* § 14)—

$$\theta = \theta_2 + (\theta_1 - \theta_2)\left\{\log_e (b/r)/\log_e (b/a)\right\},$$

and—

$$Q = -k \cdot 2\pi r (d\theta/dr)$$
$$= 2\pi k \frac{\theta_1 - \theta_2}{\log_e (b/a)},$$

where θ is the temperature of the dielectric at points distant r from the axis of the cable, θ_1 and θ_2 the temperatures of the core and sheath respectively, and Q is the thermal flow per unit length in calories per second. Hence the thermal conductance per unit length for a single-core cable is $2\pi k/\log_e (b/a)$. It is therefore equal to $4\pi k/\lambda$ times the corresponding electrostatic capacity. If r be the electric resistance of the conducting core per unit length and c the current flowing in it, then, when the thermal flow attains its steady value, $Q = C^2 r/4{\cdot}18$, and thus $\theta_1 - \theta_2$ can be readily found if k be known.

We shall now consider a concentric main. Let the conductivity of the isotropic dielectrics between the two conductors and between the outer conductor and the lead sheath be k_1 and k_2 respectively. Then if θ_1, θ_2, and θ_3 be the temperatures of the two conductors and the sheath, we have—

$$Q/2 = 2\pi k_1 (\theta_1 - \theta_2)/\log_e (b/a),$$

and—

$$Q = 2\pi k_2 (\theta_2 - \theta_3)/\log_e (d/c),$$

where $Q/2$ is the heat generated per unit length in each conductor per second and c and d are the radii of the outer dielectric. Hence—

$$\frac{Q}{\theta_1 - \theta_3} = \frac{2\pi}{(1/k_2)\log_e (d/c) + (1/2k_1)\log_e (b/a)}.$$

If we can write $b = c$, and $k_1 = k_2$, without appreciable error, we get $2\pi k_1/\log_e (d/\sqrt{ab})$ for the thermal conductance.

We shall now consider the much more difficult problem of the thermal conductance of a polycore cable. When the dielectric is

isotropic, formulæ for the electrostatic capacity between the cores in parallel and the sheath are given in Russell's "Alternating Currents," vol. i., chap. v. The corresponding thermal conductances can be at once deduced from these formulæ by multiplying the capacities by $4\pi k/\lambda$. In a three-core cable, for instance, of a certain "clover leaf" pattern, if Q be the heat generated in the three cores, per unit length, we have—

$$\frac{Q}{4\pi}=\frac{k(\theta_1-\theta_2)}{(2/3)\log_e\left[\left\{2R^3-(c^3+b^3)\right\}/(c^3-b^3)\right]},$$

exactly, where R is the maximum inner radius of the lead sheath, and b and c are the minimum and maximum distances of points on the cores from the axis of the cable.

By using Kelvin's method of images it may be shown that if the centres of the n cores are symmetrically situated on a circle of radius a, and if the cross-sections of the cores are small circles of radius r, we have—

$$\frac{Q}{4\pi}=\frac{k(\theta_1-\theta_2)}{(2/n)\log_e\left\{(R^{2n}-a^{2n})/n\,R^n a^{n-1} r\right\}},$$

approximately, where R is the inner radius of the lead sheath. When $n=3$, this formula agrees with that given above, provided that r/a and $(a/R)^3$ can be neglected compared with unity.

It is to be noticed that all the logarithms given in this paper are Naperian. Their values are best found directly from Bottomley's Tables. They may also be found with an accuracy sufficient for practical purposes by multiplying the corresponding ordinary logarithms by 2·3. In many cases they may be computed easily, without tables, by the formula—

$$\log_e(b/a)=2x+2x^3/3+2x^5/5+\ldots,$$

where—

$$x=\left\{(b/a)-1\right\}/\left\{(b/a)+1\right\}.$$

For instance—

$$\log_e 1{\cdot}5=0{\cdot}4+0{\cdot}016/3+\ldots$$
$$=0{\cdot}405.$$

For a further discussion of the formulæ in this Appendix see Russell's "Alternating Currents." G. Mie gives diagrams of the lines of flow of heat across the dielectric in polycore cables in a paper in the *Elektrotechnische Zeitschrift*, vol. 26, pp. 137–143, 1905. R. V. Picou has also made valuable and interesting researches on this subject. In particular his paper on the "Capacity and Heating of Underground Mains," printed in *l'Industrie Électrique*, vol. 15, pp. 245 and 281, 1906, will be found helpful.

Discussion.

Dr. J. A. Fleming, F.R.S.: I think the Institution is to be congratulated on receiving from Mr. Russell at the opening meeting of the Session such an interesting paper. It suggests a large number of points for discussion, but I will only touch very briefly on two or three. We may consider the dielectric, as Mr. Russell has done in his paper, as an insulator—that is, as an obstacle to disruption or to sparking—or we may look at it as a means of storing energy, limited by the dielectric strength. We are concerned mostly with the first mode in the case of cables, where we are interested in insulation. The energy storage is then rather a nuisance than otherwise. In the case of condensers the energy storage is of prime importance. It is an interesting fact in connection with dielectrics that the energy they can store is proportional to their volume. Take the case of an air condenser. Imagine two plates, 1 m. square and 1 cm. apart. Let them be charged to the potential which Mr. Russell gives as the dielectric strength of air, 3·8 k.v. per cm.; then the capacity of that condenser is about $\frac{1}{1130}$ microfarads. When it is charged to the breaking-point, the energy storage is 0·64 joule, and this is stored up in 10,000 cub. cms. of air, that is at the rate of 64 joules, or 47 ft.-lbs. per cubic metre of air. If worked out for any other size of air condenser it will be found that it comes to the same value. If we take another dielectric, such as glass, we have a much larger dielectric constant, say, 7, and a much larger dielectric strength varying somewhat with the thickness, roughly speaking, 200 k.v. per centimetre. If we work out the capacities and the energy storage for a glass condenser, with plates 1 m. square and 1 cm. apart, it will be found that in that glass plate condenser we can store 125 joules in 10,000 cub. cms., or 12,500 joules, or 9,125 ft.-lbs. per cubic metre of glass. If we try ebonite, we shall find that 14,000 joules can be stored in a cubic metre, or 9,273 ft.-lbs. per cubic metre of ebonite. In the case of mica or micanite, 90,000 joules can be stored, or 65,000 ft.-lbs. per cubic metre of mica. Of course the question of cost of storage is also important. What does it cost to store up, say, 1,000 joules weight of materials? Taking the ordinary commercial prices of these materials we find as follows: In the case of a condenser with ebonite for the dielectric, to store up 1,000 joules the dielectric will cost £70, irrespective of the plates; if made of micanite it will cost £15; if made of glass it will cost between £4 and £5; if made of air it will cost nothing. The moral is perfectly obvious. The bulkiness of an air condenser is, however, a great obstacle in many cases. Fortunately, the dielectric strength of the air increases almost proportionally to the pressure, so that if we compress air to 14 atmospheres, or 200 lbs. on the square inch, it will have fifteen times 3·8 as its dielectric strength, or something not far from 50 to 60 k.v. per centimetre. That is equal to the dielectric strength of many oils. I think electricians have not yet sufficiently exhausted the possibilities of compressed Dr. Fleming.

Dr. Fleming. air as a dielectric, and I look forward to the time when mains will be laid underground in steel tubes, under compressed air, as a means of transmitting power. Another matter to which Mr. Russell has briefly alluded is the factor of safety in dielectrics. It will be agreed that this is an important matter. If we have to test a cable we must know how much over-pressure it is safe to use. The case is parallel with the case of testing boilers. We want much more information than we have as yet about that point. In the case of glass, for instance, used as condensers it is not safe to work it to more than one-third of its breaking strain. It must have a factor of safety of at least 3. Another interesting question which is very briefly mentioned in the paper is the question of the effect of time of use upon the dielectric strength. In the case of dielectrics there is a phenomenon somewhat analogous to the ageing of iron for transformers. I have had this prominently brought to my notice in the case of glass as a dielectric used in condensers for high-frequency currents. Certain kinds of glass age. The condensers will stand a certain pressure to begin with, but they break down after a certain time. There is room for much further investigation on this matter.

Mr. Nisbett. Mr. G. H. Nisbett: Although unfortunately I must disagree with Mr. Russell in many of the remarks he has made, still I wish to congratulate him on his paper, and to say with what interest it has been read by cable makers. I do not propose to cross swords with the author mathematically, because I am not a foeman worthy of his steel, nor is it necessary, for I can summarise my attitude by saying that I am a critic as to his premises and a sceptic as to his conclusions. A really wonderful mathematical structure has been built upon one little sentence early in the paper, "*Dielectric Strength of Isotropic Solids.*—If the spherical electrodes can be entirely embedded in the insulating material, then we can proceed as for liquids and gases, the same formulæ being employed." Apparently there is no attempt made in the paper to justify the assumption in this sentence. Personally, I do not think that the behaviour of a gas or air, or even a liquid, is at all comparable with that of a solid dielectric under similar circumstances. We have with a solid dielectric apparently nothing equivalent to a brush discharge nor to the ionised layer of conducting material which surrounds a point when it is charged in air. I am further strongly inclined to the opinion that no such effect has been observed as a partial breakdown of a solid dielectric where a layer of air has been entirely absent. I see that Mr. Russell quotes Mr. Watson, as having observed such an effect in high-pressure cables. I have very little experience of cable breakdowns of course, but I have never seen anything of that sort. I think the effect in question can be readily explained by mechanical reasons. The cable was probably impregnated with wax insulation which, on cooling, shrank away from the conductor and left a small air space between the conductor and the solid dielectric; or it may have been due to the bending of the cable, with the consequent separation of the conductor and the dielectric. And here I would say

Mr. Nisbett.

that in my opinion it is very necessary to use a fluid dielectric for high-pressure work, not only to make sure that all interstices are completely filled up to the exclusion of air, but also that the conductor when bent shall not leave the dielectric, for it should be remembered that by the nature of the case the insulation used in cables must be of a yielding character, and that the conductor in the cable has to be bent through that insulation. Consequently, once the cable has been bent, the conductor inside it is never again truly concentric with the sheathing, although it may approximate to it. Then the author says that in the opinion of many engineers too high a testing pressure may be detrimental to the insulation, although no breakdown may occur. I do not agree with this, unless, of course, air is present. For example, it is known that a dielectric will break down with a long continued pressure, which it would readily stand for a few minutes. Supposing it is predetermined that a cable shall break down at, say, 20,000 volts in 2 hours, if pressure be applied for 1¾ hours and the current be then removed, and re-applied within a short interval, that cable will break down in a quarter of an hour. If, on the other hand, the cable be given 24 hours' rest, it can be re-strained again at the same pressure for the whole 2 hours. Further than that, when a cable is very much overstrained in that way, there is a remarkable drop in the insulation resistance. The resistance will drop from hundreds of megohms down to hundreds of ohms shortly before the dielectric punctures, and much more than would be accounted for by temperature effects. But if before the cable punctures the pressure be removed and the cable be given 24 hours' rest, the original insulation resistance will have come back again. This seems to me to prove that no damage does really occur to a solid dielectric unless it is actually punctured.

I would point out in this connection, while talking about test pressures, the need for applying to a cable a pressure in excess of that which will break down a space of air equivalent to the thickness of the dielectric, otherwise the test pressure does not show the presence of any gaps or cracks in the insulation. Coming to the question of the influence of size of conductor on the insulation, I see the author backs his conclusions by quoting Mr. Jona, whose experiment consisted of insulating a thin wire 0·1 cm. diameter in one case and a copper cylinder 2·9 cms. in the other. The smaller cable broke down at 40,000 volts, and the larger one at 80,000 volts. I think this can be readily explained by reasons other than those given by the author. The smaller wire was only 0·04 in. in diameter, and it was wound with more than half an inch radial depth of insulation. It is impossible to put paper to that thickness on to so small a conductor without breaking the paper and cockling it so as to include air spaces, and I think that is the explanation of the earlier breakdown. I myself have recently made an experiment—in fact, since this paper came into my hands—and it points, perhaps quite erroneously, to conclusions exactly opposite to that arrived at by the author. A $\frac{7}{16}$ and $\frac{8}{16}$ cable

Mr. Nisbett. were taken, being practical sizes, and each was insulated with the same quality, thickness, and number of papers. They were dried together on the same drum, they were impregnated together, and they were each covered with thin copper foil. Taking the mean of ten tests on each, the $\frac{7}{16}$ cable broke down at 53,000 volts, while the $\frac{37}{16}$ broke down at 37,700 volts. I do not put any theory forward based on those results. Coming to grading, I have made many experiments on this, and have come to the conclusion that even if there is a fractional advantage theoretically it is entirely outweighed by practical considerations. As an example, one need only refer to Mr. Jona's graded cable, particulars of which are given in the paper. We see there that this cable was built up approximately as to two-thirds of rubber and one-third of paper. The insulation of this cable would cost £700 per mile, but an ordinary ungraded paper cable of the same thickness of insulation would cost £130—not the cable, but the insulation only. It would stand the same test, and that without designing a special voltmeter to measure it by. Further, this compound cable has a practical disadvantage in that its capacity is high and its use would be dangerous on high-tension work. Then, again, we must remember that the dielectric hysteresis loss would also be great, so that we have three practical considerations before us as to why we should not build a graded cable on Mr. Jona's lines. I should like to take this opportunity of saying a word in defence of the cable maker. Many very hard words have been said about him in this room, and naturally he resents it. It has been said that he is unscientific, works by rule of thumb, and wastes material. I would put the position in this way—that the cable maker, as he is situated at present, does not know the circumstances under which his cable is to work. He is not told anything about how it is to be handled, nor as to the temperature at which it has to work, nor the shape of the voltage curve, and last, but not least, the electromotive force to which it is to be subjected. In connection with this last point, the company with whom I am connected supplied some years ago to one of the London lighting companies some 6,000-volt cable, and after it had been at work some time it was discovered that periodically, on the occurrence of a certain set of circumstances, a pressure of about 18,000 volts was momentarily reached. The cable did not break down, but had it done so I am very sure the makers would have had a claim to repair the damage. Therefore, while the present want of knowledge continues on the part of engineers as to exactly what it is they want a cable to do, I suggest that we are very wise, both from the buyer's and the manufacturer's points of view, in providing a very ample margin of safety, and the general success of British underground cable systems shows that this has been done. I hope Mr. Russell's paper will not have the effect of inducing engineers to spend unnecessary money with the cable makers by striving after some theoretical advantage of a microscopical nature when there is already material in the market that is commercially as good as it can at present be made.

Mr. W. H. PATCHELL: In the first place, I wish to thank the author for bringing this paper before us, because it has enabled a great many people in this room, possibly for the first time, to see a cablemaker. We really hardly knew what the real man was like, and wish we saw him oftener. Mr. Russell says that grading is due to Mr. Jona, but the lead sheathing round the copper and the grading of the insulation are patents of our friend Mr. O'Gorman, which were taken out some six years ago. As regards the question of graded cables, when I was in the United States two years ago the engineer in charge of the Niagara Falls Station told me that they had experienced trouble through the breakdown of the 20,000-volt cables in the station, but that trouble was entirely overcome by the use of graded cables. The grading there consisted of varnished cambric round the copper and paper outside that. Subsequently, by the courtesy of the General Electric Company at Schenectady, I had a sample of the cable given to me. I brought it home and showed it to one or two English makers, and asked them why we could not get that sort of cable here, as we were not absolutely free from trouble at even lower pressures, but at present I have not heard anything of it commercially. Mr. Patchell.

Mr. MERVYN O'GORMAN: I cannot but feel grateful to Mr. Russell for reopening the subject of insulation design. It is six to seven years since the question was broached in England, and whereas in Italy, Pirelli, and in Germany Siemens-Schuckert have taken more than an academic interest in it, in this country nothing whatever beyond the manufacture of the original length of cable has been done, and we are told to-day, almost in the same words as we were told seven years ago, that no improvement on cables is possible. This was, I think, the opinion of rubber cable makers when "paper-lead-covered" first came in. Mr. O'Gorman.

To turn to Mr. Russell's interesting paper, much of it is founded on a scientific position which must, I think, be accepted on faith to a certain extent. I refer to the relation of stress and strain in dielectrics to potential gradient. One is accustomed to say, as Mr. Russell says, that the "stress" is the name we give habitually to the potential gradient. If it were only a question of the name we are free to call it anything we like, but what is of moment is the true proportionality between the force upon a small point charged to unit potential and the mechanical tearing asunder of the particles of the dielectric substance in which that point is situated. In air it is true that we have experimental proof of a straight line law connecting disruptive distances and potential gradient for all distances except, perhaps, those of the order of the wave-length of light. In some other substances there is an indication that such may be the law, but as regards all solid substances there is no very clear proof that the straight-line law—the law of proportionality, the law which is the basis of Mr. Russell's paper—holds for the particular voltages with which we are concerned in practice—that is to say, where the thicknesses of the dielectric are those employed on cables dealing with anything up to

Mr. O'Gorman.

15,000 volts. While admitting the absence of clear proof I believe that the proportionality can be accepted from the numerous indications we have, and from the general trend of experiment. We must simply recognise the difficulty of maintaining smooth surfaces, and the difficulties of calculation when surfaces other than plane or spherical are used as well as the difficulties in experimenting when the distances are small, and await confirmation of the hypothesis. Having accepted the straight-line law as the best approach to the facts, Mr. C. S. Whitehead and I took the trouble (I do not know whether many of the members have taken the trouble) to go through and re-work the whole of Mr. Russell's figures. Moreover, a new and independent proof was obtained by Mr. Whitehead. I hope, indeed I am sure, he will allow me to tender it to the Institution, so that it may be placed in its archives. Its complete independence of Mr. Russell's work is what makes it important, while the fact that it confirms his results may perhaps in later years, a long time hence, have a slight effect upon the mind of the cable makers. A great many very interesting points arise in connection with the subject. I may perhaps be pardoned for referring to a diagram which I made in 1901—just a simple statement of how it is that one considers a cable should be graded in proportion to its capacity for alternating currents and in proportion to its resistivity for direct currents. If the chairman, however, thinks there is not time to do it this evening I will merely draw attention to the fact.*

After disposing of the scientific difficulty about the proportionality of stress to potential gradient there is still another difficulty, which is, I think, the true reason for Mr. Nisbett's hesitation to adopt a graded dielectric. It is this, that the insulating substances found in commerce are frequently impure or irregular in composition, so that he and others doubt if it is worth while to try to reduce the radial thickness of a cable coating by 30 per cent. I can suppose him to say that if there is more than a 30 per cent. margin required to meet irregular fibre, dirt, drops of water, carbonised oil, and so on in the cable, and until we get rid of these, it seems scarcely worth while to play with another 30 per cent. which he would think more difficult to obtain. In justification of our own English cable makers, I think that is the reason for the postponement of any immediate attack. I do not agree with this view, because evidently *both* economies may be obtained, namely, one by grading the insulation, and another additional one by obtaining a purity of dielectric which allows of employing a diminished factor of safety.

If I am allowed to address myself to a point which is raised by the discussion rather than in the paper, the question of comparative cost, of graded and ungraded cables, I should like to say that when one of the speakers calculates as unimportant the value of the dielectric saved by grading, he is not dealing with the chief saving. The chief saving is in regard to the lead sheathing, the armouring, the iron pipes, the diminution of joints owing to the handling of greater lengths of the

* *Journal Institution of Electrical Engineers*, vol. 30, pp. 661–665.

Mr. O'Gorman

smaller size of cable—the supporting of less heavy materials in the air, etc. In fact, the indirect economy from reducing the size of the cable is very great, even if we ignore the saving in the amount of insulation upon it. One question was raised, to which the answer may not be known to all here, so that it may be worth while to put forward an attempt at an explanation. I refer to Mr. Nisbett's interesting remark, that if one breaks down a cable by a high pressure its insulation falls very greatly just before and at the time of the break-down; and further, that that broken-down cable will recover after a sufficient delay, and will then take the same time to break down again at the same volts. The explanation is something like this. Suppose a matrix of oil or air in an electric field, and suppose materials of higher capacity immersed in that matrix, these will range themselves in that part of the matrix where the intensity is greatest. That is why a pith ball in air moves towards a charged point, but in oil moves away from it. I do not propose to prove that now, but it is a fact. Water happens to be a material of enormously high specific inductive capacity, and it tends to put itself, if there is any water (and there is nearly always some in the cable dielectric), in the region of highest stress. Having got a little water there, a little streak, a little film, a little bead, it will not at once spread itself over the surface of the copper conductor, but it will stick out and so make another point of high intensity, and on to that bead another bead of water will in time attach itself, till the thickness of the dielectric is wholly or partially bridged. This appears to explain the whole observed phenomenon.

1. Before actual disruption and on disruption the insulation resistance will fall.

2. After a lapse of time this minute quantity of water will re-mix itself up with the dielectric, the insulation will rise, and on re-applying the pressure test for the necessary time to make the bridge breakdown occurs again.

Obviously this is only a theory, and as a theory it is only valid in so far as it fits the facts. It will also be found that if the original fault is localised, but the cable not cut, the second breakdown will occur at or very near the original place as would be expected from theory.

The same remarks would apply to any material of higher specific inductive capacity than the matrix, such as particles of metal of minute size, carbonised oil, or even a pure oil of bad insulating resistance.

Mr. Campbell.

Mr. A. Campbell: Of the two points to which I wish to refer very shortly, the first is a question of nomenclature. I intended to explain to the meeting, because I was afraid some might not understand it, that Mr. Russell's dielectric coefficient, which is quite a new term, means the same as specific inductive capacity, or, as it is sometimes called, dielectric constant. I would draw attention to the fact that both of those terms as well as Mr. Russell's seem objectionable, and ought to be changed. Specific inductive capacity is very clumsy; we

Mr. Campbell.

do not want three words. Dielectric constant is quite colourless and may mean several things, and so may dielectric coefficient. One might as well talk of the insulation coefficient as meaning the resistivity of an insulator. I would suggest that Mr. Heaviside's clear nomenclature should be adopted, which uses the word "permittivity" for this quantity. It is expressive and quite distinctive. I am glad to say that Mr. Heaviside's nomenclature is gradually being adopted; we talk of resistivity, resistance, inductance, conductance, impedance, and so on, and now "permittance" is coming in. I think it is time we adopted his terminology more completely, now that the great value of his work in connection with telephone cables is so universally recognised.

I pass from this to the practical question of testing the dielectric strength of isotropic solids. Mr. Russell suggests that the best way is to embed two spherical electrodes in the solid. This is comparatively easy if we have to deal with plastic or fusible solids like paraffin wax or bitumen, but it is not at all easy to apply to materials such as mica press-spahn. I would suggest that the difficulty might be overcome in the following way:—

Let the spherical electrodes be placed touching the sheet A B at

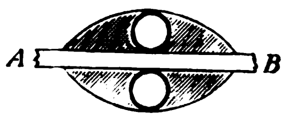


FIG. A.

opposite sides and let most of the space between be filled up by another auxiliary dielectric of permittivity similar to that of the sheet A B. If possible, the auxiliary material should be of somewhat higher dielectric strength than A B. In this way one can ensure that the distribution of electric stress between the spheres shall resemble what it would be if the spheres were embedded in A B, at least in so far as it depends on the permittivity. But, as Mr. Russell remarks, the distribution of the dielectric stress also depends on the resistivity of the materials. I should like to ask him, in conclusion, to what relative extent, in the case of a good insulating material, the distribution is modified by the resistivity of the material, and whether the permittivity effect or the resistivity effect plays the most important part with ordinary materials?

Mr. Paterson.

Mr. C. C. PATERSON: I do not propose to criticise any of the conclusions at which the author has arrived; as an old pupil of his I generally believe what he says. But I do want to ask him one or two questions, and I notice that other speakers have also put similar questions to him. I should like to know if he had sheets of insulating material to test, how would he do it? It is very desirable to get some satisfactory method of testing insulating sheets, a method which will determine the dielectric strength of the material on the same basis that

one is able to determine the dielectric strength, say, of oil when it is tested between spheres. Mr. Paterson.

I take it that Mr. Russell's objection to the use of flat electrodes with square edges is that the lines of equipotential, although parallel at the centre of the plates, tend to draw in at the corners, causing a steeper voltage gradient in the dielectric at these points. I have, as a matter of fact, noticed that with such electrodes breakdown seldom takes place at the centre of the field, but usually at the extreme edge. It is clear that flat electrodes with well-rounded edges must be better from this point of view than those with square corners. I should like to hear whether Mr. Russell thinks it possible to calculate the multiplying factor "*f*" for flat electrodes of given dimensions and hemispherically rounded edges of given radius. I suppose it would never be possible to have a multiplier of unity, since the air filling the space between the sheet and the rounded part of the electrode must finally reach a conducting condition at which it would act like a pad on the insulator, but it would be interesting to know the correction for a given case.

Would the author favour large spherical electrodes which would only touch the sheet at one place? If neither of these methods are satisfactory, it occurred to me that it might be possible when taking a sheet of insulating material, to cut out a disc of it, which had a smaller diameter than the electrodes between which it was being tested. Sparking across at the edges would be prevented by using a ring of high grade rubber tightly pressed on the specimen and projecting beyond the edge of the metal. Under these conditions it appears to me that an even potential gradient would be maintained across the material under test.

The CHAIRMAN (Mr. Charles P. Sparks): I should like to say a few words about Mr. Russell's paper. First of all, there is great practical difficulty in the grading of cables, owing to the fact that insulating materials have different chemical properties. The user of cables has to look at the matter, not from the point of view of a few years' life, but as a question of cables lasting for long periods, twenty-five or thirty years being a reasonably low limit of life. Hence I have no wish to use cables constructed with more than one kind of insulating material. I have some figures bearing on the past experience with rubber cables and paper cables, which appear to me to strengthen that part of Mr. Russell's paper which deals with the difficulties due to the use of cable cores of small diameter. The figures are as follows: Taking a small rubber concentric cable having a cross-section of 0·075 in., a diameter of inner core 0·36 in., and a dielectric wall of 0·09 in., the experience in eleven years' use of 17 miles of this size of cable at 2,200 volts pressure, 50 $\sim$, is that thirty-two faults have developed in the cable. This has been a serious difficulty, resulting in abandoning the use of that size of cable, and taking up of the bulk of the 17 miles laid. The next size of concentric rubber cable is 0·15 sq. in. cross-section, the diameter of inner core is 0·5 in., the dielectric wall is 0·11 in., and the length is 22 miles. In the last ten The Chairman.

The Chairman. years, working at 2,200 volts 50 $\sim$, there have been three cable faults. There is a difference of 20 per cent. in the thickness of the dielectric in favour of the latter size, and the working result shows less than one-tenth the number of faults. These two cables were made by the same firm; the insulation is of the same general character, rubber, but there is an extraordinary difference in the life. In another concentric rubber cable, 0·15 sq. in. cross-section, made by the same firm, diameter of inner core 0·5, dielectric wall 0·14, the experience with 49 miles of cable working for the first four years at 2,200 volts and for the last six years at 6,600 volts 50 $\sim$, is that no cable faults have developed. Turning to the question of paper cables, from an experience of three times the length of cable, namely, some 200 miles working at 2,200 and 6,600 volts, I have no data at all with regard to failures in the cable itself. Faults have occasionally developed at the joints, but with the factors of safety that have been adopted by engineers, assisted by the cable makers, paper cables have been put on the market with a factor of safety which has proved, after ten to eleven years' experience, to be sufficiently high to prevent the cables working at these pressures from developing any faults at all. I am not a cable maker, but I feel considerable sympathy with them to-night. I know from my dealings with them that they spend a great deal of time and a large amount of money in thrashing out the question of improving the manufacture of cables. I do not want you to go away to-night feeling that England is behindhand in the matter of cable-making. We have always been the leaders, and I believe we are the leaders in the cable business to-day.

Mr. Jona. Mr. E. JONA (Milan) (*communicated*): I find quoted on page 10 the opinion of the American Institute of Electrical Engineers (which seems to be shared by the author) that "a given setting of the spark gap is a measure of one definite voltage, and as its operation depends upon the maximum value of the voltage wave, it is independent of wave-form, and is a limit on the maximum stress to which the insulation is subjected."

On page 12 further reference is made to the same statement, where the amplitude factor is dealt with. Now I should like to ask if this opinion has been arrived at by conclusive experiments, and if so, by whom and when, or is it instead merely theoretical? Should such experiments be lacking, I should hesitate to accept this statement.

Mr. Russell propounds the theory that in a cable in which a is too small and less than b/ϵ, when subject to high tension, the dielectric begins to char near the copper, and that the discharge takes place only when the radius of the charred dielectric is less than b/ϵ. ("It is only when the radius of the charred dielectric gets greater than b/ϵ that a disruptive discharge ensues.") He refers here to one of my own experiments, and reckoning its results on the basis of this theory, finds that they agree fairly well with the theory itself. It is a skilful hypothesis to which I had myself occasion to allude in the past, but I do not know if it perfectly answers to the truth. In the hypothesis referred to, if we

test the cable for several hours to the highest possible tension without reaching the breakdown point, the dielectric near the conductor ought to be charred to a great extent. Then measuring the capacity of the cable before and after the test at high tension, one would expect to find that such capacity is increased after the test. Now this does not occur, or at least does not occur always. It is true that this experiment is not totally conclusive for various reasons, which would require too long a discussion. Nevertheless, it is an instructive experiment. In my paper at the St. Louis Congress I mentioned an experiment of this kind. Let us take a cable formed as in Fig. B : *c*, conductor of 4 mm. thickness; *j*, impregnated jute 2 mm. thick; *t*, very thin copper tape; *b*, layers of rubber 3 mm. thick; *p*, lead; and another cable (Fig. C.) exactly similar but for the absence of the tape *t* of copper. Let us subject cable No. 1 to an alternative tension of 8,000 volts. In calculating the initial distribution of the potential we find that the strain on the jute was 3,300 volts. It is too high a tension for such type of insulating material. The layer of jute *j* is very soon perforated Mr. Jona.

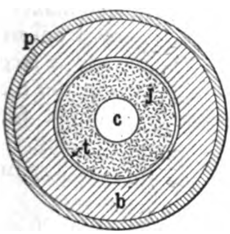


FIG. B.

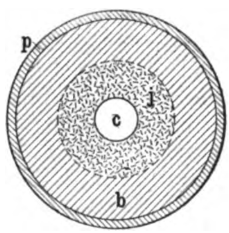


FIG. C.

by a disruptive discharge, and the total tension of 8,000 volts is brought to bear on the layer of rubber, between the copper tape *t* and the lead *p*. This layer bears such tension quite well, and the cable continues to work.

Let us repeat the experiment with cable No. 2. The distribution of the potential and the gradient are similar to those of cable No. 1. Let us put in circuit a reflecting electrodynamometer measuring the capacity current, and we shall find the following readings at the respective tensions :—

Volts ...	5,000	7,000	9,000	11,000	13,000	15,000
Deviations ...	33	62	105	150	220	297

The square roots of the deviations are respectively in the ratio of the tensions, from which can be deduced that the capacity of the cable is unchanged. And this happens after several hours' test at tensions much higher than the 8,000 volts that perforated the jute in cable No. 1. Should the jute char in cable No. 2 the capacity ought to have changed.

Mr. Jona. I have been engaged for over ten years upon this subject, and my present conclusion is that unfortunately the experiment is most difficult, and often gives results which are not in accord, owing perhaps to the non-homogeneity of the dielectrics, and that the theories based solely on the gradient of the potential are deficient. Such theories are partially true, but they do not represent the whole truth. I am therefore the more glad to see that such an eminent authority as Mr. Russell is interested in studying these subjects, and is applying to them his scientific and technical knowledge.

May I be allowed to correct a remark made by Mr. Russell on my work? On page 26 it is stated that "Jona's experiments indicate that the dielectric strengths of paper insulated cables do not vary much when the range of temperature does not exceed 60° C.; they are probably slightly less at the high temperatures." I found instead that they are perhaps slightly inferior at very low temperatures.

In conclusion, I must thank Mr. Russell for his allusion to my work and for the pleasure afforded by the present paper.

Mr. Beaver. Mr. C. J. BEAVER (*communicated*): I first noticed the phenomenon of partial breakdown of the dielectric of a paper insulated cable under test five or six years ago, but have only once or twice since seen positive instances of it in paper dielectrics. In a 6,000-volt rubber cable which broke down at 30,000 volts I have seen distinct evidence that the layers of insulation had not been sufficiently well graded, the layer of pure rubber next to the conductor having failed, and the remainder of the insulation left intact, the inside surface of the rubber showing slightly charred marks of a branched and zigzag character, similar in form to lightning discharges, and penetrating the pure rubber only. In each case which I have seen a disruptive discharge has occurred on the same length of cable as the partial breakdown, and in the case of the paper insulated cables I was inclined at the time to think that the partial breakdowns were due to impurities in the paper, as I found several carbon spots in the vicinity which were in turn traced to oily matter in the rope stuff from which the paper was made. The b/a ratio in one case (2·4) rendered this theory feasible, although in the other case where the ratio was approximately 3 it may have been only a case of partial breakdown relieving the maximum stress.

The experiments described by Jona in his paper to the St. Louis Congress and the author's deductions therefrom are, to my mind, much more convincing than observations of the charring effects, which are naturally made under complicated conditions, because positive evidence is usually destroyed when a dielectric burns out, even if only partially. It is, of course, highly probable that the partial charring of an inner portion of the dielectric is only a stage of the well-known overstraining effect of the application of too high a pressure, being preceded by considerable local ionisation, but it is very possible that many of the observed cases have not been due to the high b/a ratio.

It has been pointed out by Mr. G. L. Addenbrooke that when the breakdown point of a cable is approached the power factor increases

at a rapid rate. This may be due to the successive layers of the dielectric gradually reaching their maximum dielectric strength. Mr. Beaver.

As regards the author's remarks on the dielectric thicknesses adopted by the Engineering Standards Committee, it is a well-known fact that the thicknesses of paper or jute which have to be used on low-tension cables for mechanical reasons give absurdly low values of R_m, and on E.H.T. the factors of safety over R_m values—though considerably reduced below those which obtain on low-tension cables—are still sufficiently great for practical purposes, as demonstrated by the freedom from trouble in using small E.H.T. cables for the maximum pressures for which insulated cables are used at the present time. There are some well-known instances of E.H.T. paper insulated cables working in this country which have a b/a ratio of over 4, and have not suffered at R_m values exceeding those given in the author's table.

The author's remarks as to the possibility of using hollow conductors in E.H.T. cables of a small sectional area are by no means new to cable designers, but there are manufacturing difficulties in the way of making, say, multicore cables in this way, which, while not insuperable, entail a considerable departure from uniform manufacturing processes. This feature, as well as the correct adjustment of the b/a ratio, would certainly be relieved if economical conditions were such that aluminium conductors could be substituted for copper. This fact is also well appreciated by cable designers, and reference was made to it by O'Gorman in his paper on "Insulation on Cables."*

Mr. F. J. O. HOWE (*communicated*): The extreme in high-tension cable design is found in the case of the testing leads between high-voltage transformers and cable tanks at a cable works. Here the voltage is necessarily three or four times that of the latest commercial cable, while the current is extremely small. Mr. Howe.

As far as the conductor is concerned some small strand, say 7/18, would be chosen as suitable, but this has the disadvantage of requiring some 7 in. (radial) of rubber for a working pressure of 50,000 volts (R.M.S.), so a hollow conductor has to be resorted to. For such a cable one is tempted to adopt dielectric stresses of the same order as those used by Mr. Jona for his 100,000-volt cable, namely, approximately 9,000 volts per millimetre for rubber and 6,000 volts per millimetre for paper. However, such values are not allowable for any practical cable. Fixing on a reasonable safety factor (two to three) one has to design with 6,000 volts (R.M.S.) per millimetre for rubber.

The following are the dimensions of a high-tension cable designed and manufactured some time ago by Messrs. Johnson & Phillips, Ltd., and in use in their cable works for voltages up to 50,000 volts (R.M.S.) or for occasional work up to 70,000 volts:—

Fibrous core	0·55 in. diameter.
Conductor	0·65 ,, ,,

* *Journal Institution of Electrical Engineers*, vol. 30 p. 608, 1901.

Mr. Howe.

Tape	
Compound rubber	1·75 in. diameter.
Tape	
Lead	1·95 " "
Jute	
Galvanised steel wires	

This gives a radius over conductor $a = 0·325$ in.
" " radius over insulation $b = 0·875$ in.

$$\text{therefore } \frac{b}{\varepsilon} = 0·322 \text{ against } a = 0·325.$$

Any enlargement of the conductor (keeping the overall diameter constant) would mean an increase of the dielectric stress.

The author points out that for the most economical cable the value of a should be equal to that of d. In the above case we have—

$$a = \frac{0·325}{0·328} = 0·99\, d.$$

In connection with the author's theory on the breakdown of the innermost insulation layers in badly designed cables, the following details of a cable test I carried out some eighteen months ago in a continental cable factory are submitted. The cable was 3-core and paper insulated; each conductor consisted of $19 \times 1·55$ mm. ($= 0·05$ sq. in.) copper wires in sector form. The insulation thickness between copper and copper was 16 mm., between copper and lead was 16 mm., and the working pressure 20,000 volts. The length was 146 metres, and its insulation resistance (one core to other two and to lead) about 160 megohms per kilometre at 15° C. This cable was tested in the ordinary course with—

30,000 volts (3-phase, star point earthed) for 24 hours in water at 20° C.
50,000 volts (3-phase, star point earthed) for 15 minutes in water at 20° C.
50,000 volts (single phase, 3 cores against lead) for 15 minutes in water at 20° C.

The temperature of the tank water was then raised to 68° C. and the cable tested at 50,000 volts for 1 hour. The readings for this test are recorded below :—

The plant used was a 3-phase generator driven by a 110-H.P. 3-phase induction motor. The generator was connected through a 3-phase choking coil to the primary of a 200-k.v.a. 3-phase oil transformer 200/100,000 volts, whose secondary was connected to the cable, and the high-tension star point earthed. The choking coil was to protect the low-tension instruments (hot wire) should breakdown occur. All the gear had been in use the whole night, so had quite settled down.

Mr. Howe.

Time.	Machine.		Transformer (Y) 3 × 200 100,000 Volt.	
	Volt.	Ampere.	Primary Volt.	Primary Ampere.
6.00 a.m.	52	128	95—103— 98	100—120—120
6.27 „	76	154	—	—
6.32 „	81	165	96—103—101	140—160—140
6.36 „	89	173	95—101— 99	150—170—150
6.41 „	98	186	95—101— 99	160—170—160
6.44 „	101	190	95—100— 99	170—180—170
6.46 „	108	198	96—101—100	175—185—175
6.48 „	111	202	95—100— 99	180—190—180
6.49 „	114	206	95—100— 99	180—190—180
6.52 „	127	222	96—101—100	200—200—200
7.00 „	Switched out.			

The four voltmeters read correctly one with the other, likewise the four ammeters; also the three legs of the choking coil were calibrated to be exactly equal. It may be mentioned that the cable was a "rejected" length owing to its electrically dissimilar cores.

Unfortunately it was not convenient to measure what part of the current was energy current. This current increase may have been due—

(1) To excessive heating of the insulating paper; in this case, supposing that 110 amperes represents the charging current throughout the test, then we have an energy current of 167 amperes at 6.52 a.m., which corresponds to an insulation resistance per phase of 0·08 megohms, as against 1,100 megohms its insulation resistance at 15° C. Of course it is well known that insulating oils used for impregnating paper cables have excessively large temperature coefficients.

The impregnating oil used for this cable had the following specific resistance :—

at 47° C.	... ...	$13·4 \times 10^4$ megohms per cm.³.
„ 35° C.	... ...	44·8 „ „ „
„ 25° C.	... ...	141·8 „ „ „
„ 18·8° C.	... ...	316·6 „ „ „

(2) To alteration of the dielectric constant of the insulating materials and temporary breakdown of some of the innermost insulating layers, thus decreasing $\log \frac{D}{d}$ and consequently increasing the capacity and decreasing the insulation resistance.

Continuing the test, the temperature of the tank was again measured at 7 a.m. and found to be still at 68° C. This necessitated a

Mr. Howe. pause of a few minutes, but the results show how the cable had largely regained itself in this short time. The cable was now brought up to 90,000 volts, the readings being:—

Machine, 100 volts, 242 amperes.
Transformer, 172/193/179 volts, 164/180/170 amperes.

The phase voltage on the cable was therefore at least 86,000/96,500/89,500 volts, the ratio of 500/1 being slightly increased when working with this order of capacity.

This test was continued for two minutes.

The same cable, in other experiments, was subsequently tested with—

30,000	volts	(3-phase)	for	3	hours in water	at	40° C.
50,000	"	"	"	2	"	"	40 "
40,000	"	"	"	2	"	"	31 "
50,000	"	"	"	3	"	"	31 "
50,000	"	"	"	2	"	"	40 "
30,000	"	"	"	2	"	"	49 "
40,000	"	"	"	3	"	"	48 "
50,000	" broke down after 40 mins.					"	48 "

Upon the cable being stripped, the paper insulation was found to be very dry and brittle, but no charring was evident except at the fault.

Dr. Garrard Dr. C. C. GARRARD (*communicated*): A matter which I think has some connection with Mr. Russell's paper has come under my notice in connection with fuses intended to protect small instrument potential transformers on the high-tension side. For this purpose No. 48 S.W.G. copper wire has been used, but it has been found that this is entirely unsuitable. According to Sir W. Preece's formula, the fusing current of such a fuse is 0·65 ampere. The magnetising or load current is not anywhere as large as this. At the instant of switching on the magnetising current may be very much larger due to the residual magnetism of the core, and this would account for the fuses often blowing immediately they are placed in circuit. At the same time, however, these thin fuses seem to deteriorate, and I think this is due to discharges into the air which take place due to the small diameter of the wire. At 3,750 volts above earth, and with a distance between phases of 1 ft., the maximum potential gradient in the neighbourhood of the fuse wire is 293,000 volts per centimetre, which is much above the dielectric strength of air. Thus a discharge occurs which in time mechanically damages the thin wire and causes it to break circuit. By substituting a S.W.G. 40 wire for a S.W.G. 48 the maximum stress is reduced to 111,000 volts per centimetre, which, while still above the dielectric strength of air, will naturally not give rise to such a bad discharge. The No. 40 copper fuse wire has been found to be satisfactory.

The particulars given of Mr. Jona's 150-kilovolts cable (p. 23) are interesting, especially that he finds it advantageous to encase the inner

Dr. Garrard.

core with a lead tube in order to secure a smooth surface. The case of porcelain line insulators is analogous to this, in that these are generally fixed to pins which have a screw thread cut upon them. The electric stress in the porcelain in the neighbourhood of the sharp tops of the thread must be very great. It would be interesting to know whether, on any of the extremely high-voltage transmission lines, it has been found that sharp projections on the pins, such as screw threads, have been of appreciable effect in the durability of the insulators. I should imagine, however, that there would not be much effect. With a paper or rubber cable a gradual burning no doubt occurs, due to the concentration of the charging current along a conducting path in the interior of the insulation, the conducting path being due to partial breakdown of the dielectric. Such a burning effect could not happen with porcelain, and a very large liberation of heat would have to take place before fracture would occur simply for this reason.

Mr. Ashton.

Mr. A. W. ASHTON (*communicated*): In this paper the author has treated the subject mainly from the mathematical standpoint, and his conclusions with regard to gaseous dielectrics are supported by experimental evidence. With regard to solid dielectrics, however, and more especially with cables, the agreement between electrostatic theory and experimental results does not appear to be very close. From breakdown tests on single dielectric rubber cables, I have come to the conclusion that the potential gradient, as calculated from the formula $\dfrac{V}{a \log \frac{b}{a}}$, increases as $\frac{b}{a}$ increases instead of being constant. On the other hand, the ratio $\frac{V}{t}$ (t = thickness of dielectric) decreases as t increases, but not to the extent shown by theory. Actual results on pure vulcanised rubber show $R_{max.}$ 400,000 volts per centimetre for $\frac{b}{a} = 1{\cdot}4$, and 550,000 for $\frac{b}{a} = 2{\cdot}7$. Similarly for rubber cables having pure, separator, and jacket, the potential gradient for disruption apparently increases as $\frac{b}{a}$ increases. These results were obtained by breakdown tests in which the pressure was raised as quickly as possible until the cable broke down.

The results obtained by Jona on the two paper cables of equal thickness but different diameters are in agreement with those given above. Thus $R_{max.}$ is 78,000 volts per centimetre for $\frac{b}{a} = 2$, and 238,000 volts per centimetre for $\frac{b}{a} = 28$. The author considers this to indicate the breaking down of the inner layers at about one-third of the ultimate breakdown pressure in a manner similar to that in which the "electrostatic corona" is formed in gases. This effect can only

Mr. Ashton. take place in cables which have $\frac{b}{a} > 2{\cdot}7$, and as a similar variation in $R_{max.}$ is obtained with cables for which $\frac{b}{a}$ is $< 2{\cdot}7$, it is evident that this theory rests on a very slender foundation as far as this experiment is concerned.

Coming now to the result obtained by Jona on the graded cable, it is doubtful if this is any better than what has been obtained in the case of the Helsby patent cable without any attempt at grading. In this type of cable, the inner and outer layers consist of a material which is practically unattacked by the gases produced in a brush discharge. The three internal layers are vulcanised rubber, and the dielectric constant increases as we travel outward from the copper, the dielectric strength being practically the same for all three layers. This cable is different in size to the Jona "graded" cable, and a strict comparison between them is difficult.

Assuming the law $R_{max.} = \dfrac{V}{a \log \frac{b}{a}}$ is true within the narrow limits of these two cables, the quantity $d = a \log \frac{b}{a}$ is a measure of the strength of the cable, provided the dielectrics are equally strong. The above quantity d is dependent only on the section of the dielectric, and since it may be defined as $\dfrac{V}{R_{max.}}$, we might call it the "modulus of section" of the cable.

Taking $R_{max.}$ in volts per centimetre on the peak of the curve, and V in R.M.S. volts, the "modulus" for the Helsby cable is 0·363, and that for the Jona cable is 0·608. The section of the latter is therefore 1·67 times as strong as the Helsby cable I am referring to. Now the Jona cable stood 75,000 volts continuously, and 100,000 for 4 hours*; dividing by 1·67 we get 45,000 volts continuously, and 60,000 for 4 hours for a Jona cable of same section as the Helsby one. This latter has stood 60,000 volts for 8 hours without breaking down, and 120,000 for one minute, a result even better than the Jona cable gives, and one which is obtained not by simply ignoring, but actually defying, the electrostatic theory.

Mr. Morris. Mr. J. T. Morris (*communicated*): Instead of grading the dielectric, another possible method of obtaining an approximately even distribution of the potential gradient in the insulation of a cable is one which was suggested by me some two years ago at a course of lectures on E.H.T. cables. It can be most easily applied in the case of alternate current transmissions. To the best of my belief it is novel.

The plan is to insert one or two wrappings of conducting layers (such as thin copper tape or thin lead sheathing) so that the dielectric of the cable is divided into two or three equal parts. These conducting sheaths are then connected to suitable tapping-points on the trans-

* *Transactions o the International Electrical Congress*, St. Louis, vol. 2, p. 550.

former supplying the cable, thus maintaining the sheaths at the desired intermediate potentials. Mr. Morris.

Mr. J. F. WATSON (*communicated*): I think some of the speakers were under the impression that Mr. Russell was criticising the design of standard high-pressure cables, whereas I take it that he suggests that the potential gradient on the dielectrics must be taken into account when designing cables for 100 k.v. and upwards, which are not pressures commercially used at present. Mr. Watson

The alteration of our standard walls of dielectric to bring them into line with Mr. Russell's calculated values might not be altogether desirable, as our figures have been deduced from practice and experiment, and I think if the two values were compared for cables of all commercial sizes up to 11,000 volts working pressure they would not differ in a sufficiently marked degree to justify the cost and trouble of cylindrical conductors, but when the working pressure exceeds 11,000 volts it is possible that a saving might be effected by the use of his values.

Mr. Nisbett stated that insulation resistance fell on a cable that had been subjected to high-pressure alternating voltage, and also held that the insulation resistance of the cable recovered, after a rest of twenty-four hours, to its original value, and that the dielectric strength had not been reduced.

In this I think he is wrong, as it is in direct contradiction to observations I have made, and I am of opinion that long lengths of cables (by this I mean to omit sample test lengths) of 10,000 volts working pressure, tested at 20,000 volts for a period of one hour, are strained to an unnecessary degree.

The reason for this falling off in the insulation resistance in oil-paper cables is probably due to the disintegration of the oil between the layers of paper in the wall of the dielectric by an excessive potential gradient in these layers. The reason that the insulation resistance gradually rises when the testing pressure is taken off is due to the mixing of the affected oil with the unaffected oil, but I think that if very accurate tests were made on long lengths it would be found that the insulation resistance never completely recovered.

I have experimented to determine whether the size of the conductor determines the breakdown pressure in a concentric cable. I find that with paper put on in layers satisfactory results cannot be obtained except by making a large number of tests (widely varying). This is probably due to the difficulty in getting the same number of layers of paper on to two cylinders of largely differing diameters so that the thickness of dielectric wall is equal.

In the following experiments I have endeavoured to eliminate errors by using "Empire" cloth or varnished cambric, a material that is very uniform in texture and possesses good pressure-resisting qualities. I took a roll of this material and cut it in halves, and from the two outer edges I cut the selvage. Each of these strips was then 17 in. wide. My inner conductors were brass tubes, the small conductor measuring

Mr. Watson. 9·9 mm. and the large conductor measuring 22·3 mm. I wound on thirteen complete turns of this cloth, full width, to each conductor as tightly as possible.

The diameter over the insulation of the small conductor, was 16 mm.

The diameter over the insulation of the large conductor, was 28·4 mm.

The thickness of wall on the small conductor was $\frac{6 \cdot 1}{2}$, 3·05 mm.

The thickness of wall on the large conductor was $\frac{6 \cdot 1}{2}$, 3·05 mm.

The ends of the insulation were then sealed into porcelain insulators to prevent sparking over.

The alternating pressure was then applied between the insulated smaller conductor and tinfoil wound round and bound down on the insulation. When this reached a value of 40,000 volts a puncture was effected. The pressure was then applied to the sample with the larger inner conductor as on the above, and this punctured at 49,000 volts.

As the walls of insulation of each of these conductors were equal within 1 per cent., it seems to point to the fact that the potential gradient on the smaller conductor was steeper than was the case in the larger one.

Mr. Rayner. Mr. E. H. RAYNER (*communicated*): The co-ordination of results on sparking distances in air seems to be most valuable, and it will be extremely interesting to see how future experiments will fall in with the author's figures now that the proper conditions have been laid down. It will also be interesting to see whether the workshop method of measuring maximum voltages by discharge between needle-points—a method somewhat discredited, but largely used in America—may not be modified to give more consistent results.

This was brought to my notice by an engineer to one of the 60,000-volt transmission plants in Mexico. He was troubled to know which of the two figures was more likely to be correct.

Referring to his transformer, with which he tested his line insulators :—

Ratio of Transformer.	Am. I.E.E. Rules Sparking Distance.
97,300	127,000
108,000	147,000
125,000	182,000
132,000	200,000

I suggested that it might be due to earthing one pole rather than the middle of the transformer, and referred him to Mr. Russell as being the most likely person to assist him.

From the experiments done on the point of appearance of coronal discharge, it seems to be produced at a definite voltage for any given form and size of electrode. It would be interesting to know whether

its appearance in the dark between two parallel wires might not be a useful indication of maximum voltage, wires of different diameters and distances apart being used to suit different pressures. Mr. Rayner

In connection with the potential gradient in concentric cables I should like to bring before the Institution an article by Rudolph Nagel.* The paper deals especially with very short cables. The author was led to his design by the necessity of insulating efficiently the high-voltage terminals of a transformer of 400,000 volts, where they passed through the transformer tank. He considers first the insulation required for a conductor of 10 mm. radius, such that the potential gradient does not exceed 5,000 volts per millimetre, a figure comparable with that of the author's given on page 24 last column. The calculation leads to the result that a thickness of 54 cms. is necessary for a cable about 3 ft. 6 in. in diameter to carry about $\frac{1}{5}$ of an ampere.

The reason is obvious; the inner conductor is so small compared with the thickness of the insulation that the maximum stress may be greatly reduced by increasing the diameter of the inner conductor.

In the above case—

Maximum stress ...	... 5,000	volts per millimetre.
Minimum stress ...	... 91	" "
Mean stress ...	... 370	" "

The most economical quantity of insulation, he states, is when the radius of the inner conductor is 51 mm., and the thickness of insulation is then 63 mm. for a maximum potential gradient of 5,000 volts per millimetre as before [making $\frac{b}{a} = 2{\cdot}24$, see page 25]. This leads to a cable about 9 in. in diameter.

On these lines, the author states, have such questions so far always been treated.

What I especially want to bring before members is an exceedingly elegant method by which the author has further equalised the potential gradient in a concentric cable so as to render the potential gradient almost the same throughout. Unfortunately it is only applicable to very short cables, or it would appear to solve the question of the disposition of insulation for high voltages.

If the insulation of a concentric conductor have a series of concentric thin metallic tubes embedded in it the cable remains practically the same electrically speaking, the tubes acting as concentric condensers, and they do not affect the potential gradient. This is what the author does, but in the case of a short cable it is possible to remove the outer layers of the insulation at the ends of the cable and also the ends of the condenser tubes, and leave the ends conical in form, arranging the capacities of the condensers so as to render the potential gradient approximately uniform. In this way the potential gradient in the insulation may be regulated and kept as nearly constant as may be

* *Elektrische Bahnen und Betriebe*, vol. 4, p. 275, 1906; *Eclairage Electrique*, vol. 47 p. 465, 1906.

Mr. Rayner. required by increasing the number of concentric condensers and adjusting their length correctly. The author in his sketch shows seven such tubes which he suggests may be of tinfoil.

The method also greatly decreases the trouble due to disruptive discharge from the inner conductor along the surface of the insulation. The potential gradient in the immediate vicinity of the free end of the conductor is by his method considerably diminished.

Professor Schwartz. Professor A. SCHWARTZ (*communicated*): The author has referred to the damage to the insulating material of cables which may follow from the application of too high a test pressure, and I should like to call attention to the fact that considerable injury may also accrue to the dielectric of paper insulated cables by bending the cable either in coiling round drums, in laying, or in testing for flexibility. In addition to the electrical properties of insulating material used for cable work, it is essential that such material should be flexible, and this being so it is desirable that the finished cable should be tested for this property.

The following is the specification of a test which is frequently applied:—

"The dielectric must be bent six times (three times in one direction and three times in the opposite direction) round a smooth cylindrical surface not more than twelve times the diameter of the finished cable."

Assuming that the intention is that the six bends should be made on one and the same portion of the cable, the results given below seem to show that the test is too severe for paper cables.

The tests were carried out in the electrical laboratories of the School of Technology, Manchester, by Messrs. Grant and Lustgarten and myself on lengths of lead-covered paper and bitumen-covered paper cables kindly loaned for the purpose by Mr. S. L. Pearce, the chief electrical engineer of the Manchester Corporation.

In all some eighteen cables, varying from 1 sq. in. to ¼ sq. in. in section were subjected to the test specified above, and in each case the paper strips were badly damaged.

The paper strip is lapped on spirally, and the outer layers on bending are placed in tension, which in many cases has produced rupture of the strip in a direction parallel to its length, while the inner layers are crushed, but as the direction of bending is reversed half-way through the test both these effects are superposed.

The paper is stressed in tension across its width, and rupture usually takes place close to the line of overlap of contiguous turns, doubtless owing to the adhesion between the layers.

The following short report is typical:—

3-core cable for 6,500 volts, 37/15.

Paper insulated, lead covered and armoured—

Diameter of finished cable = 2·64 in.
Diameter inside lead = 2·11 in.
Insulation 40 layers paper, 76 × 5·2 mils.

Interstices between the insulated cores packed with jute. Insulation of cores— Professor Schwartz.

Diameter outside insulation	0·88 in.
Diameter copper	0·535 in.
Dielectric thickness	0·174 in.

The cores were insulated with paper impregnated with resin oil, sixteen layers being wrapped in a clockwise direction and thirteen layers in the opposite way. Paper strip, 132 × 5·2 mils.

Result of Bending Test.—The two or three outside layers nearest the lead were not damaged to the same extent as the remainder. In nearly all the other layers the paper had been ruptured in many places.

The paper round the individual cores did not suffer to the same extent, as it had not been bent round so small a diameter relatively to the outer insulation, but it was frequently cracked through, and in some layers completely ruptured.

With the paper-insulated bitumen-covered cables the paper was found to suffer in the same way as with the lead covering, but the bitumen was apparently uninjured. Subjecting the lead-covered and bitumen-covered paper cables to a breakdown voltage test before and after the bending test, it was found that in each case the dielectric strength of the lead-covered paper cable was reduced from 20 to 50 per cent. by the bending test, whereas in four out of five cases with the bitumen-covered paper the dielectric strength was actually increased from 12 to 25 per cent. I do not care to base any deductions on so small a number of experiments, but should be interested to know if the author could suggest any explanation.

The author has shown that within limits the failure of a portion of the dielectric may reduce the dielectric stress per centimetre, but such partial failure would reduce the voltage required to pierce the total thickness of insulation.

Provided the damage due to bending was not too great, and the test voltage was below that required to break down the insulation, the author's contention would account for cables under these conditions passing the voltage test after bending, although their ultimate resistance to puncture had been reduced by injury occasioned in the bending.

With regard to bitumen, although it withstood the bending test, it was found that it was easily cracked by a smart blow, and the experiments would seem to show that considerable injury may be done to paper insulation, whether lead or bitumen covered, by careless handling.

Mr. A. RUSSELL (*in reply*): Several of the speakers in the discussion have questioned the validity of the assumption underlying the grading of cables, namely, that the stress on the material is proportional to the potential gradient. I cannot imagine any reason why the stress should not be proportional to the potential gradient. I also believe that when the electric stress attains a definite value, depending on the Mr. Russell.

Mr. Russell. physical condition of the insulating material, it will break it down. To me any other state of affairs is almost inconceivable. I consider that the electric (dielectric) strength of a medium, in a given physical condition, is a real constant, and that none of those experiments from which the extraordinary conclusion is deduced, that the electric strength of materials in thick slabs is less than in thin slabs, will stand examination. I agree with Mr. Jona that in certain cases it may be exceedingly difficult or even impossible to calculate this electric stress, as other causes, the magnitudes of which we can only guess at, intervene to modify it, but the breakdown at a point occurs when the electric stress at the point attains a perfectly definite value.

The interesting question naturally arises, what is the appearance of the broken-down dielectric in single-core mains which have been subjected to pressures high enough to break down some of the dielectric, but not high enough to cause a disruptive discharge? Mr. Jona has noticed excessive heating, Mr. Beaver that the rubber showed slightly charred marks of a branched and zigzag character, Mr. Watson that the material was charred, Mr. Nisbett has not noticed charring except when air spaces were present, and Mr. Howe, in the test of his three-core cable, says, that the paper insulation was very dry and brittle, but that there was no charring except at the fault. From this it is evident that charring occurs in some cases, but whether charring occurs or not I think that once the dielectric has broken down it will never prove of much future use, mechanically or electrically, as a covering.

Whether grading would be useful or not depends on many circumstances which have always to be considered. If any manufacturer has an insulating material of great mechanical and electrical strength, a good conductor of heat, having negligible absorption, etc., then he would be foolish to hanker after grading until the pressures get so high that he has to use comparatively thick insulating walls. It is then possible to effect economies by using several insulating wrappings and arranging them in the best order. In order to do this it is absolutely necessary to know the electric strengths of insulating materials. My experimental results have led me to conclude that at high pressures the disruptive voltages for some homogeneous dielectrics can be predicted with a mean inaccuracy of about 10 per cent.

I am not convinced by Mr. Beaver's and Mr. Watson's apologetics for the British Standard Radial Thicknesses. They doubtless do very well in practice, but it is an odd thing to standardise rule of thumb thicknesses. Economies can be effected by using some of the new insulating materials which have excellent mechanical, thermal, and electric properties. Why prevent inventors from reaping the harvest to which they are entitled?

Professor Fleming raises the question of the ageing of dielectrics. Any data on this point would be of great value at the present time. I have noticed that the ten Stanley 3-microfarad paper condensers which I have used continually during the last six years in high

frequency and resonance experiments often get exceedingly hot. But notwithstanding the severe electrical and mechanical stresses to which they have been subjected, none of them seem any the worse. Dr. Fleming has utilised most successfully the low electric strength of rarefied neon in his cymometer. It would be of interest to know the electric strength of this gas at ordinary pressures. The use of compressed air or nitrogen or carbonic dioxide in experiments on electric strengths or for insulating conductors is well worth considering. Mr. Russell

I am grateful to Mr. Nisbett for his careful study of my paper, and highly appreciate his criticisms. Being unlearned in the mysteries of the cable-making craft, I know little about costs and the practical difficulties in the way of manufacture. My objects were to point out the way of calculating the "factor of safety" of a cable and to indicate the magnitude of the saving that could be effected by properly arranging the insulating wrappings. I understand that Mr. Nisbett's experimental results contradict the foundation of the theory outlined in this paper, and, that even if they did not, yet since actual cables contain air spaces my assumptions are not permissible. I have dissected samples of a good many cables, and have assisted at post mortems on a few, but in single-core cables at least I have never noticed any appreciable air spaces. Some people might infer from Patent No. 11149, 1906, granted to the British Insulated Wire Co., that methods have been invented which eliminate air spaces. In my opinion the possibility of air spaces emphasises the importance of designing the cable so as to make the electric stresses as uniform as possible. No one appreciates more highly than I do the way in which B.I.W. cables illustrate electrostatic principles (see Russell, "Alternating Currents," vol. i., p. 110 *et seq.*). The experimental results quoted by Mr. Nisbett, therefore, greatly surprised me. They directly contradicted the conclusions at which I had arrived after several years' experience in measuring electric strengths with high voltages. Consequently I gladly accepted an invitation from Mr. Watson to witness in one of the test-rooms of Callender's Cable Company some experiments which had a bearing on this point. Mr. Watson used large sheets of empire cloth tightly wrapped round brass cylinders of various diameters, the thickness of the covering being the same in all cases. The outside conductor was a small cylindrical sheet of metal foil, and the rods were suitably suspended in air by high-tension insulators. According to my ideas the maximum stress occurs near the edges of the outer conductor. The voltage was applied by a 75-k.w. 100-k.v. testing transformer actuated from the public supply mains. The supply company being the same as that which supplies my own laboratory, the shape of the applied voltage wave was well known to me. At pressures greater than 40 k.v. the brush discharges from the metal foil were continuous, but as the pressure was brought up to the puncturing point in about a minute the heating of the dielectric was very slight. It was barely warm to touch immediately after the breakdown. The results obtained were conclusive and quite in accordance with my ideas of electrostatic theory

Mr. Russell. which, although old-fashioned, were not finally adopted for practical testing until after many experiments. I was interested to learn from Mr. Nisbett about the decentralising of the core of concentric mains. This is the explanation of the magnetic effects sometimes produced by the currents in these cables on telephone wires. The magnetic force in the external field produced by the current in the cylindrical tube is the same as if the current were concentrated along the axis of the tube. If this axis does not coincide with the axis of the core, a magnetic field will be produced external to the concentric main, similar to that produced by two parallel current filaments. Mr. O'Gorman's explanation of how it might be possible for a broken-down dielectric to recover is interesting and convincing. He very properly lays stress on the economies effected by grading in the lead sheath, armouring, etc. It seems to me, however, that there may be difficulties in jointing graded cables.

In reply to Mr. Campbell, the phrase "dielectric coefficient" is a contraction for Maxwell's "coefficient of specific inductive capacity of the dielectric medium." I think it a good phrase—Principal Carey Foster and others use it. It is not the same as permittivity. It is a pure numeric, and is the ratio of the permittivity of a body to that of the standard æther. Dr. Heaviside's permittivity (see "Electromagnetic Theory") is measured in terms of the permittance of unit volume, and so it is $\lambda/4\pi$. I think that all teachers ought to explain Heaviside's rational system of units to their students in addition to the ordinary system. It is most instructive. As to whether the permittivity or resistivity effect is the more important in determining the potential gradient, this depends on the relative values of the dielectric and insulation coefficients, and on whether direct, alternating, or impulsive pressures are applied. The example worked out in § 16 was meant to illustrate this point. I think that Mr. Paterson's idea of a guard ring of rubber of the same dielectric coefficient as the circular lamina of the material under test would prove useful in practice. The interesting and important data given by Mr. Sparks prove that even at pressures less than 10 k.v. the potential gradient theory can be trusted to give an indication of the life of a cable.

I attach great weight to Mr. Jona's criticism. At high voltages he considers that the maximum potential gradient cannot be trusted to give the breaking-down voltage of a homogeneous dielectric, even when the test is made under practically ideal conditions. There is some other physical law apparently that has to be taken into account. It is highly probable that in some cases the physical conditions make it exceedingly difficult to calculate the maximum electric stress, but I consider that it is the value of this quantity that ultimately determines the breakdown at a point. The ordinary theory gives its value with sufficient exactness in many cases. As I said at the meeting, Lord Kelvin, in 1860, was led to infer from his experiments that at high voltages the value of the potential gradient in air at the instant of the disruptive discharge would be found to be sensibly constant, and four years ago

Mr. Russell.

(by analysing other people's experimental results) I satisfied myself that this was the case. I attempted also to account for the anomalous results obtained at low voltages by supposing that some of the pressure was lost at the "anchors" of the Faraday tubes. At high pressures it is unnecessary to make any assumption of this nature, as the numbers found by the ordinary formulæ are in close agreement. Unfortunately very few of the published results of tests of the electric strengths of solid dielectrics are of any use from my point of view. I may say, however, that my experience of measuring the electric strengths of solid dielectrics at high voltages (10 k.v. and upwards) has been that the nearer I could get to the ideal conditions indicated by electrostatic theory the more concordant were the numbers obtained.

The sparking voltages between needle-points given by the American Institute are due to Mr. Steinmetz.* They have been verified experimentally by W. Voege † and others. I have not yet found any method of predicting them by calculation from the known dielectric strength of air.

I am interested to hear from Mr. Beaver that there are E.H.T. paper-insulated cables working in this country which have a b/a ratio greater than 4. It looks as if high insulation resistance and low capacity were the main objects the designers had in view.

Mr. Howe's data illustrate that theory can be usefully applied in practice. Dr. Garrard's remarks on the deterioration of thin fuses on high-tension circuits, and his query as to whether sharp projections on the pins have any appreciable effect on the durability of insulators, are of great interest. Mr. Ashton raises the question as to the applicability of simple theory to solid dielectrics. I have referred to this above. It may be of use to call $V/R_{max.}$ the "modulus of section" of high-tension cables.

Mr. Morris's method of grading the dielectric is sound theoretically and may be convenient in a limited number of cases. Owing to the appreciable capacity currents which the conducting layers would have to carry in order to equalise the potential gradients in the dielectric it would be better to make them of thin copper. I agree with Mr. Watson on the inadvisability of testing cables at excessive pressures. As he has tested cables with both direct and alternating pressures greater than 100 k.v., his experience is probably unrivalled, at least in this country. Mr. Rayner's explanation of the discrepancies between the observed sparking voltages between needle-points and those given by the American Institute is probably the correct one. I think highly of his suggestion that the appearance of the coronal discharge between two parallel wires might be taken as an indication of the pressure between them. Research in this direction seems promising. I have to thank him for his reference to Mr. Nagel's paper. The method is very similar to Mr. Morris's. Professor Schwartz calls attention to the damage done to the insulating material of cables

* *Transactions of the American Institute of Electrical Engineers*, vol. 15, p. 281, 1898.

† *Annalen der Physik.*, vol. 14, p. 556, 1904.

Mr. Russell. by coiling them round drums. The fact that the electric strength of the bitumen-covered paper was increased by the bending is very puzzling, and I can offer no explanation.

In conclusion, I want to call the attention of electrical engineers to the fact that the power factor of cables, even when only a small length is available, can now be measured accurately. If we employ Wien's method and a vibration galvanometer, the maximum inaccuracy of the measurement need not be greater than 1 per cent. (see Grover, Bureau of Standards, Bull. 3, pp. 371–431). When using Duddell currents of high frequency I have frequently been struck with the excessive rise of temperature in certain types of paper condensers, and I have no doubt that cables will be found to differ very considerably amongst themselves in this respect. The losses, which I believe are due to absorption, increase very rapidly as the voltage is increased.

The Chairman. The CHAIRMAN: I will now ask you to accord a hearty vote of thanks to Mr. Russell for his interesting paper.

The resolution was carried by acclamation.

The meeting adjourned at 9.30 p.m.

Proceedings of the Four Hundred and Sixty-fourth Ordinary General Meeting of the Institution of Electrical Engineers, held in the room of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, December 5, 1907—Mr. F. GILL (Vice-President), in the chair.

The minutes of the Ordinary General Meeting held on Thursday, November 28, 1907, were taken as read and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council:—

TRANSFERS.

From the class of Associates to that of Associate Members :—

Evan Evans. | Frederick T. Hall.

From the class of Students to that of Associate Members :—

Percival H. Powell.

Donations to the *Library* were announced as having been received since the last meeting from The Associazione Elettrotecnica Italiana, H. Borns, Messrs. A. Constable & Co., W. D. Hunter, Sir Oliver Lodge, and Messrs. McMillan & Co., to whom the thanks of the meeting were duly accorded.

Messrs. H. Carpmael and L. Dixon were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

Wilfred Bowman Brady. | John Charles Eadie.
Ernest Edward Brooks. | John Henry Havelock.
George Edgar Seymour.

The following paper was read and discussed :—

AUTOMATIC CAB-SIGNALLING ON LOCOMOTIVES.

By J. PIGG, Member.

(*Paper received from the* NEWCASTLE LOCAL SECTION, *October* 17, 1907, *read in London on Dec. 5th, and at Newcastle on Dec.* 9, 1907.)

Introduction.—Of late years the subject of the signalling of railways—that part by which instructions are given to the drivers of trains—has received a good deal of attention, and the difficulties under which drivers labour under exceptional conditions of weather have been officially recognised by references and recommendations from the inspecting officers of the Board of Trade after inquiry into the causes of certain accidents. Numerous systems have been devised and put forward during the last ten or twelve years for supplementing the ordinary outdoor mechanical signals but, practically, no progress has been made in their application. Many of the suggestions have obviously, from internal evidence, been made by persons having little or no knowledge of the conditions to be satisfied in railway working, and are impracticable by reason of the arrangements proposed; others are impossible because of the expenditure involved, and some, again, could not be adopted because they involve departures from the regulations under which traffic has to be worked. In some cases the apparatus has been purely mechanical, in others purely electrical, and in others, again, a combination of the two has been made use of. In certain cases the indications are to be given directly upon the engine, in others they are to be produced on the line, but to be of such a character that they could not fail to be noted by the driver in the opinion of their designers. In some cases visual indicators are provided, in others audible signals are given, and also a combination of the two is suggested. In certain proposals indications are to be obtained by direct impact between apparatus carried upon the engine and other apparatus fixed on the line; in other proposals impact is lessened by the use of brushes sliding over prepared surfaces, and some contactless systems, depending for their action upon magnetic influence, have been proposed. Whatever the merits of the various proposals, not one has made any headway towards adoption, and railways still continue to use the system of visual signalling—during clear weather—supplemented by the audible, explosive signal during fogs, etc., with which all are familiar, notwithstanding its admitted deficiencies, and the lessons taught from time to time by accidents of a grave character. This delay cannot, however, be laid to the charge

of the railway companies who have on many occasions furnished opportunities for the trial of apparatus, but whose officers have, necessarily, to gauge carefully the merits of proposals from every standpoint.

Of other causes which have led to the admitted want of progress in this class of signalling it is not intended to speak here, although in most cases they are evident. The problem to be faced is not a simple one. There are many factors to be taken into account which render the designing of a system a matter of some difficulty. The engineering difficulties are evident when consideration is given to the speeds at which trains travel at the present day. At high speed the time allowable for the collection of a signal, by any reasonable design of apparatus, is very short, and the apparatus must be exceedingly prompt in operation. Where mechanical impact is relied upon for the operations, the stresses to which the parts brought into contact are subjected are tremendous, and in such forms as have been used, sooner or later, according to the frequency of use imposed by the design of the signalling apparatus as a whole, flaws develop in the stoutest material, and consequent failure. In all forms of actuation involving mechanical contact between parts on the engine and parts on the line, the greatest possible care must be taken to minimise the blow experienced.

The financial difficulties connected with such proposals are no less evident than the engineering difficulties. The requirements in connection with signalling have extended so marvellously, of late years, that its maintenance is likely to become a severe tax on railway companies. Any system for supplementing the present signalling which does not take into account the cost factor will fail in a most important point. If the proposals hold out any prospect of relief in this respect so much the better. It is comparatively easy to devise means to almost any end if cost is immaterial. The engineer's object, however, should be the keeping of the cost within the value of the service given. Certain things must be provided for railway operations—as in other industrial concerns—regardless of cost, and signalling is one of those, but the expenses incurred fall in the end upon the customer. Railways, however, are in a position in which it would be difficult to maintain increased charges to cover increased expenses of operation.

If we carefully consider the operations involved in that mass of conventions known as the "block system" (in which term is included all apparatus used for signalling), we find that the sole aim of that complex system of signalling is the exhibition of appropriate signals to the drivers of trains for their guidance, and that it implies implicit observance on their part of the signals exhibited. We further find that whilst the signalmen, by whom the driver is guided, are provided with reminders to a certain extent, and hedged about more or less completely with restrictions on their actions, the driver must rely upon his physical powers—sometimes under most difficult conditions—to enable him to observe and obey the signals by which

he is to be guided. We find also that, whenever accidents have taken place due to errors of signalling, they have had their origin in variations of one main cause—the human element of control and operation which is the basis of the system.

There are three men concerned in the passage of a train past any signalling point; the signalman at that point who controls it by the mechanical signals, the signalman at the next signalling point ahead, from whom the former takes instructions before allowing the train to pass forward, and the driver who must obey the signals exhibited.

A study of the causes of railway accidents will show that each of these men has failed in his duty, and collisions have been caused by:—

(*a*) The signalman at the receiving end of a section giving permission for a train to enter the section before it is clear of the preceding train.

(*b*) The signalman at the sending end of a section allowing a train to enter the section before he has obtained permission to do so.

(*c*) The driver of a train ignoring the signals exhibited for his guidance at that point.

If we carefully consider these causes, it will be seen that they show the driver to be by far the most important of the three men engaged. Errors on the part of the signalmen *may* be neutralised and disastrous consequences obviated by the vigilance and promptitude of the driver; but if the latter acts under misapprehension, and ignores the guidance provided for him, there is no power capable of averting the consequences that may follow from the conditions obtaining at the point at which the error is committed, or subsequently, before he again comes under control. The driver, then, is the pivot of railway signalling, and it is of the utmost importance that every means should be taken to ensure his duly receiving the guiding instructions provided for him. During the last two years the author has been associated, in a minor part, in experiments having in view the best means of supplementing the ordinary signals given to drivers. The apparatus has been designed by Mr. Vincent Raven, the Chief Assistant Mechanical Engineer of the North Eastern Railway, by whose permission the author is enabled to give the details which follow, and to show one of the instruments fitted to a model representing the locomotive and a section of line as used in practice. The system is essentially an electrical one, which aims at reproducing, in the cab of the locomotive, all the information that the driver now obtains from the present mechanical signals, but is not intended as a substitute for those signals. Before proceeding to describe the apparatus, however, it may be advisable to look a little more closely than is usually done into the characteristics of the outdoor mechanical signals, in order to ascertain what is required from an efficient form of cab-signalling, and to

compare the results obtained with the apparatus put forward for the purpose.

Characteristics of Semaphore Signals.—Consider the signals provided for main-line work, with which alone this paper deals. The principal signals are the "distant," the "home," and the "advance," the relative positions of which are shown by Figs. 1–5. The "distant," as its name implies, is fixed at a considerable distance from the box from which it is operated. It is the first signal belonging to the signalling point from which it is operated reached by a train approaching that point. The "home" signal is placed in the immediate neighbourhood of the signal box, and so as to protect any points which may be situated there, and the "advance" is placed at some distance ahead of the signal box in the direction in which trains pass towards the next signalling point.

Signals may be divided into "stop" and "non-stop" signals: the "home" and "advance" signals are "stop" signals in that drivers must not pass them when at "danger"; the "distant" signal is a "non-stop" signal inasmuch as the driver is not required to stop at it when at "danger."

The principal indications given by semaphore signals are "on" and "off," indicating "danger" and "line clear" respectively, and the positions are the same for both "stop" and "non-stop" signals. In this respect the indications represent "condition of line" signals. A distinction, however, must be drawn between "stop" and "non-stop" indications of this character. The cautionary character of the "non-stop" distant when "on" has already been alluded to. When "off", the information given to the driver is fuller than that given by a "stop" signal in the same position, inasmuch as the interlocking between the points and signals, at any signalling point, is such that the "distant" cannot be lowered to "off" unless the succeeding "stop" signals have also been lowered. Hence a driver, when passing a "distant" signal which is "off," is assured that the "stop" signals are also "off" and the road is clear into the next section. If the signal is "on" he knows that the road is not clear, and he must be prepared to stop at the "home" signal. Hence the "distant" signal may be considered as *an indication at a distance* of the "condition" of the "stop" signal.

In addition to this characteristic of the "distant" signal, it is a "position" signal, in that it marks to the driver his position relatively to the signalling point he is approaching, and is to that extent a warning of his position. "Stop" signals are also "position" signals, but of a different character, in that they represent positions which must not be passed when they are at "danger."

A further characteristic of semaphore signals, common to "distants" and "homes," is also shown by Figs. 2, 3, and 5. As will be seen, there are as many "distants" and "home" signals as there are diverging lines at the junctions, and that the signals are erected in the same order, relatively to each other, as the diverging lines are to each other. Thus: "distant" "1," and "home" "1," refer to lin "1," and so on. Hence the "distant" and "home" signals for a diverging junction are

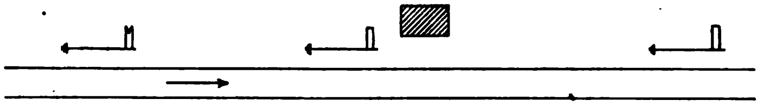


FIG. 1.

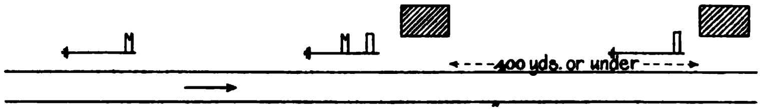


FIG. 1a.

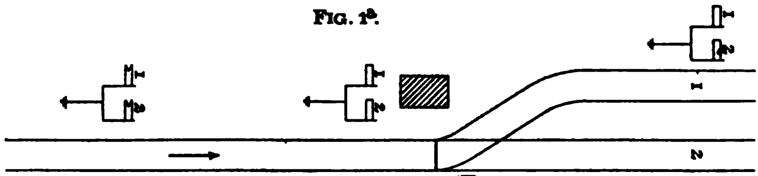


FIG. 2.

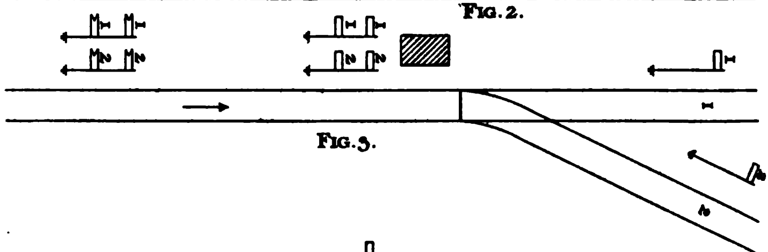


FIG. 3.

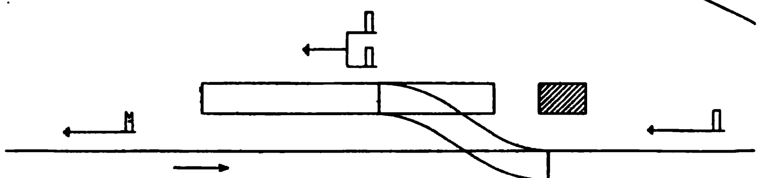
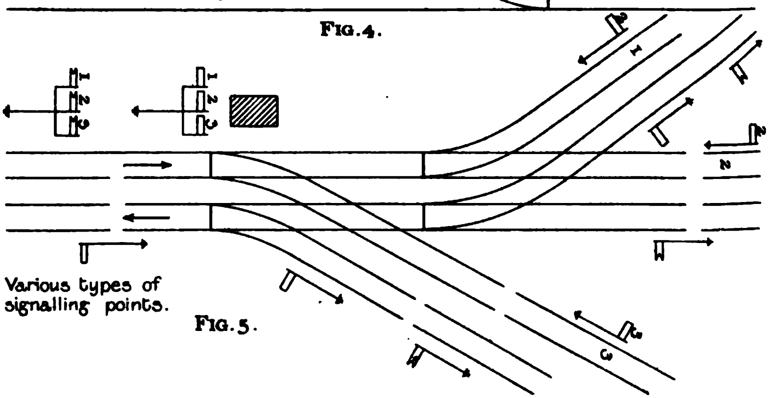


FIG. 4.



Various types of signalling points.

FIG. 5.

FIGS. 1–5.—Various Types of Signalling Points.

"route" indicators in addition to their other characteristics. The duplication of signals shown on Fig. 3—one high up on the post and the other lower down—is intended to allow the driver to see the high signals at as great a distance as possible in clear weather, and the lower signal is to facilitate his observing the indications exhibited when weather conditions do not admit of his seeing the higher signals.

Need for Supplementary Indications.—The time when the driver most urgently requires supplementary indications is undoubtedly during fogs and snowstorms, but it by no means follows that it is only during such exceptional conditions of weather that supplementary indications are of value. The history of railway accidents is not without cases where absence of mind of the driver has resulted in serious disaster in clear weather, and there have been other cases in which it has been strongly suspected that absence of mind has been the principal cause. Apart from this, however, the need for additional means of signalling during fogs, etc., is sufficient. No more strenuous duty can be imposed on a man than the driving of a 100-ton engine, with from 250 to 350 tons behind him, at a speed of 60 or 70 miles per hour, through a grey wall which never seems to end, where sight is useless, and on the rack with the endeavour to keep track of his position by the "feel" of the road, and for the explosive signal which means for him the "danger" signal—the only practicable signal given to him. Special arrangements have to be made for giving the present supplementary explosive signals, but instances are not unknown where they have broken down with lamentable results.

Point at which Supplementary Indications are Required.—Many suggestions for providing supplementary indications have been made during the last twelve or fourteen years, most of which have had as their object the giving of the indications directly on the engine, in the cab where they are most easily observed. Others, however, have aimed at producing a signal, ordinarily an audible signal, on the line.

The latter are merely variations of the present supplementary explosive signal, with the added disadvantage that they are not so readily heard. Much of the efficiency of the explosive signal is due to transmission through the body of the engine. The difficulty of conveying sounds from the line to the cab of an engine, running at a high speed, under ordinary circumstances is so great as to render such systems totally useless. Cab-signalling is undoubtedly the only method worth consideration.

Character of Supplementary Indications to be Provided.—The choice of means by which the indications shall be given is limited, and to all intents and purposes lies between the adoption of visual or audible signals, or a combination of the two. Purely audible signals leave no record; and purely visual signals need considerably more attention than the driver can easily give. With the first he may forget *which* indication he obtained after it has ceased, either automatically or by his own action; with the second he may forget to observe it. From these considerations, alone, it seems imperative that both classes of

signal should be employed, the audible signal being of the nature of an alarm, or "call attention" signal, and the visual signals giving the "condition of line" signals, and "route" indications.

Choice of Design of Apparatus.—The design of the apparatus divides naturally into two parts—that of the indicating apparatus, and that by which the indications are to be produced. The conditions to be satisfied in either class are not easily overcome. The indicating apparatus will be subject to violent vibration whatever arrangements are made, and must be capable of withstanding it. The means by which the indications are produced, if a contact system is used, will necessarily be subject to severe shocks, and must be strong. Purely mechanical apparatus for producing the indications, depending as it does upon impact, involving movement of some part against a resistance, has small chance of adoption on account of liability to failure under the stresses sustained, and the difficulty of operating such devices at the distances rendered necessary by the high speeds of trains. Where "off" signals are to be given, the cumulative effects of the multiplied blows would render such apparatus liable to constant failure. Moreover, mechanical systems, at their best, can never hope to reproduce in the cab, under the driver's immediate notice, all the information he obtains from the line signal.

The use of electricity offers much greater advantages, and enables the effects to be produced at any required distance from the operator without effort. The combinations that may be made with a given apparatus are more numerous, and the methods by which electricity can be utilised preclude the necessity for the violent shocks which are almost inseparable from purely mechanical operation. Nevertheless mechanical means may, if designed with a knowledge of the conditions to be met, be made to form a valuable auxiliary to electrical systems.

The collection of signals on the engine is a matter of the highest importance in any system of cab-signalling. In purely mechanical systems, as has already been stated, this is done by contact of apparatus carried on the engine, and apparatus fixed on the line which partakes more or less of an impact or blow. This blow may be minimised to some extent by the adoption of yielding devices on the engine or track, or by applying the contact more or less gradually by sloping devices, but the effects are but slightly reduced owing to the extremely short time during which the contacts are made, with any reasonable length of slope, etc., when trains are travelling at high speeds, and the effects are enhanced when heavy bodies are brought into contact by their inertia, and by strong control.

Electrical systems have generally to provide some form of mechanical contact between the circuits on the engine and those on the track, but as they do not necessarily involve the movement of the apparatus, their design need not follow the same lines. If efficient contact is established that is sufficient. The collection of currents from the track has other points, however, which need to be considered.

Metallic contact is necessary, and it has been thought that under the conditions of use snow and ice, or dirt, might form an objection by causing failure to make efficient contact. In any system whatever the design is the most important point. If the apparatus brought into contact is not such as will tend to clean, but rather to press and consolidate whatever the bar may be covered with, failure is sure to result.

Considerations like these have given rise to suggestions for contactless systems of collecting indications, of which Mr. W. S. Boult's system is perhaps the best known. These systems depend upon magnetic influence for the operation of the signalling apparatus on the engine, and are therefore independent of such conditions as have to be provided for in contact systems.

Requirements to be Satisfied by Supplementary Apparatus.—Having reviewed the conditions under which signalling is carried out and the characteristics of the various signals, it will be gathered that the "distant" signal is one of the greatest importance, since at that signal the driver gains information by which he is guided in his immediate subsequent actions. At that point he is informed of his position relatively to the stopping place of that signalling point, and he there obtains the "condition of line" and "route" indications if the line has been prepared for his further passage. What is required, therefore, in any supplementary apparatus is that it shall advise the driver, when he is passing a position corresponding to that fixed upon as the distant signalling point, of the condition of the "stop" signals in advance. If this is done, the driver is informed on all the points which it is necessary for him to know. From what has been said it will be seen that the following points should be provided for:—

1. The first useful operation is to inform the driver, by way of warning, of his position relatively to the signalling point he is approaching, at such a distance as will enable him to carry out any steps that may be necessary.
2. To advise him immediately afterwards of the condition of the "stop" signals he is approaching, *i.e.*, whether they are "on" or "off."
3. If the "off" signal is obtained it should be accompanied by a "route" indication, which will enable him to judge whether the right road has been prepared at a diverging junction. It should be possible to reverse this indication at some point or points before the train passes the "home" signal, in case of emergency, just as it is possible to reverse an indication with the mechanical signals by throwing them to "danger" before they have been passed.
4. If the "on" signal is obtained, the indications on the engine should be maintained until the "off" signal is received. It should be possible to receive the "off" signal at some point or points between the "distant" signalling point and the

"home" signal to prevent unnecessary delay, just as the driver is able to note the lowering of the signal before he arrives at it by the projection of his vision in clear weather. It should be possible for the apparatus to indicate to the driver at some point or points how far he has travelled between the distant signalling point and the stop signal when the indication continues at "on." It should be possible for the driver to obtain the "off" indication when standing at the "home" signal. It should also be possible to give a signal to a train standing at the "home" signal which would be of a cautionary character and distinguishable from the ordinary authority to proceed.

As corollaries, the following conditions should also be provided for :—

(*a*) The indication required under (1) *i.e.*, the "warning" signal, should be given by the natural operations of the apparatus, and should require no action on the part of the driver or signalman to bring it into operation.

(*b*) The "condition of line" and "route" signals required under (2) should be under the sole control of the signalman, should be subject to the control imposed by the interlocking, and must be such that failure shall not be liable to give a dangerous false indication.

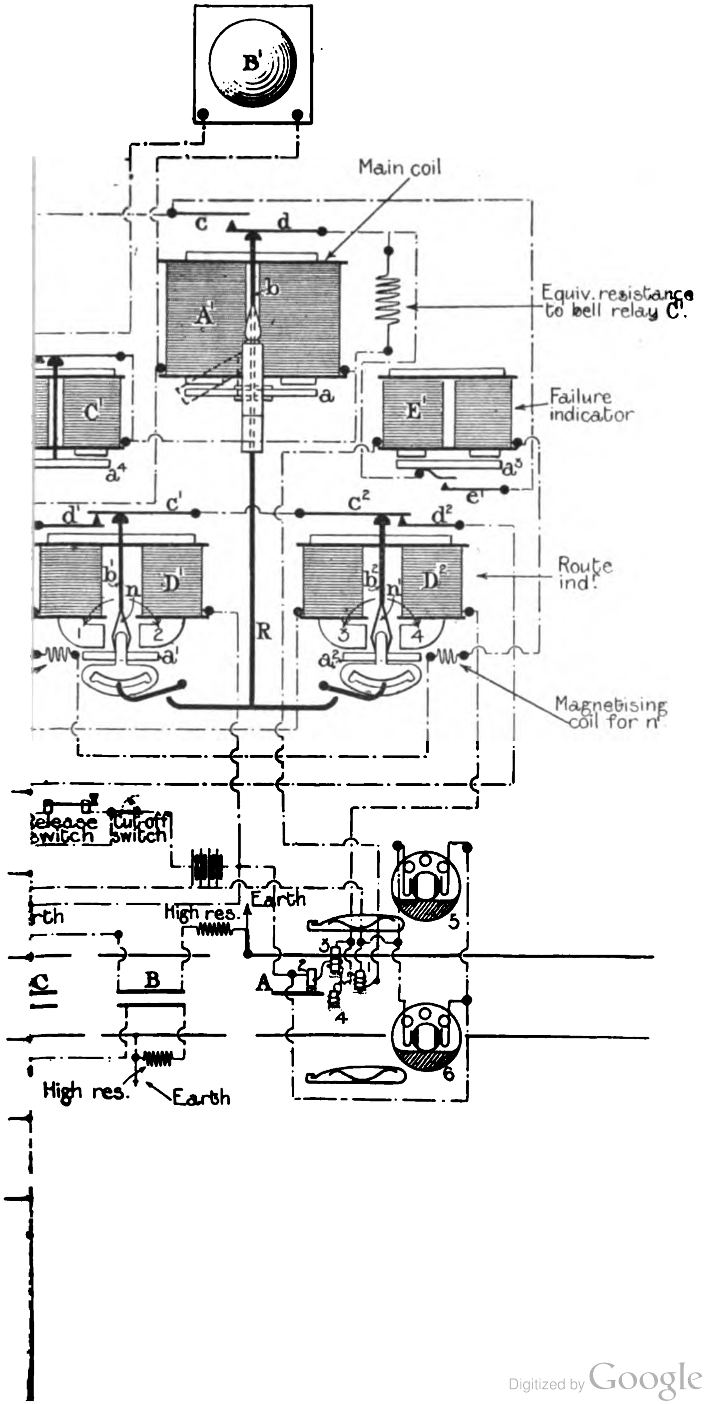
(*c*) The signalman shall be provided with indicators which will show him that the apparatus on the line for giving the signals on the engine is in order, and that the apparatus prepared is in accordance with the positions of the signal levers.

(*d*) The apparatus on the engine should be of a reliable character, easily seen and heard, and such that the indications shall, as far as possible, correspond with the apparatus it is intended to supplement. It should be self-testing and be continually in use, so that failure may be instantly indicated, and it should be so arranged that attention can be readily given and defective apparatus easily removed and replaced.

(*e*) The apparatus should be capable of easy adaptation to single or double line working.

Raven's Electrical System of Cab-signalling.

Actuation of Apparatus.—As already stated, this system is electrical, and it is designed to collect indications by the rubbing of metallic brushes (Fig. 10) carried on the engine over metallic bars (Figs. 15 and 16) placed on the line. This method of collection is not essential to the system, since it is capable of being operated equally well without contact, by causing electromagnets on the line to influence magnets on the engine. This method of collection is not now being put forward.



B'
Main coil
c
d
b
A'
a
Equiv. resistance to bell relay C'.
C'
a^4
E'
Failure indicator
a^3
e'
d'
c'
c^2
d^2
b'
n
D'
1
2
a'
R
b^2
n'
D^2
3
4
a^2
Route ind'.
Magnetising coil for n'
Release switch
Cut-off switch
Earth
High res.
5
C
B
A
6
High res.
Earth

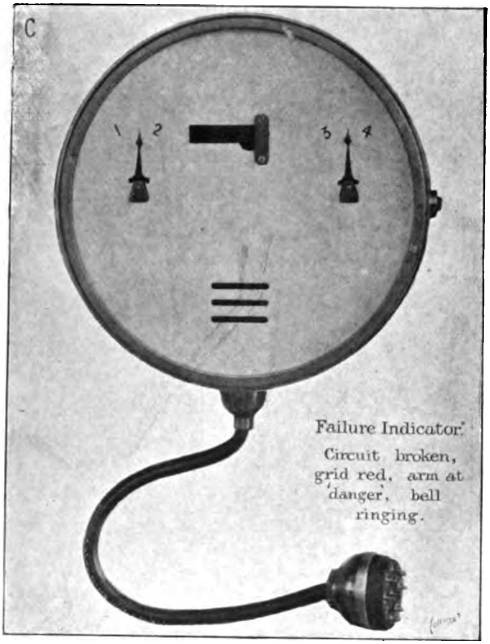


Fig. 6.

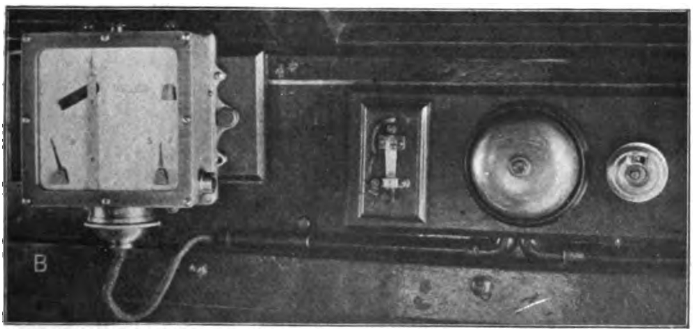


Fig. 7.—" Off " and " Route " Indications.

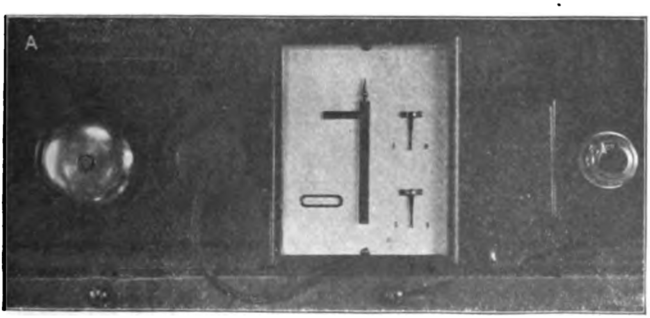


Fig. 8.—" Warning " and " On " Indications (Bell Ringing).

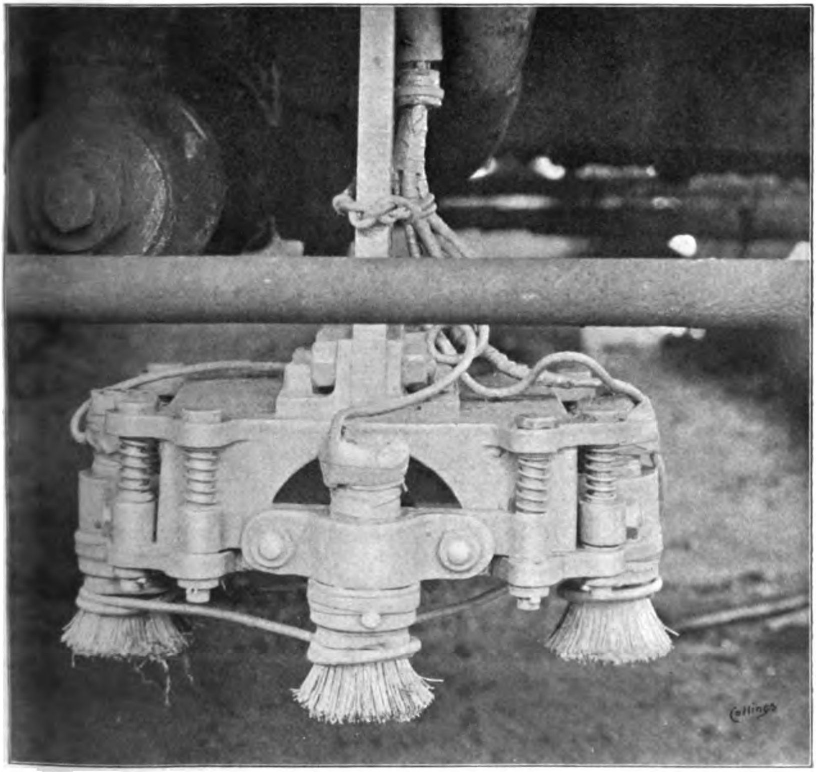


FIG. 10.—Steel Wire Brushes 1, 2, 3, and 4 carried on Engine. Showing "Failure" Indicator Circuit Wire wrapped around Brushes.

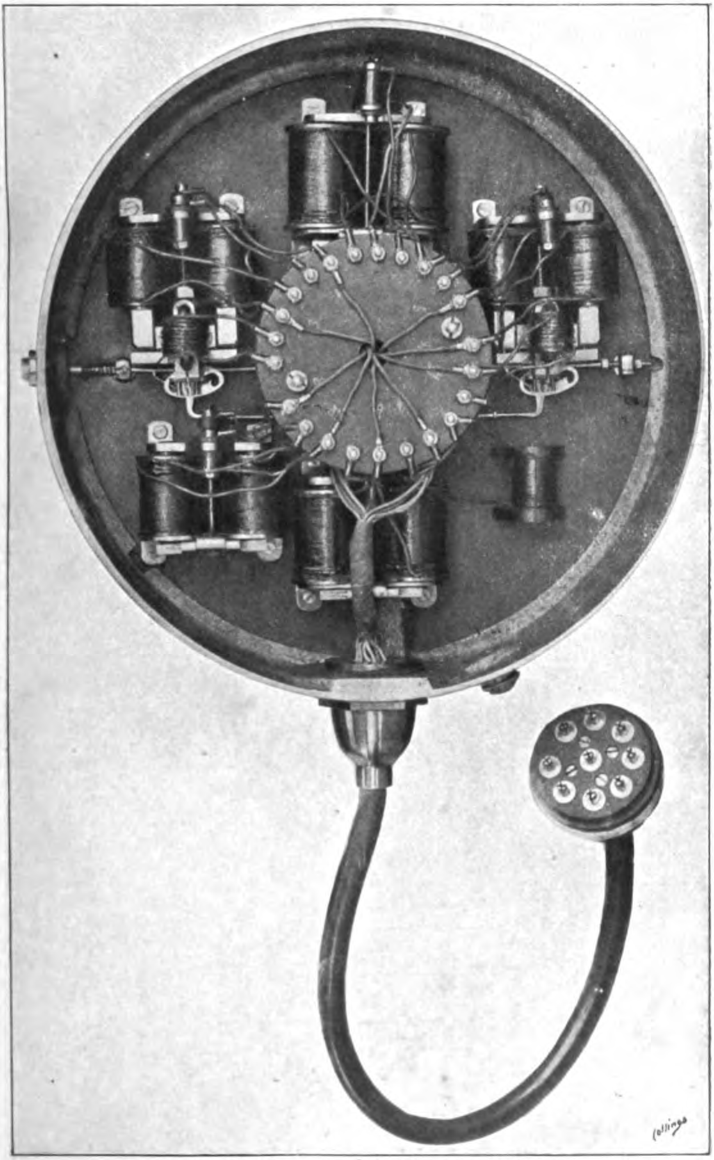


FIG. 11.—Back of Circular Type of Indicator. Cover removed exposing Apparatus.

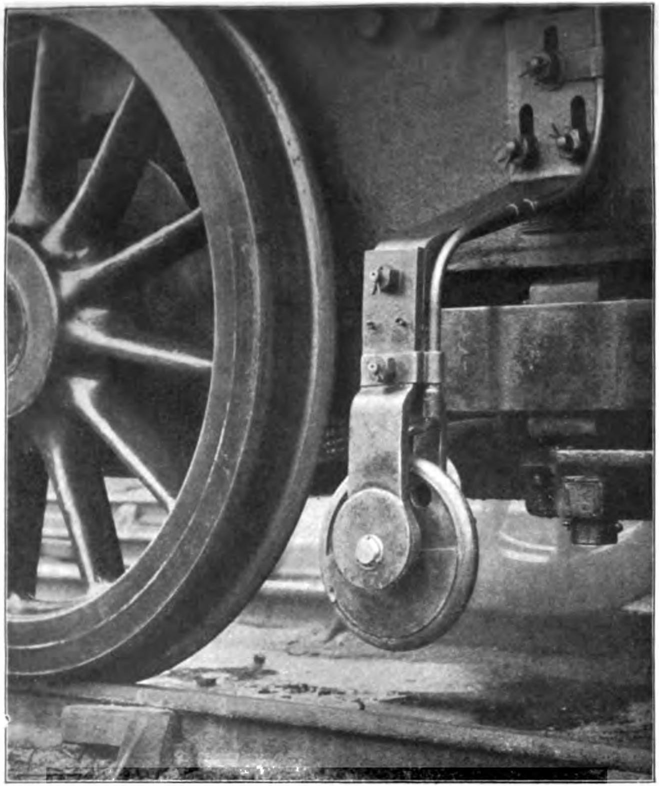


FIG. 12.—Rotary Switch fixed on Engine Bogie, showing Serrated Plate for convenience of adjustment.

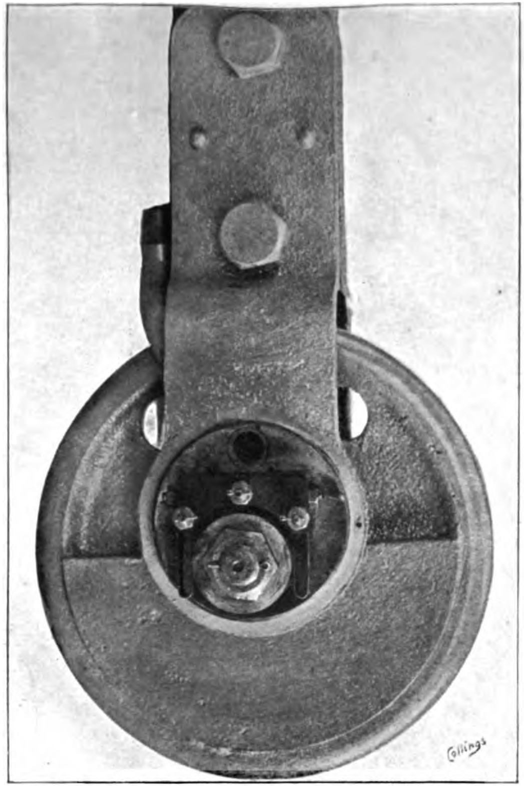


FIG. 13.—Rotary Switch with Cover removed, showing Commutator and Springs.

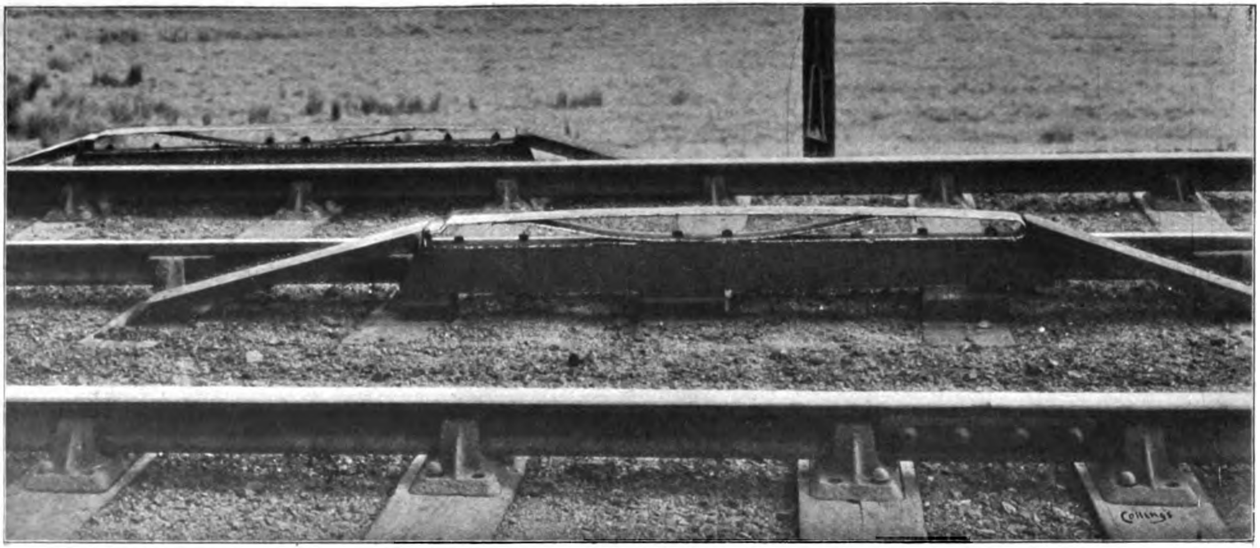


FIG. 14.—Spring or Yielding Bars with Ramps. Length of Bars 6 ft.

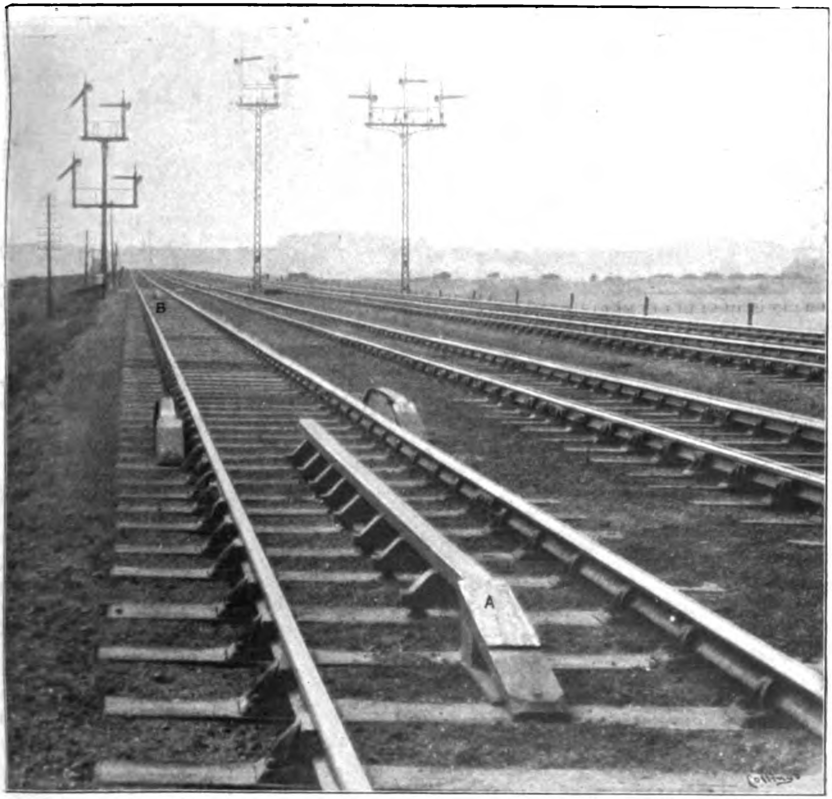
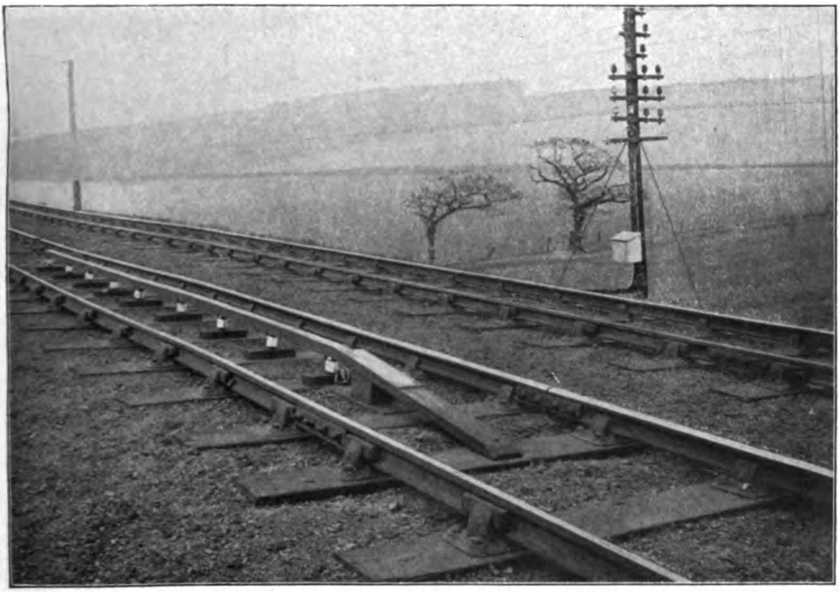


FIG. 15.—Bar A in Centre of Track. Spring or Yielding Bars in Advance. Bar B in Centre of Track in Distance. Distance between A and B 100 yards. "Distant" Signals (in duplicate) "Off" for "Route" 1. Length of Bars A, B, C, D 30 ft. Length of Bar E 60 ft.



Bar B. Mounted on telegraph insulators with ramp for protecting from hanging coupling chains, box containing high resistance fixed to telegraph pole.

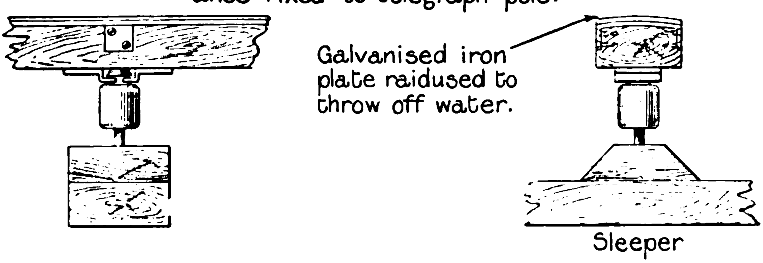


FIG. 16.

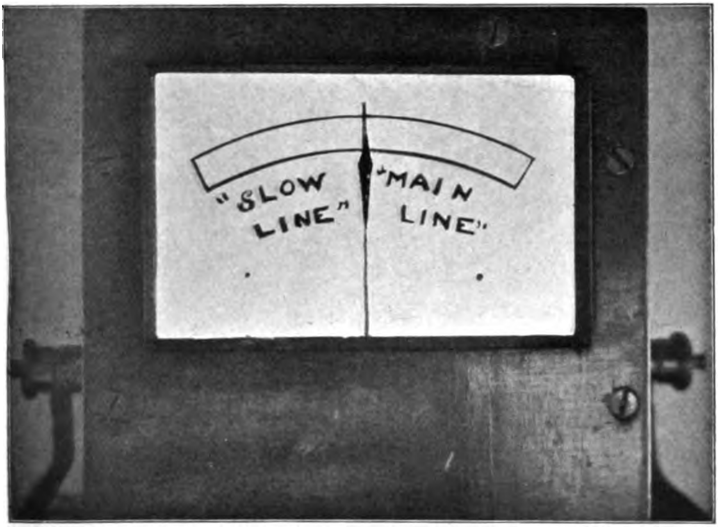


FIG. 17.—Cabin Indicator.

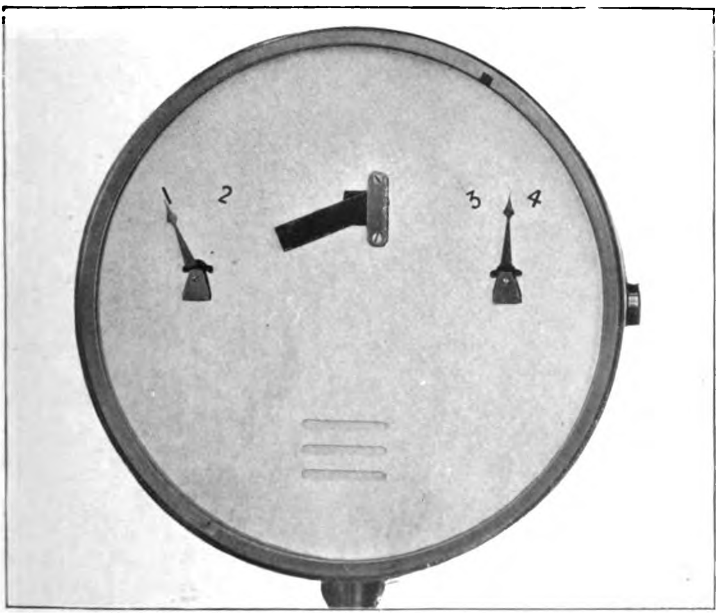


FIG. 18.—1 {Left-hand Road at Diverging Junction.
Straight Road.

Characters of Indications.—The system is one which uses visual and audible signals. The visual signals are (1) a small semaphore arm by which the "condition of line" signals are given, and (2) two small pointers showing 1–2 and 3–4 respectively, which are the "route" indicators. The audible signals, which are of the nature of "call attention" signals, are given by a bell. Besides these indicators, the instrument carried on the engine includes a visual "failure" indicator, by which the condition of the apparatus can be gauged.

Figs. 6, 7, 8 show several forms in which the indicator on the engine has been made, the circular form being the latest. Fig. 9 shows in diagrammatic form the complete equipment of engine and line circuits, the latter being for a 3-way diverging junction. Fig. 11 is a photograph of the back of the engine indicator with the cover removed to expose the apparatus.

Details of Indicator.—The action of the apparatus is of the simplest possible character, the main principle being the invariable operation of the apparatus at certain points by the natural action of certain parts without the aid of either the signalman or the driver, and the subsequent continuance of the indications resulting from the natural operations until they are stopped or reversed by the action of the signalman.

Considering Fig. 9, and leaving, for the present, consideration of the line of bars out of the centre of the space between the running rails, it will be found that the short-circuiting of brushes 1 and 2 on, say, bar A, causes a current to pass through the main magnet, A^1, by which its armature is raised, putting the semaphore arm to "danger." At the same time the armature closes the circuit of the springs *c*, *d*, diverting the current direct back to the battery after passing through A^1. Hence the armature of the latter will remain attracted to the poles as long as may be necessary for the purposes of the apparatus.

Besides passing through A^1 and the brushes, the initial current passes through the bell relay C^1 during the continuance of the short-circuiting of the brushes 1 and 2; the armature is attracted and breaks the circuit through the spring contact (*e*). This contact forms part of the bell circuit, which itself is connected in shunt across the electromagnet A^1. Hence when the armature of A^1 is raised, the current from the engine battery tends to divide, part passing by A^1 and part by the bell. The connections, however, are such that current only passes to the bell when C^1 is unenergised, and this condition only obtains when the brushes 1 and 2 are not short-circuited. When the brushes are on a metallic bar, say A, therefore, the bell is silent, but as soon as they pass off the bars it begins to ring.

In addition to passing through the electromagnets A^1 and C^1, as described, the current to the brush 1 passes through the springs c^1 d^1, and c^2 d^2, each pair of which is normally in contact. These springs are opened by the raising of the armatures of D^1 and D^2 respectively. Opening the circuit at either c^1 d^1 or c^2 d^2 obviously releases the armature of A^1, and, as a consequence, stops the ringing of the bell and lowers the semaphore arm.

Currents passing through D^1 are collected from the line by the brush 2, currents passing through D^2 are collected from the line by one or the other of the brushes 3 and 4.

Between the poles of D^1 and D^2 are placed magnetised needles n, n^1, pivoted to turn under the polarity of the poles when the electro-magnets are energised. The spindles carry the pointers shown in Fig. 6. Each spindle also carries a small metal sector, slotted as shown by Figs. 9 and 11, in which rides a small metallic loop, pivoted at the other end. The passage of a current through, say, D^1 deflects the needle to one side, and the loop drops into a recess at the end of the slot, and locks the needle and pointer on the front of the instrument in the deflected position. At the same time that this occurs the lifting of the armature of D^1 breaks the contact c^1 d^1, and lowers the semaphore arm, and stops the bell as already stated.

Auxiliary Apparatus on Engine.—The engine carries, in addition to the apparatus described, two rotary switches, of which further details are shown in Figs. 12 and 13. Each switch consists of a cast-steel wheel free to rotate, the spindle of which carries a two-part commutator, on which bear two springs. The wheel is weighted so as to take up a normal position. In this position the springs bearing on the commutator are insulated from each other, but when the wheel is rotated they are connected through the commutator. The springs are connected with the brushes 1 and 2 respectively, and each rotary switch, when turned from its normal position, connects the brushes in the same way as the latter are connected when on the bar A, or any subsequent bar of those shown in Fig. 9.

Track Apparatus.—The rotary switches 5 and 6 run over fixed bars on the line side of the general form shown on the diagram, and of which more detail is shown by Fig. 14. These bars are fixed in close proximity to the bar A, as shown by Fig. 15. Hence the rotary switches are only actuated at or near the bar A.

Turning now to the line equipment, the point represented as being approached is, as already stated, a 3-way diverging junction. The six levers shown represent the "home" and "advance" signals for each of the diverging lines. Each "home" lever is fitted with a double-pole, and each "advance" lever with a single-pole switch, which are operated by the levers in the ordinary movement for operating the signals. The "home" and "advance" levers for the lines marked Nos. 1 and 2 connect the battery in the cabin with the bars B, C, D, E, placed in the centre of the track, the only difference being that the levers marked 1 apply the positive pole of the battery to these bars, and the levers marked 2 apply the negative pole to the same bars. The two levers, No. 3, connect the battery in the cabin to the supplementary bars, and if the junction was a 4-way one, other levers would reverse the polarity precisely as is done by levers No. 2.

As will be seen, the battery is not applied to the bars unless both the "home" and the "advance" signal levers are pulled over. The mechanical interlocking prevents the levers for more than one line

being pulled over at once, or the "home" signal for one line and the "advance" for another, and therefore in the case under consideration there is no need for more than one battery.

A view of the bar A is shown in Fig. 15. It is mounted on wood blocks, which are in turn mounted on stoneware reels. The insulation is not high, as there is no need to aim at a high degree of insulation for this bar. Fig. 16 shows the arrangement for mounting the bars B, C, D, E, and a side elevation and end section of these bars.

It is necessary that these bars should be well insulated, and they are therefore mounted upon double-shed porcelain insulators of the ordinary telegraph pattern.

A further consideration of Fig. 9 will show that the preparations made for signalling to trains are indicated in the signal cabin. The two indicators required in the case of a 3-way or 4-way diverging junction are shown in Fig. 9, and fuller views of the indicator are given by Fig. 17.. When the levers are pulled over the current passes through a high resistance fixed at bar B (a box containing this resistance is shown fixed to a telegraph pole in Fig. 16), which limits the current passing before and after the engine reaches the signalling bars, but which, being in shunt with the engine circuits when signals are being given, does not affect the current to the comparatively low-resistance circuits of the engine. The resistance of the indicator is kept low with the same object. The permanent deflection is comparatively small. When the signals are being given the deflection is increased, and it can be used as an indicator to the signalman (1) of the position of the train which is approaching, and (2) whether the signals are being *given* on the engine.

Consecutive Operations.—Consider now in further detail what takes place in a typical instance. Assume that an engine is approaching the junction shown in Fig. 9, and that line No. 1 has been prepared for it to pass forward. The "home" and "advance" signal levers No. 1 are both in the "off" position, and the battery in the cabin is connected positive to line. All the bars E, D, C, and B, are connected to the battery. Bar A is never connected to the battery, and is in no way under the signalman's control.

When brushes 1 and 2 are on bar A, the current from the engine battery passes through c^2 d^2, c^1 d^1, A^1, C^1, brush 1, brush 2, and to the battery. At practically the same instant, the same circuit is separately established by each of the rotary switches 5 and 6. The semaphore arm is put to danger, and as soon as the brushes are clear of the bar, the bell commences to ring. Ordinarily the time occupied in passing over the bar is from ½ to 1 second, so that the bell practically begins to ring simultaneously with the raising of the semaphore arm.

The visual and audible indications given at bar A continue until the brushes, or brush 2, comes into contact with the bar B. A current then passes from the bar B to brush 2, thence to the coils of D^1, and the engine frame, and the rails, etc., to the battery in the cabin. The armature of a^1 D^1 is raised and breaks the circuit through the springs

c^1 d^1, lowering the semaphore arm and causing the bell to stop ringing. At the same time the polarised needle *n* is deflected so that its pointer indicates 1, and the wire loop drops into the depression and locks the pointer in the position it has taken up (Fig. 18).

The visual and audible signal given at bar A is a "warning" signal indicating locality with reference to the signalling point being approached: the reversal of the "warning" signal is the "off" signal. If the further passage of the train on its journey to the "home" signal be followed, it will be found that the bell will ring momentarily at the instant the engine passes over each of the bars C, D, E, but no change is made in the character of the visual indication.

The indicator now shows the "off" signal by the semaphore arm, and route 1 by the pointer. These indications continue until the next signalling point is reached, and are a reminder of the last signal received.

Assume that the engine has reached another bar A. The same actions take place as described for the previous signalling point, but, in addition to raising the semaphore arm, the electromagnet A, by the rod, R (Fig. 9), raises the wire loop out of its recess, and allows the pointer to resume its normal position. It is assumed again, also, that the road has been prepared for the train to pass forward, but, in this case, it is the right-hand road of a 2-way diverging junction. On reaching the bar B, the same operations are carried out with the exception that No. 2 route is shown. It happens, however, say, that at the time the engine obtains the "off" signal, and "route" indication, 1,000 yards away from the cabin, the signalman is being informed of a circumstance which makes it imperative for him to stop the train if possible, and he instantly throws his "home" signal to danger, and immediately afterwards the "advance." The engine at the moment is just reaching bar C, say, and on passing on to it, the "off" signal shown by the semaphore arm is reversed and danger shown, the "route" indicator is displaced, and the bell commences to ring as soon as the engine is completely over the bar These indications will continue as long as may be necessary.

These actions constitute the receipt of a "warning" signal, the "off" and "route" indications and an "emergency" signal, calculated to avert a disaster from circumstances which have suddenly arisen.

Assume now that another bar A, belonging to another signalling point such as is shown by Fig. 1, has been reached. Precisely the same effects are produced there as have been already described. The line, however, has not been prepared for the passage of the train. On arriving at bar B the bell stops ringing momentarily, but the semaphore arm remains at danger. Immediately the brushes have left the bar the bell recommences. The same effects are produced at bars C, D and E if no other signal is obtained at either of those points. The last bar is placed close to the "home" signal and is double the length of the other bars to allow of the train being brought to a stand easily with the brushes on the bar.

Hence the "on" signal is obtained by the continuance of the "warning" signal after the engine has passed over bar B, and that indication is continued until a subsequent indication is given by the signalman. The "on" signal, moreover, is of such a character, considered in view of the momentary stoppages of the bell by the intermediate bars, as to enable the driver to determine his position between the point at which he obtained the "warning" signal, and the "home" signal at which he must be prepared to stop. The indication afforded under these circumstances is shown by Fig. 8.

Assume that as the engine approaches the bar D, the signalman lowers the "home" and "advance" signals. When the brushes come upon the bar the "off" signal and "route indication" 1 will be received precisely as already explained.

Hence the "on" signal originally received at B may be reversed, and an "off" signal may be obtained at points between B and the first "stop" signal just in the same way as the driver sees the line signal lowered before he reaches it in clear weather by the projection of his vision.

Making another assumption, suppose that the engine has been brought to a stand on bar E close to the "home" signal, and is waiting for the receipt of an "off" indication. The semaphore arm is at "danger" but the bell is silent. The signalman lowers the "home" and "advance" line signals for the train to proceed. Immediately the semaphore arm on the engine is lowered, the "route indication" 1 appears, and the bell begins to ring and continues to do so until the brushes have left the bar.

Hence, the receipt of the "off" signal when standing at the "home" line signal is given, and the driver's attention is called to the change by the bell beginning and continuing to ring.

Suppose, now, that instead of sending the train straight away after bringing it to a stand at the "home" signal, the signalman wishes merely to call the train forward to communicate with the driver, or to bring the train forward to the "advance" signal, the signalman lowers the "home" signal and works the "advance" signal lever back and forward.

The semaphore arm on the engine will be worked up and down and the bell will ring intermittently and call the driver's attention to the character of the indication given.

Hence a cautionary or "calling on" signal can be given to trains standing at the "home" signal, and the indication is of a different character to other signals obtained in that position.

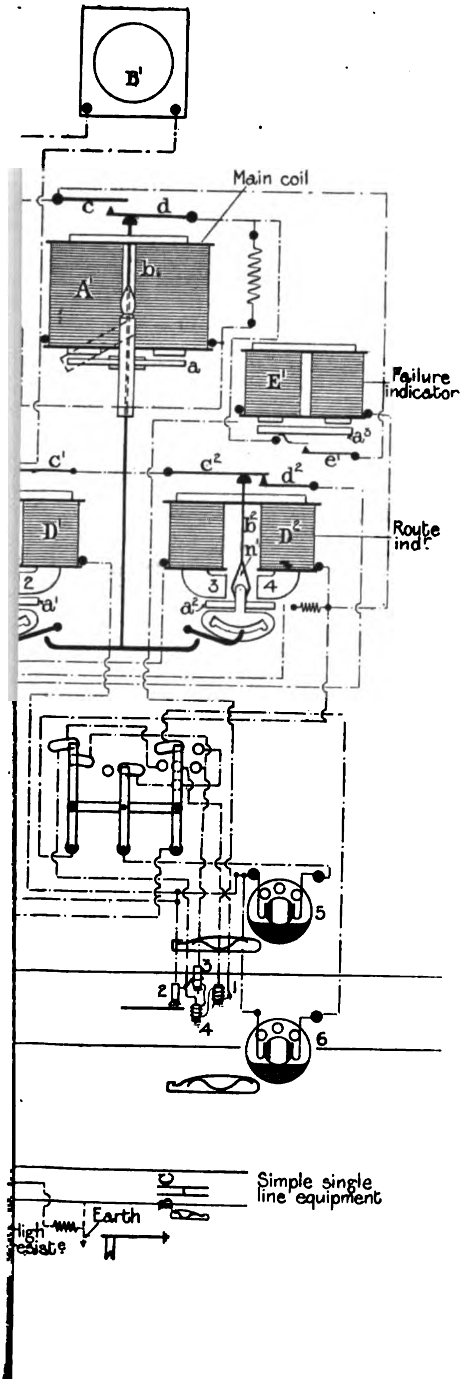
In the foregoing description the signals have been given for a 2-way diverging junction, or for a straight road. Suppose a train travelling to the signalling point in Fig. 9, and that road No. 3 has been prepared for it. The brushes 3 and 4 are connected together so that either is available for the same purpose. As shown by the diagram, brush 4 will engage with the bars parallel to B, C, D and E. The "warning" signal is given by brushes 1 and 2 on A as before. On the arrival of the engine at B, the brush 4 takes the current from the

bar, after which it passes through the coils D^2, deflecting the needle n^1 to 3, and by the raising of the armature breaks the contacts c^2 d^2. The wire loop drops into the recess at the end of the slot in the sector, and locks the "route" indicator in the position required. The net result is precisely the same as described for the "route indications" 1 and 2 with the exception that the first of another pair of such indications is given. Had the junction been a 4-way one, the action of the levers for the signals for the fourth line would reverse the polarity of the bars, and the indication 4 would be obtained. The next operation of the electromagnet A frees the "route indicators," 3–4, precisely as described for 1–2. The double brush, 3–4, is to enable signals to be obtained whether the engine is running engine or tender first, towards a 3-way or 4-way junction, and it is also used in connection with single line working to be described later.

"Failure" Indicator on Engine.—The means for testing the condition of the circuits and battery on the engine have not yet been referred to. The actual indicator is a small disc (Figs. 7 and 8) or a grid (Fig. 6) appearing at an opening in the front of the instrument, which is white when the engine battery is in operation, and red if the battery fails or is cut off. The "failure indicator" circuit is independent of the other apparatus. Current is taken to the coils of E^1 from one end of the battery, and thence to the brush 2, which completes the circuit. Interposed in this circuit are two small electromagnets, the poles of which embrace the polarised needles n n^1, and tend to preserve their magnetism. As the "failure" indicator is always in action when the engine is in work, there is a constant current available for this purpose. In carrying the "failure indicator" to brush 2, the insulated wire is wrapped around all four brushes as shown in Figs. 9 and 10. Hence any obstruction on the line which displaces the brushes will break the "failure" indicator circuit and bring it into action. The "failure" indicator magnet E^1 has a back contact, e^1, which is open when E^1 is energised, but is closed if the armature, a^3, is released. This contact simply bridges the open springs, c, d, of A^1, and if the armature a^3 is released, through the breaking of the circuit of E^1, the semaphore arm rises to danger, the bell commences to ring, and the disc or grid (Fig. 6) shows red. When the battery fails the disc or grid shows red.

Single Line Working.—Fig. 19 shows the engine equipment available for single or double line working. It only differs from Fig. 9 in the addition of a small 3-pole, 3-way switch. It is, perhaps, unnecessary to describe this in detail, as the side references afford sufficient information to follow the connections. In the centre position of the switch the apparatus is ready for double line working, engine or tender first. In the left-hand position it is available for single line working, engine first; and in the right-hand position for single line working, tender first.

The peculiarities of single line working require modifications of the track apparatus to obviate the receiving of signals on an engine



B'
Main coil
c
d
A'
b
a
E'
Failure indicator
a³
e'
c'
c²
d²
D'
D²
b²
n'
Route indr.
2
3
4
a'
a²
5
3
2
1
4
6
Simple single line equipment
High resistce
Earth

proceeding in one direction, from the track apparatus provided for traffic proceeding in the opposite direction, on the same pair of rails. A simple form of single line equipment is shown in Fig. 19, an examination of which will show that the bars A are double, but connected together electrically, and that only one spring bar for operating the rotary switches is provided near A, instead of two as in double line working as described. The additional bar connected to A is always on the left-hand side of the latter looking in the direction in which traffic passes for which the line signals are provided. Similarly the spring bars for the rotary switches are always on the left-hand side.

An examination of the switch-circuits will also show that the operation of the switch to right or left always cuts out of use the right-hand rotary switch and the right-hand brush (3 or 4) looking in the direction in which the engine is travelling. It also puts out of action brush 1. Hence only the left-hand rotary switch and the left-hand brush (3 or 4 according to whether the engine is running engine first or tender first) are operative, and whilst the right-hand rotary will be turned by the spring bars for trains going in the other direction, the brush 1 will always bear upon the centre bars, and the right-hand brush (3 or 4) will always bear on the right-hand bars, their usual functions being in abeyance in consequence of the position of the switch. The "warning" signal is given by the short-circuiting of the brushes 2–3 (or 4) and the rotation of the left-hand rotary. The "off" signals are taken up by the brush 2 for the "route indicators" 1–2, and by the brush 3 (or 4) from supplementary bars for the "route indicators" 3–4. All other operations remain as described. Hence a simple movement of the switch to one side or the other, according as the engine is running engine or tender first, is all that is necessary in passing from double to single lines, or *vice versâ*.

Recording Indications Obtained.—The system lends itself readily to the adoption of means by which the "condition of line" signals obtained may be easily recorded for each signalling point. The length of time during which the semaphore arm is maintained at "danger" differs in accordance with the condition of the line. If an "off" signal is obtained at B the time is short; if the "on" signal is received the time will be longer, and will depend upon the point at which the "off" signal is ultimately received. In any case the difference is appreciable. This difference may be utilised in order to produce marks of corresponding length upon a cylinder which rotates and travels longitudinally at the same time, by adding a marking pen or pencil to the rod *b* of A^1, so that a mark is made as long as the armature (*a*) is raised. The motion of the cylinder causes the marks to form a spiral. For places where the "off" signal is obtained at the bar B, the mark is a dot only. Where the "on" signal is obtained at B, the mark is a line of greater or less length according to the time that elapses before the "off" signal is obtained. The drum carrying the paper cylinder is driven by clockwork, and is under the control

of the signalling apparatus, so that it is only running when marks are to be made, and the driving mechanism and the marking cylinder are therefore kept of quite moderate dimensions. Arrangements are made by which the driver can produce a space longer than that provided by the design of the apparatus, and so distinguish between the marks made during one journey and the next.

CONCLUSION.

A careful consideration of the description will show how fully the apparatus meets the conditions found necessary or desirable in the introductory examination of the requirements.

I.—(*a*) The natural action of the bar A, through the brushes, and that of the yielding bars upon the rotary switches upon the engine-circuits constitute a signal, warning the driver of his approach to a signalling point, at which further indications must be looked for *at once.* (*b*) Neither the engine driver nor the signalman is required to do anything to produce this signal. (*c*) The indications being given from three independent points for double lines, and from two for single lines, provide ample margins for failures of apparatus from any cause outside ordinary maintenance of the battery. (*d*) The alternative methods for producing this indication, and the difference in their positions on the engine and track, are guarantees that anything likely to affect one means prejudicially is not likely to affect the other.

II.—The continuance of the "warning" signal constitutes an effective "danger" "condition of line" signal, relating to the "stop" signals which are being approached. The bar A is situated at a distance of about one hundred yards from B. The "warning" signal proper is therefore of short continuance only if the "stop" signals are "off." No time is lost in conveying the further indication "on" when the "stop" signals are in that position. The subsidiary indications, given by the momentary stoppages of the bell, are valuable for indicating the progress of the train towards the "stop" signal, when conditions of weather prevent the ordinary landmarks from being seen.

III.—The stopping of the "warning" signal by the return of the semaphore arm to the "off" position, the stopping of the bell, and the receipt of the "route" indication constitute a complete "off" or "line clear" signal, which, since it cannot be given unless both the "stop" signals controlling entrance to the next section are "off," gives also the exact information now given by the lowering of the line distant signal. The convention under which the "route" indications are numbered is in strict accordance with the convention under which the line signals are erected. As will be seen from diagrams 1–5, the left-hand signals refer to the left-hand road, and so on, for any number of diverging roads. The numbering of the "route" indicators is from left to right, as will be seen by reference to Fig. 9. A straight through point is always signalled as 1.

IV.—Either "condition of line" indication received at bar B can be reversed before the engine reaches the "stop" signal. That is to say, an emergency "danger" signal can be given at any one of several intermediate points if the "off" signal has been obtained; or an "off" signal can be received at the same intermediate points if the "on" signal was originally obtained at B. The number of intermediate points at which these reversals can be obtained is limited only by the number of intermediate bars provided. Additional bars are not costly, as will be obvious from the illustrations.

V.—The "off" signal can be given when the train is standing at the "home" signal equally as well as when it is travelling, and attention is called to its being given by the audible signal. The signal given when the train is standing at the "home" signal can be modified by the signalman to indicate a "calling on" signal, or the equivalent of a flag signal, and this is totally distinct from the "off" or "line clear" signal referred to already in this section.

VI.—Indications are provided in the cabin by which the signalman knows whether his apparatus is in order, and by which he can, if he desires, trace the progress of a train towards the "home" signal from the bar B, and note whether the signals appear to be given correctly.

VII.—The system, being electrical, admits of the signalling point—the bar B—being put at any distance from the cabin without affecting the efficiency. There is, therefore, no reason why it should be put at the point at which the line distant is now erected. Such signals are usually visible at some distance, and there is no reason why the bar B should not be set at this "sighting" distance. The increasing speed of trains makes it desirable that the signals given at B should be obtained as early as possible, to admit of easy braking when necessary.

VIII.—The line distant signal plays no part in the working of the system, and may be dispensed with if desired.

IX.—A further examination of the system will show that failure of the apparatus on the line will not cause a false "off" signal to be given. Any failure of the battery in the cabin, or breakage of the line wire, puts the bars B, C, D and E into exactly the same condition as A, which can only produce the "danger" indications. Contact of the line wire with telegraph wires on the same poles is not likely to give a false signal, as the apparatus on the engine requires stronger currents than are to be met with in telegraphy. Failure of the apparatus on the engine provides its own indications. These have already been mentioned.

X.—Probably the most important requirement from such a system is that of reliability. No matter what other advantages a system may possess, a want of reliability will be fatal. The most important point in any system is the means by which the signals are collected. Theoretically, the maintenance of such means of collection should be a matter of difficulty during bad weather, as snow or ice is liable

to form an insulating layer upon the top of the line bars, and thus prevent the collection of the currents. As has been said, this is a theoretical consideration, and, necessarily, owing to the absence of bars for such purposes in this country to such an extent as to allow of the forming of a definite judgment, there is little or no local evidence for or against. The experimental work in connection with this system, however, has extended over the last two winters, and no difficulty has been experienced in collecting the currents from the presence of snow or ice. The surface of the bars is slightly rounded to allow water to run off, and only a thin film of ice can form. The contact between the brushes and the bars is of a particularly searching character, due to the construction of the former. They seem to clean the bars effectually at every operation, and if more engines were equipped, this question would hardly appear at all. In any case, occasional rubbing of the surfaces of the bars with an oiled rag, which can easily be done by the platelayers when making their daily inspection, seems to prevent the formation of continuous films of ice throughout the lengths of the bars. This matter is largely a question of adequate maintenance, obtained by co-operation with other departments. Like all engineering work, cab-signalling apparatus requires attention. Such attention is always better directed towards the prevention of failures than to repairs after failures have taken place. Intelligent maintenance is all that is required.

During the two years' experimental work, it has been found that the maintenance of the insulation of the energised bars, B, C, D and E, is of much greater importance than the contact between the bars and the brushes. The first energised bars were insulated in the same manner as the bar A, and worked perfectly well in good weather. Long-continued rain was, however, found to soak the sleepers and wood blocks supporting the bar to such an extent as practically to short-circuit the battery. Since the double-shed insulator has been adopted, nearly eighteen months ago, no trouble in this respect has been experienced, and the insulators have given no trouble from breakage, except when they have been used as targets by mischievous boys.

The question of the reliability of the system is bound up in the maintenance of good insulation and perfect contact, since, although failure to collect the currents results in the maintenance of the "danger" indications, and is therefore on the side of safety, the "off" signal is of almost as much importance as the "on" signal, from the point of view of despatch of traffic, and railway companies could not countenance any apparatus the working of which unduly delayed their traffic.

On the reliability of the systems depends the important question of whether railway companies will obtain any of the financial relief which, as already stated, is most desirable. The design of the apparatus is such as to render unnecessary the whole of the complex and dangerous arrangement of fog signalling by explosive detonators now universally used by railways at great expense.

XI.—Further consideration will show that this system is capable of being adapted to use of parts, instead of the complete system as herein described, on less fully developed railways where the conditions of service are not so onerous. It is capable of being adapted to show signals for "distant" and "home" signals, or as already stated, it can be adapted to operate with the present "distant" signal only. As described, however, the arrangements are such as to give the maximum information with a minimum of apparatus, and, moreover, it provides further inducement in that, as the line distant signal is superfluous, it may be dispensed with. It is essentially a contact system in which shocks due to impact have been practically eliminated without impairing its reliability. As already stated, however, it is capable of being operated as a "contactless" system, with all operations for giving signals on the engine, carried out under magnetic influence.

XII.—The system has now been in use experimentally on the North-Eastern Railway for nearly two years, on what is the fastest short-distance train in the world, and on other express passenger trains. No attempt has been made to develop it by trials on slow trains. Under the conditions of use, the apparatus is working perfectly. The directors of the North-Eastern Railway Company have arranged for the equipment of twenty more of their express engines and of about 14 miles of their main line between Newcastle and Durham.

Dry cells have been found to give the best results on the engine, and have given perfectly good results in connection with the track-circuits. Six cells are required on the engine, and twelve cells are used in the cabin for the track-circuits. The wire for connecting the energised bars with the battery in the cabin is carried on the telegraph poles, as seen in Fig. 16.

Discussion.

Mr. Jacomb-Hood

Mr. J. W. Jacomb-Hood: The system which the author introduces to our notice is undoubtedly an extremely useful one, and I venture to express the opinion that the principles involved in it, or at least some of them, are such as railway engineers in the distant future will certainly depend upon for governing the safety of passenger traffic. I do not suppose many people would think that this particular system as it stands meets all the requirements of the average railway man who has not possibly catalogued his requirements—I mean to say it is not at all difficult for any railway man to criticise the many details of this paper. But rather than criticise I would emphasise again what I have already said, that I am fairly satisfied in my own mind that in time to come—not possibly in Mr. Pigg's time or in my time shall we see it—but in time to come railways are almost bound, I think, to depend upon some such system as that of communicating the signal of

Mr. Jacomb-Hood. the man who watches the passenger traffic from point to point to the engine. I say to the engine because this system does not communicate to the engine; it communicates it only to the driver. That introduces the first criticism I venture to offer on the whole scheme. It is particularly disappointing to me to find (because I had an idea that the scheme was slightly different in detail) that this scheme is nothing but a supplementary system of signalling to the outdoor visual signalling upon which we have been brought up, and on which railways throughout the country depend. I rather anticipated that there was an intention to disregard entirely the use of the old system in time, this being not merely supplementary, but a new system upon which railways might work hereafter; but I see from the paper that it is not so; it is essentially a supplementary system. It is a question in the minds of railway men whether it is justifiable that any supplementary system such as this should be added to the existing signalling. For what does it do? All it does is to bring an indication to the driver from the standing-post outside his cab to a signal inside his cab. That is a very important step, I admit, but I do not think it will be found in the future that the expense and difficulty involved in that step will be thought to be justifiable. The other objection to this particular system as a supplementary one is that it does not entirely remove the weak spot in the chain of connection that already exists between the signalman who controls the traffic and the man who drives the train; that is to say, as things are at present the driver is at liberty to do as he pleases with regard to taking notice of the signal, and in the future if such a system as this were adopted generally he would still be at liberty to disregard the signal. If the history of railway accidents is examined, I think it will be found that a very large proportion of accidents are due to this disregard of signals. That is the most important point that railway people who are interested in the question will consider. I would like to remind the meeting that it has been shown not to be an impossibility to introduce some system that gives the means of controlling the passage of the train by the signalman himself. I will not mention any particular device; there are several. If there is to be an advance, I am sure it is in that direction that railways generally will move, rather than in this what I may call a half-step, or supplementary system. Mr. Pigg has introduced a very interesting description of signals in the early part of his paper, and one which is new and interesting to me. He distinguishes between stop signals and non-stop signals, and he calls the distant signals the non-stop signals. That seems to me an uncommonly good expression, but it would rather lead those who had not been brought up to signalling to forget the fact that the distant signal that we are accustomed to was essentially, originally, a repeater or indicator signal of the home signal; it grew out of the home signal. It is true that as time has passed it has altered its characteristic to some extent, but still it is worth remembering that that was the original essential of the distant signal: it was a repeater.

Mr. A. C. Brown: I should like to speak on a few points with the credential of having been the first, I believe, to introduce this particular method of connection on which the details of the system all depend—I refer to the steel wire brush as a means of making the connection between the running engine and the track or *vice versâ*. In connection with my friend Mr. Burn—for it was largely due to him—we fitted such an apparatus on many railways as far back as 1898; among others the Great Northern Railway at Finsbury Park, where such a brush was put down and proved its efficiency by lasting for some two or three years, perhaps longer, and passing two hundred trains per day, making contact with every one of them, and I may add not only making contact in spite of bad weather, but also making contact in spite of paint and various articles that were at different times placed upon the contact surfaces. A brush seems rather a small detail to talk about, but after all that is the soul of any apparatus of that kind. Everything depends on the connection—whether it can be absolutely depended on to last in spite of all kinds of weather and different things that may happen to it. Every one who has had anything to do with the question of bringing up a connecting piece, having any inertia or weight, against another piece at the rate of 60 miles an hour or more, knows that unless great care is taken the apparatus will smash to pieces because the impact is so terrible. The only thing apparently that will maintain a contact under those conditions is something which is almost weightless in itself and is flexible right up to the point of contact, which the brush is. I believe that a wire brush is the very best thing that can be used to make sure of a contact on a train. Some of the details of the brushes are rather different. We had a rather better arrangement for making sure that the brushes should not disintegrate; but there is scarcely time to go into minute details. It being admitted that the brush is the best way of making contact, I am rather surprised that Mr. Pigg, or Mr. Raven, should have introduced an auxiliary apparatus which certainly does not appear to have anything like the efficiency of a wire brush, namely, the disc. That disc, coming on to a steel rail at a high speed, is, I am afraid, going to cause trouble, more especially when snow or anything of that nature drifts underneath the flexible rail. I should like to know what is going to happen when a stone from the ballast gets wedged in under that flexible rail. It appears to me that the steel disc at the side of the engine will fly to pieces, and that in the neighbourhood of the rail track of the running wheels of the train. I take it that the disc is required to continue the contact, is that so? Mr. Brown.

Mr. Pigg: No, the reason the disc is used is because it is the strongest form of lever that can be obtained for that purpose. Mr. Pigg.

Mr. Brown: I would suggest, then, that if a disc is required, would it not be better to make the disc flexible; in other words, would it not be better to place round the periphery of the disc a number of wire brushes, to make in fact a circular wire brush, so that instead of requiring a flexible rail, which has its inertia to overcome every time, Mr. Brown.

Mr. Brown. simply to push or strike an obstruction as the brush comes in contact with the rail?

Mr. Pigg. Mr. PIGG : That disc does not make any contacts ; it simply turns round and puts, what I call the commutator—it is perhaps hardly a commutator, but a closing device within it—into operation. So long as it turns round a quarter of one revolution it is sufficient for all purposes.

Mr. Brown. Mr. A. C. BROWN : I think that it would be really the best way to make a mechanical contact which would turn the disc round. Instead of having a flexible rail, which will fail, I am afraid, from the congregation of substances underneath, there will then be a flexible piece of apparatus to take the impact, and the disc will still turn as it does now, so that all that is required is done with it. A flexible wire brush apparently is the very best thing with which either electrical or mechanical contact can be made between a moving train and appliances fixed in the track.

Passing from details to the general principles of electric railway signalling, I quite agree with the previous speaker that it does seem a pity, when the railway companies are perhaps intending to fit a new system, that they should continue in that system the limitations of the old visual system. Mr. Jacomb-Hood suggested that the reason of the distant, the home, and the advance signals being used is that the driver cannot see all along the line ; but with the electric system it would be quite possible to arrange that he should see the signal throughout the whole of the block section. That could scarcely be done by a contact system, but I am able to say it can be done by an inductive system. As long ago as 1880 I suggested to Mr. Morgan, the then Managing-Director of the Telephone Company which preceded the National Telephone Company, such a system, and experiments were made on it. We laid down a wire to represent a wire running along the track of a railway, and wound a coil to the exact size and shape of a coil which could be wound on the frame of an engine. We did not at that time actually wind the coil on an engine, but we wound a corresponding coil, and we were able to keep up perfect connection from the single wire, which represented the track wire, on to the coil which represented the engine wire, and I think we could have actuated this identical apparatus. With modern means there is not the slightest doubt at the present time that such an apparatus could be actuated, not only by a wire laid along the track, but, I believe, by a wire hung in the ordinary way on the telegraph poles, perhaps 60 ft. or so from the running line. In addition a feature was attached to that method of signalling which, I think, would be very useful here, namely, that the clear signal which the driver would then have throughout the whole of his block section would depend, not merely on a momentary impulse as of the contact of a brush passing in about $\frac{1}{12}$th of a second, but on a constant repetition of a characteristic signal from the box. The driver to keep running must receive not merely one signal, but a constant repetition of a signal which can

Mr. Brown.

only affect his engine, and which is automatically reproduced from the box so long as the lever remains "off," so that no matter what other wires may get into contact with the signal wire it would be impossible for the driver to receive a clear or go on signal unless the lever of the home signal (of course the name home signal would then be abolished as there would be only one signal required for a block section) were pulled off and that signal intentionally given. The signal could on emergency be put on or against the train at any portion of the section. I am afraid that with this present apparatus a contact is only required between a battery lead (or wire) at the signal box and the particular wire to give the clear signal. I am aware that Mr. Pigg says that this apparatus takes rather more current than the ordinary telegraphs are likely to use; but one has only to cut out the resistance to get the required current, and that appears to me to be a point that wants looking after. I am sorry to have to criticise, but these points have occurred to me in the course of many years' study of the subject, and they certainly do want considering if a new system is to be brought in.

I conclude with many thanks to Mr. Pigg for his very interesting paper.

Mr. Blackall.

Mr. A. T. BLACKALL: All railway men who have studied the problem of signalling to trains in motion, and particularly signalling to them in foggy weather, must have come to the conclusion that the ideal system is one in which the driver shall have a signal given to him in the cab of his engine. Such a signal must be unmistakable. Again, in my opinion, any failure of any part of the apparatus giving the signal should always result in giving the driver the danger signal, as it does not matter at all if a driver gets a danger signal when he ought to get an all-right one, but it would matter very much if he got an all-right one when he ought to get a danger one. On the Great Western Railway, with which I have the honour of being connected, we have such a signal. We have tried it for a long time on two or three portions of the line, and on one line, the Fairford Branch, 22 miles long, it has proved so successful that we have taken away the distant signals altogether, and rely entirely upon the cab signal. The signals given in that apparatus are a bell for all-right, and a steam whistle for danger. The steam whistle would always blow except that it is prevented from blowing by an electromagnet which forms part of a local circuit upon the engine. The steam is always trying to get through it, but it is prevented from blowing by the means I have mentioned. It is obvious, therefore, that any failure of the battery on the engine or of the mechanical part of the apparatus must result in letting the thing go, and the whistle then blows. It is a loud signal, loud enough, I should think, to wake a man, even if he were asleep; in fact, on the Fairford Branch, the engine men when running bunker first stand with their back to the apparatus, and they are signalled to without looking at it. The men have every confidence in it, and I think, therefore, that all railway engineers who are trying to signal in the cab should be

Mr. Blackall.

encouraged by what we have heard is being done on the North-Eastern, and if I may venture to say so, by what we have already done on the Great Western. No electrical appliance, I fancy, is quite free from the risk of possible failure, and it is for that reason that, in designing the apparatus we use on the Great Western Railway, we were very particular to give the danger signal by the breaking down of an electric circuit and not by picking up a current. We pick up a current for the all-right signal, but we break down an electric circuit for the danger signal, and to my mind that seems to be the right line to go on. It is not quite clear to me, from reading Mr. Pigg's instructive paper, whether the system presented to-night quite satisfies the condition I have referred to of giving the danger signal in the event of failure. I am quite aware that there is an indication given when the local battery on the engine is out of order, but should not only the local battery but all the electric apparatus break down, and should the driver omit to notice the failure of the indicator, he would not even get the warning bell, I take it. I do not see how he could if he had no batteries working. In such a case might there not be a risk of the driver missing his distant signal altogether, and coming on top of his home signal before he knew his position. I offer this slight criticism in a very friendly spirit; but I am very much convinced that any failure of any part of the apparatus should result in giving the driver the danger signal. I should like to compliment the author on his very able paper, and to say that I for one have derived a great deal of instruction from it.

Mr. Johnson.

Mr. A. H. Johnson: The idea of giving a preliminary indication to show that a driver is approaching a distant signal is an old one; it, however, is a very vital one. I thoroughly agree with Mr. Blackall, and I think every signal engineer would, that it is absolutely necessary to think of first principles. One of the first principles that theory and also practice dictate is that in signalling for safety, as compared with signalling for information only, all the apparatus must be designed so that should any vital part fail by any possible means, the danger signal will be given. The ideal system to cope with that, it seems to me, would be a continuous rail running right along the railway; the signal would be held up normally by an electric current (if an electric current is to be used for the purpose at all), and if the signalman puts the signal to danger it should denude that rail of electric current, and the signal should go to danger by gravity as with our ordinary semaphore line signals. I am rather inclined to think that Mr. Blackall and his friends have solved the difficulty, because they give a danger signal by a mechanical operation, by means which appear to deal effectively with the inertia trouble. They do not depend on electrical contact for the stop signal. We should always remember that, however ingenious the safeguard, some day the battery or the failure indicator itself will fail—it will be so easy—and when it does fail the driver will not look at the indicator. I see that Mr. Pigg mentioned the possibility of doing away with all contacts, and getting the indi-

cation by magnetic induction upon the engine. That is practically the ingenious system devised by Mr. Boult, and even that system with all its ingenuity depends on an electrical contact on the engine to make the danger signal. Mr. Johnson.

Mr. ROBERT BURN: I have in my pocket a letter from Mr. Henry Oakley, Great Northern Railway, dated thirty years ago, 1877, in which he says that he has written to the General Managers of the Brighton Company and the Midland Company to say that an audible signal had been given on the Great Northern 500 yards beyond the distant signal; fifteen main line signals had been fitted, and he thought it well worthy of encouragement. I have been for thirty years working at this audible signal problem, and venture to say that one of the first points that would strike most of us in looking at this interesting and well-illustrated paper is that the system is exceedingly complicated, which means that it must be an exceedingly costly system. Although the engineering departments of a railway company will carry out experiments gladly in pursuit of safety, yet on looking at the financial side of the question one meets with a practical difficulty. Looking at this engine apparatus and at the outdoor fittings one can estimate what the cost will be. I suppose the engine fittings would cost at least £25, to say nothing of maintenance, and the signalling apparatus for a two-road signal cabin, with all these bars and batteries and wires, could not be done for less than £100. I submit that there is a more economical way, and, after all, any system which is to commend itself to all departments of a railway company must be a very economical one, and, if possible, must show a saving over the present detonating system, which is a very costly one. I venture to say that the most economical method of audible signalling is to give the signal at the roadside and not on the engine. The gentlemen who have been reading and speaking say, "No, give it on the engine." My reason for venturing to oppose men so experienced is this: First, the cost of fitting up every engine is prohibitive; second, the foreign engines that are running over the railways are an insuperable difficulty—one of the worst difficulties that I have met with in actual practice. For instance, on the Great Northern, engines of the South-Eastern Company, the Midland Company, the North-Eastern, and the North-Western, and others are met with. If Mr. Raven's system is adopted every engine of every company must be fitted with the same apparatus, and if one unfitted locomotive should go on to the road the whole system falls into chaos at once. On the other hand, a simple apparatus can be fixed by the roadside which can give a duplicate signal—a "go on" and a "stop" signal. In the first place, it can give a loud-sounding hoot from a hooter like that used on the Tube railways, to signify "stop," which can be intensified to any extent. The author says that a signal off the engine cannot be loud enough to wake up the driver. I think the only difficulty, on the other hand, is that everybody in the neighbourhood would be woke up. A danger signal, consisting of a vibrating diaphragm, loud enough to rouse the driver in his cab, would conquer that difficulty. Mr. Burn.

Mr. Burn. The "all-right" signal can be just as loud, but it can be made of a different intonation or by a different method—a bell, for instance, if it is loud enough. I have had the pleasure, in conjunction with Mr. Brown, under Mr. Harrison, Resident Engineer, of carrying out an experiment of that sort at Lamesley, on the North-Eastern Railway, and it was very satisfactory indeed. By means of the wire brush, designed by Mr. Brown, the flange of the wheel passing between the rail and the brush completed the electric circuit; the hooter and the bell sounded loudly enough to satisfy all the drivers. If they had been the only people who had to be consulted the signal would have been immediately adopted. The vital point in connection with the subject is, Keep down expense, and to keep down expense the signal must be given to the driver, not in the cab, but on the roadside.

Mr. Siemens. Mr. ALEXANDER SIEMENS: It is with some diffidence that I speak to-night on this subject, because I have not troubled myself about railway signals until quite lately. I must say that the study of a new subject is very interesting. All sorts of points come as a novelty and are quite fresh, whereas those who have studied the subject all their lives are working in a groove and do not see matters in the same light. Mr. Pigg has called attention to the same point as the last speaker, namely, that the chief object of all railways is to save money, and that, as in all engineering undertakings, the cost is the governing factor. In railway signalling the factor of cost is somewhat modified by the requirements of safety, but, nevertheless, cost is the great thing which has to be taken into consideration. In looking at the paper I have been very much struck, like Mr. Brown, with the complication of the system described. I have had the pleasure of going over the Fairford Branch of the Great Western Railway and seeing the arrangements there, which were described by Mr. Dawson before the recent Engineering Conference held in September. In this system Mr. Pigg does not propose one bar for a set of signals, but four or five, and he also goes in for two extra ones outside. On the Fairford Branch, as Mr. Blackall has told us, they have done away with the distant signals altogether, which means that a great length of rod is dispensed with and the maintenance of those rods, which is a considerable item, is saved, as well as the maintenance and lighting of the signals. In place of that a ramp is put down. It is possible to instal such a system with economy, so that the railway company is satisfied to go on with it.

Mr. Pigg, on the other hand, has rather treated the subject from the engineer's point of view, who likes to have a beautiful toy very nicely made, but, in my opinion, it is too expensive. The point has already been elaborated by other speakers, but the fatal defect in this system is that the danger signal depends on the making of an electrical contact. Mr. Blackall has already called attention to the fact that when the battery on the engine fails there is an indication, but the driver may not be able to look at the indicator, and, even if he does look, he sees that his battery has failed, but that tells him nothing. It does not tell

him at all where he is. In any system where there is a mechanical indication at a certain point in the line, quite independent of any electricity, there the danger signal is given at a known spot. Even if the battery fails, even if the connection with the signal-box fails, a danger signal is given, which is the necessary requirement to keep the train safe. I also believe—I am not absolutely certain on the point—that it is a requirement of the Board of Trade that the audible communication for danger and for safety shall be different, and in the Great Western system on the Fairford Branch that is the case. As Mr. Blackall has told us, the danger signal is a whistle and the safety signal is a bell. In that case the driver knows what he is after; but in Mr. Pigg's apparatus the driver hears the bell, he has to turn round in order to learn what the bell means and take his eyes away from forward, and that is not always desirable. It is certainly very interesting to learn from Mr. Jacomb-Hood that railway people would entertain a system which does away with all the visual signals now existing, and which would only deal with indications on the engine, especially if those indications not merely informed the driver what he had to do, but did it for him. I do not believe there is the least difficulty in doing that, but this is not the time to discuss the subject. Mr. Siemens.

Mr. H. D. ANDERSON: Being connected with the Great Western Railway, it may perhaps be of interest to say that when we first thought of introducing some indications supplementary to the present, which may be described as silent semaphore signals, we drew up a list of what we thought must be indispensable features in connection with any such supplementary form of signals, and on reading Mr. Pigg's paper, and comparing the substance of it with those requirements, I find that his system complies with all except two. One of these requirements was that the danger signal, in order to be perfectly reliable, must be produced by mechanical means; and the second was that if audible indications were to be given two separate ones would be necessary, one for safety and one for danger. With these two important requirements the Raven system does not comply. I think we can cordially agree with all that Mr. Pigg states as to the desirability for some supplementary indications, especially in foggy or bad weather. I should like it to be clear that what I say to-night must be regarded as purely from a traffic man's point of view; I have not sufficient technical knowledge to criticise or deal with the technical part of the paper. One thing that struck me in connection with the scheme was that the normal position of the visual signal given on the engine is "all right," whereas the normal position of the running track signals is "danger." That seems an anomaly to me which is open to criticism. I may say in this connection that when we installed some automatic electric signals recently (the usual practice in connection with those signals is to show a normal "all-right" position), we introduced a novelty in retaining the standard position of normal "danger." Then, from a practical point of view, I was not quite clear from Mr. Pigg's demonstration what would happen in a very common case of Mr. Anderson.

Mr. Anderson.

working. For instance, we will say a signalman wants to admit a train within his home signal, waiting for permission for it to go forward. The train approaches the home signal, and the regulation is that when it is arriving there, and before it has stopped, the signalman can lower that signal to allow the train to draw inside. I gather that if the signalman does that the visual signal on the engine still shows "danger" and the bell will continue to ring. How is the engineman to shut the bell off while the signalman is keeping that train outside his box until he is ready to let it forward? I do not think the engine is provided with any means for shutting off the bell after the brushes clear the bar. Another point is with regard to single-line working. An engine driver passing from a double to a single line, or *vice versâ*, has to reverse the main switch on his engine. That seems to me to be another weakness in connection with the system, as there is imposed upon the driver an additional duty to remember, and that, too, at a time when he very often is much occupied. In the apparatus that we are using on the Fairford line there is no change necessary; the driver simply runs from the double line to the single line or off again without having anything to do at all; the apparatus is always ready for work. A good deal has already been said about the cost of the Raven system. It seems to me, from a management point of view, that, as has been already said, the cost is the prime factor almost. Safety, of course, is the first consideration, but there must be a limit to the expenditure as well. Although the Raven apparatus is very ingenious, I am afraid that its cost and its complicated character would be too much for any British railway to face at the present time. In addition I would go further and say, Is such an elaborate system really required? What is the necessity that we aim at? I think Mr. Pigg in his paper states that if one can give an engineman at the distant signalling point an indication of how the stop signals ahead stand that is all that is really required. That is what we have done in connection with our audible system on the Great Western Railway; at the distant signalling point the man gets a distinctive audible indication as to how the signals ahead stand. While I am on that point I may mention that there seems to be another complication in the Raven system by the fact that the indication at the distant signalling point is not given by the distant signal lever itself, but by the action of the home and starting signals, so that it is possible to have a confliction of signals there, as the signalman might leave the distant signal at danger, whereas the home and advance signals might be off, and the driver would then get the "all-right" signal on his engine at the distant signalling point, although the track semaphore distant would be showing "danger."

I might add, in conclusion, that in the G.W.R. system, as we have only one ramp and are absolutely certain to obtain a mechanical movement of the engine shoe every time an engine passes over it, we consider it is not necessary to provide any indication in the signal cabin, as, of course, the driver is bound to get a signal every time the engine shoe is operated. In the Raven system I notice that an indi-

cator is provided to show the signalman whether the driver is receiving signals when the engine brushes are passing over the bars. Mr. Anderson.

Mr. H. J. JEFCOATE: I have endeavoured for the last four or five years to establish a system not only with the Great Western, but with two other companies, and the question of cost spoken of has been thrust upon me as being almost insurmountable. The system I have developed is called the "Safe" signalling system, and is worked as follows: At the distant signal there is a plain 24 ft. T-bar mounted and coupled to the ordinary wire from the same lever which operates the ordinary semaphore as now. The lever is pulled by the signalman; he lowers the semaphore, and in doing so brings the T-bar from normal danger in the raised position to "line clear" in the lower position. The danger signal at the distant is given by means of one of three plungers resting under the step or any portion of the frame of the engine coming in contact with the said bar and recording right the way through, until switched off, that the distant signal is against him, so that in operation the driver can slow his train down, bring it under control, and then attend to his indicator by touching a button. But that is not all. He has had a distant signal against him, and he knows that he is going to have a further signal at the home, either the "home on" or "line clear," so still keeps going slowly forward. If the "home on," it is given by a second bar, which is 4 in. wider away from the rails, and comes into contact with the second plunger, which gives him the "home on" automatically, so that only when he is getting that signal in the cab of the engine is he free to proceed. The moment that ceases, having been warned at the distant, he has the train under control and stops. In actual practice several experiments have been conducted before practical men, and lately before Lieut.-Colonel E. Druitt, R.E., of the Board of Trade. On those occasions each of these signals was obeyed. Afterwards a demonstration was given with the cab of the engine entirely enveloped with a tarpaulin, making it as dark as night, all outside signals being shut off, and after setting the train back about a mile below the distant signal, the train was set forward at the greatest speed it was capable of, and each of the signals was obeyed. There is no electrifying of the line. Mr. Jefcoate.

The criticisms which have been made have been directed to the cost of the Raven system, and I have been putting a comparative system against it, intending to justify the expense (if the means adopted are right) as against the present unsatisfactory and unreliable fogging system.

Mr. RAYNAR WILSON: I would like to come to Mr. Pigg's support in one matter if I may. Mr. Siemens has dealt with the question of dispensing with the distant signal. I think it will be found in the paper that Mr. Pigg says that that may be done. As to whether what he proposes is the better plan as compared with what is done on the Fairford Branch in dispensing with the distant signal, I am rather doubtful. On the Fairford Branch the distant signal lever is retained Mr. Wilson.

Mr. Wilson. for the purpose of electrifying the ramp, but Mr. Pigg couples his connections to the home and starting signal levers. The result is that the driver always gets a clear signal if those signals be "off." I know there are times and seasons when the stop signals may be lowered, but it is advisable to keep the distant signal "on"; in that case perhaps the Fairford system is the better. Objection has been made to the large number of ramps. I think that is one of the bright features of the system, inasmuch as thereby the signalman and the driver keep in touch with each other directly the driver gets in the zone of the signal-box; but there is this objection, that if the driver gets a clear signal when he runs on the first ramp the bell rings and continues ringing all the way through, and I think I am right in saying that it will continue ringing just the same, even if the signal be thrown to danger. Should he not have noticed that the arm is raised, he would not know but that the bell continued ringing as a "clear" signal. Now I wish to raise what is perhaps rather a minor matter. I am wondering what will be done at positions on the line where track-tanks or water-troughs are placed. I would like to say that the paper and the discussion to-night bring out the truth of the author's remark at the beginning, namely, "The problem to be faced is not a simple one."

Mr. Cooke. Mr. F. W. Cooke (*communicated*): In the introduction to this paper financial difficulties are mentioned, but too much importance should not be attached to these in the early or experimental stages; but every facility should be given, both financially and otherwise, in such an important matter. With regard to this particular system the problem seems to have been well thought out and the greater part of the ground covered.

I quite agree that the signals should undoubtedly be placed in the cab of the locomotive, where sufficient space should be provided for them. They should also be both audible and visual. The system in question evidently meets this requirement.

The route indicator is a very desirable adjunct, but I think in the earlier stages it might be dispensed with, as it somewhat adds to the complication and might retard the adoption of an otherwise very valuable invention, but should be added as soon as the absolute reliability of the distant signal arrangements have been demonstrated.

Wire brushes in various forms for picking up the current have, of course, been used more or less for the last forty years, and there is a great deal to be said both for and against them. There is no doubt that if they are properly fixed and well looked after they are the most reliable form of contact possible, but unfortunately such things as snowstorms must, I am afraid, seriously interfere with any system of picking up current in the manner described where the power to be dealt with is so small.

Again, a great deal depends upon the method of attaching the brushes to the locomotive, and whether proper facilities are provided for replacing them quickly. A failure of these brush contacts has evidently been anticipated, hence the provision of auxiliary rotary

commutators. But it is possible the latter would be liable to derangement when travelling at a very high speed. Nevertheless, the whole arrangement seems to be one of the best of its kind ever devised, and deserves every encouragement. Mr. Cooke.

There is one method which might be utilised for supplying the current for actuating this apparatus which does not seem to have been considered—that is, picking it up from a live insulated section of the running rail. This would do away with the necessity for extra contact bars and would have the advantage of always being clean and ready for use.

Mr. J. A. F. ASPINALL (*communicated*): It is not generally known that an arrangement which has many points in common with that spoken of in Mr. Pigg's paper was in use on the Northern Railway of France in 1877, and papers were read on the subject before the Institution of Civil Engineers of France both in 1876 and 1877 by Mr. Lartigue, who was the inventor of the system. Mr. Aspinall.

In the centre of the track a piece of timber raised above the rail level and covered with sheet brass formed what was called a "crocodile" on account of the peculiarity of the shape, and suspended from the locomotive was a brush made of a bunch of copper wires making a contact with the "crocodile." The general effect of the arrangement was that the brake could be applied, steam could be shut off, and a bell rung in the signalman's cabin in the event of any trains running past signals.

The arrangement was in use for many years on a certain section of the Northern of France Railway, and may still be in use, though I have no definite information on this point.

The fact of the matter is that these arrangements of automatic control, whether mechanical or electrical, have been invented over and over again, though the difficulties of their adoption, however perfect they may be in themselves, have hardly ever been appreciated by those who are not actually connected with railway working.

Mr. JAMES BOWMAN (*communicated*): It will be generally admitted by railway signal engineers who endeavour to keep pace with the requirements of modern railway traffic, that some such system as the author has described is required to meet the conditions on main lines, and more especially where high-speed traffic is in operation. Mr. Bowman.

The necessity for a system which will give the driver both audible and visual signals in the engine cab has been proved repeatedly by the lamentable accidents which have from time to time occurred through drivers having omitted to observe the usual semaphore signals.

It is instructive to examine Board of Trade reports and find the number of accidents due to drivers failing to observe their signals not from sheer carelessness, but perhaps owing to adverse weather conditions such as fog, snow, or even rain, or it may be through the momentary divertion of the driver's attention. The present system of signalling has a particularly weak point when we come to consider the distant signal.

Mr. Bowman.

Consider for a moment the case of a driver approaching the distant signal at night where that signal is the only light on the post, as in Fig. 1. If through any cause (and there are many) the driver should unwittingly pass the signal at "danger" he has no warning signal to tell him so, and should he then sight the home signal at "danger" he might easily assume it to be the distant signal and consequently overrun it, intending to stop at the next signal. Since there is no distinction between these signals at night it is difficult to see how he could be blamed for the error. I understand some railway companies have tried the illuminated arm or a distinctive light for the distant signal, but even if a distinctive light were in universal use I still think it would be very advisable to give an audible and visual signal in the cab as the driver enters the block section.

Whatever claims for consideration a cab-signalling system may have, either as a supplementary system or as a means for superseding the present main line signals, there can be no doubt as to the advisability of the distant signal warning arrangement. Of course, in foggy weather, as the author has remarked, the driver has a warning by detonator, but then it takes time to get the fogmen to their respective posts, and much may happen before the audible warnings are in use.

I believe many of the accidents caused by drivers overrunning signals may be traced to the driver having lost his "sense of position"; that is, he may estimate he is much further from a signalling section than is really the case, and may be actually in the section when he thinks he is only approaching it. Especially is this possible when the usual surroundings by which he notes his position are a little obscured.

I observe the author states that the system described is not intended as a substitute for the existing mechanical signals, but simply as a supplementary system, by which I understand him to mean that the driver would be required to look out for the usual semaphore signals in addition to observing cab signals. I cannot see why cab signalling should not ultimately be adopted as a substitute, since it can fulfil so completely the functions of the existing signals. I would not advocate it as a substitute for siding signals, as there is no call for such an arrangement. It would certainly take a considerable time to equip all the locomotives and track with the apparatus, and during that time it would be necessary to have both systems in use.

If cab-signalling were adopted as a substitute for existing arrangements, it would still, of course, be necessary to provide fixed indicators by the side of the line to locate the point at which the driver must stop if he should get a "danger" signal in the cab. The obvious advantage of adopting it as a substitute would be the absence of mechanical apparatus between the signal lever and the signal post, and hence the economies on initial cost and maintenance, which might help to balance the additional expenditure in cab and track equipment. If it were not for the serious question of cost, it would be much more

Mr. Bowman.

satisfactory to have continuous means of communication between the signal-box and the cab throughout the signalling section, so that the signalman could reverse the cab signals at any point in case of emergency.

I quite agree with the author's opinion regarding the design of apparatus. An electrical system appears to be the only rational method of transmitting the necessary information from the track to the cab when travelling at high speed. Referring to Fig. 9, I observe that the line circuits are operated by means of switches at the levers in the signal-box, so that when the signal levers for any one line are pulled the "direction" and "line-clear" signals applicable to that line are given in the cab. In this arrangement there appears to be no provision made for the proper detection of the facing points over which the train will pass. It is quite true that the signal levers cannot be pulled unless the locking bar and point levers are in the required positions, any such error being made mechanically impossible by the interlocking between the levers. But experience has proved that it is possible to have the point levers in the correct position, and yet the points may not be properly set for the passage of a train. This may be due to ballast between the point blade and the rail, or blades getting out of gauge, or perhaps a failure has taken place between the lever and the points; even the pulling of the lock lever does not ensure that both point blades are in their proper position. In existing mechanical signalling the wires for the junction signals are made to pass through a mechanical or electrical detector at the facing points, which ensures that, although the signal lever may be pulled, no signal can be pulled "off" unless both blades of the facing points are in the correct position for such a signal. Not only is this the case, but the Board of Trade requires that where such facing points are more than 200 yards from the signal-box the lock-bolt must also be fitted with an efficient detecting device. This arrangement guarantees that not only are the points in the correct position, but that they are rigidly bolted in that position before the signal can be pulled "off." Considering that such precautions are taken with the existing signals, I think it would be very advisable, if not compulsory, to detect in a similar manner the signals given in the engine cab, otherwise the driver might get a "line-clear" signal in the cab when the facing points were in a dangerous condition. There is also the further objection of conflicting signals.

Since it is impossible for the mechanical signal to be at "clear" when the points have not obeyed the point lever, the driver would be confused between the cab signal at "clear" and the ordinary semaphore signal at "danger," thus defeating the object of cab signalling. Detection of the cab signals for junctions could be very simply obtained by leading the wires from the switches at the levers through a detection-box at the points, in which suitable contacts could be arranged. Such a detecting device would then ensure that the point blades were not in a dangerous condition when the "clear" and "direction" signals were given in the cab, and would further be a guarantee that the mechanical

Mr. Bowman.

and cab signals would not contradict each other. Facing-point detection is, perhaps, the most important consideration in modern signalling apparatus outside the signal-box, and ought therefore to be a feature in the design of any system of locomotive cab signalling.

It would be very interesting if the author could give any information regarding the approximate cost of the cab and track equipment he has so ably described. It is the question of cost which seems to be the only plausible objection that engineers can raise against automatic cab signalling as a principle. If the author could give any figures regarding current consumption and maintenance of apparatus, these would also be acceptable.

Mr. Waite.

Mr. R. H. WAITE (*communicated*): The subject of automatic railway signalling is one which has no doubt been occupying the minds of electrical engineers and others for some time past. The question has always arisen in my mind as to whether the adoption of any class of automatic signalling, used in conjunction with the human element, enhances the reliability in the working of trains in clear weather, for reliability is the predominant factor. In the case of fogs and snowstorms it also appears very questionable whether the old-fashioned audible signal, the detonator, can be surpassed as regards reliability.

With reference to the system described in this paper, I note that no mention is made of any provision whereby the brakes of the train might be automatically applied in the event of the driver ignoring a stop signal which is "on." This could be arranged to work in conjunction with any electromagnetically controlled system of signalling in a variety of ways at a trifling extra cost.

The Chairman.

The CHAIRMAN: Gentlemen, I am sure you will wish to express to Mr. Pigg your very hearty thanks for the interesting paper that he has given to us this evening.

The resolution of thanks was carried by acclamation.

The meeting adjourned at 9.33 p.m.

NEWCASTLE-UPON-TYNE LOCAL SECTION.

DISCUSSION, *December* 9, 1907.

Professor Thornton.

Professor W. M. THORNTON: The author is to be congratulated on such an excellent paper, the details of which I feel sure must have taken probably about five years to get together.

I should like to know if Mr. Pigg does not think it likely that the driver will pay attention to the visual signals on the engine and not pay sufficient attention to the bell signals. I think also that with this system if the driver had a large number of signals, it might be difficult for him to remember exactly at which cabin he was, and I suggest that if possible a specific signal should be given at important points to let him know exactly where he was. As regards the means to be adopted, I consider the electric system of signalling is the only one likely to be a complete success in foggy weather.

Mr. G. RALPH: The system of cab signalling is obviously of greatest use in snowy weather, but it seems to me these would be the most likely times for the wires to break down, in which case we should be as badly off as we were before. I suggest that it would be advisable to run the wires underground in very exposed places, such as we have on the north-east coast. Mr. Ralph

Mr. F. O. HUNT: I should be glad if Mr. Pigg would say to what extent the fixtures upon the engine interfere with the "load-gauge," as it should be clearly understood that the brushes diminish the already slender chance of a man, lying between the rails, being passed over by an engine without serious injury. Mr. Hunt.

I would suggest that the efficacy of the cleaner's wipe with an oily cloth upon the contact bars arises from the effect of surface tension, by which the water assumes the globular form, and therefore on being frozen forms buttons of ice, which are easily swept from the bar by the passing contact brush.

Mr. W. CROSS: I should like to ask whether there is not some risk of the mechanism in the engine cab being tampered with. The number of special switches and apparatus in the cab would seem to render this possible. Mr. Cross.

Mr. CLAGUE: Can the author tell us whether any special provision has been made in the design of the signalling apparatus to overcome the exceptionally severe effect of vibration when the train was moving? Mr. Clague.

Mr. J. R. ANDREWS: With reference to the question of contact of line wire with telegraph wires I should like to ask the strength of current required to operate the cab-signalling apparatus. Although stronger than used on ordinary railway telegraphs, it is probably less than the current used on the high-speed automatic telegraph circuits of the Post Office, some of which are carried on railway telegraph lines. It is thought that contact with such wires would probably actuate the signalling apparatus on the locomotive. Mr. Andrews.

Mr. J. PIGG (*in reply*): It is somewhat surprising to find Mr. Jacomb-Hood expressing the opinion that it is questionable whether the expense of a supplementary system to the present visual signals can be justified. Three sources of economy are indicated more or less completely in the paper: (1) Displacement of the "distant" signal; (2) abolition of the present supplementary fog-signalling; (3) prevention of accidents due to errors of signalling. Each of these involves considerable expense to railway companies at present, and substantial relief would be afforded under each of these headings. Mr. Pigg.

It is true that with such a system as Mr. Raven's the driver can still disregard signals wilfully; but I have yet to meet with my first experience of this kind, and am not aware of any recorded instance where it has been proved that a driver has disregarded signals which he was conscious were against him. Drivers have often disregarded signals under misapprehension, and it is just this state of "mental coma," as it has been called, and the exceptional conditions arising

Mr. Pigg. from fog, etc., that Mr. Raven's apparatus is intended to meet. It may safely be said that disregard of signals by drivers is due to want of observance or to inadvertence; and it should be said that practically no body of men working under such onerous conditions can show better results than the drivers of railway trains. Mr. Raven may claim that his apparatus effectually informs the driver of his position at every signalling point whether he can see the line signals or not, and affords similar information to the fireman, who becomes a much more effective check upon the driver than is possible under the present system, seeing that his duties as a fireman often prevent him from keeping a look-out for the line signals. Short of shutting off steam and applying the brake, Mr. Raven's apparatus provides the safeguards necessary to prevent disregard of signals for misapprehension. Even if the apparatus performed the operations necessary for stopping, those operations would have to be regulated, so far as the degree or rate of operation is concerned, by the driver. But even in this respect Mr. Raven's apparatus is capable of adaptation to the application of the brakes. As a matter of fact, in an adaptation of the apparatus for use on the North-Eastern Railway Company's electric coaching stock, the arrangements included the operation of a whistle from the train brake pipe, and the application of the brakes as well as the operation of the electric signalling apparatus. These arrangements were perfectly successful, and have been incorporated with the patents.

When all is said and done there is some point from which, as a starting point, the driver must be trusted to do that for which he is paid. If he is given means which leave him no excuse for disregarding the signals, he may be safely left to control the locomotive. The time for a substitutional change in signalling is not yet. The financial difficulties in the way preclude any immediate change of this character. A general change in the power for tractive purposes on railways may bring about the change indicated by Mr. Jacomb-Hood, but, as also indicated by that gentleman, that time is not yet, and may not be in his time or mine.

Meanwhile, however, the matter is somewhat urgent, and requires to be dealt with at once in the most convenient and effective way. All experience goes to show that changes in railway practice are of slow growth, and consist for the greater part of adaptations. In this case there will be plenty of time to make such proposals as Mr. Raven's system profitable to users before it will be displaced by the greater changes already indicated, which are not to come in our time.

Mr. Brown has claimed to be the first to use a steel wire brush for making connection between the engine circuits and track circuits. I agree with him that it forms by far the best method for the purpose, inasmuch as it is possible to obtain a good contact without involving destructive shock or blow. Mr. Brown seems to be under misapprehension with regard to the rotary switches, which provide alternatives for giving the "warning" signal. The discs do not pick up

current from the yielding bars. The rotation simply closes the engine circuit in the same way as is done by the short-circuiting of the brushes by the first bar. There is not much chance of the yielding bars getting blocked. They stand about 4 in. above the track rail, and so are out of the way of heaps of ballast. The pressure of the disc on the yielding bar is only such as to give a depression of from ¼ in. to ½ in. As the distance between the upper and the lower plates is about 4 in., the question of blocking up does not arise. Both the bars on the track and the disc are yielding in their directions of motion, the bars vertically and the discs horizontally, in virtue of the turning movement. Some of these discs have been running for months, and have never failed in the slightest degree. Mr. Pigg.

Mr. Blackall said that it does not matter if a driver gets a "danger" signal when he should get a "clear." I beg leave to differ with him on that point. No railway company could tolerate a system which led to the delay to traffic involved in the unnecessary stopping of trains through failure to obtain the "all-right" signals. Again, Mr. Blackall desires any supplementary system to give the "danger" indication when failure of the indicating apparatus on the engine takes place. Why? It is not a correct indication. It is telling the driver that a signal is against him when it may be off, or when there may be no signal in the neighbourhood. Why not tell him at once that the apparatus is out of order? The requirement that the apparatus should give the "danger" indication is simply a relic of the necessary condition for the line signal-arm. There the driver is not required to discriminate between an arm that is off intentionally from one accidentally lowered. The position of the arm is his absolute guide. With supplementary apparatus the case is different, so different, indeed, that it seems unnecessary to refer to it further. Mr. Raven's apparatus is specially arranged to show when the battery fails, and to distinguish between such an occurrence and another failure, such as the carrying away of the brushes. It is easily possible to arrange an audible signal in connection with the failure of the engine battery should it be found necessary, but one is always afraid of the terrible word "complicated."

Mr. Johnson's ideal of a continuous rail running right through the railway is one that would be expensive. Naturally, of course, it could not be a *continuous* rail, as it is necessary to distinguish between different block sections, and it *need* not be continuous between the last "stop" signal of one section and the "non-stop" signal of the section in advance. Mr. Johnson appears to object to electrical contacts for any purpose, which, if carried to its logical conclusion, would be bad for electrical work of any kind. Even with Mr. Johnson's ideal continuous rail, a contact of some kind would be necessary. There seems, from my experience, no reasonable ground for doubt of the contacts established with Mr. Raven's system, when the insulation is maintained. As stated in the paper, this seems to be a far more important point than the establishment of a contact.

Mr. Pigg. Mr. Burn's estimates of the cost of installing Mr. Raven's system are preposterously high, and it would seem that he can hardly have considered the construction shown as carefully as he might have done. Mr. Burn's historical remarks are very interesting, but rather beside the mark. On the question of providing the supplementary indication on the line side, there is, of course, room for more than one opinion, but the confusion that is likely to arise when audible signals are employed in places where several parallel lines run together will be disastrous to any such system. In addition, any system of the kind is likely to prove an intolerable nuisance in the neighbourhood of towns.

Mr. Alexander Siemens is, by his own confession, a new recruit to the study of railway signalling, and thinks that those fresh to the subject see more clearly than those who have been longer engaged in its study. It may be so, but generally it is the older students who get the "fun." It is no part of my duty at present to criticise the system in use on the Fairford Branch of the Great Western Railway, but Mr. Siemens, like many others, seems to consider the "danger" signal given in that system a "mechanical indication," which it assuredly is not. It is quite possible to conceive of accidental circumstances arising which would prevent the "danger" signal being given at the proper time. Moreover, the applicability of a system is only proved after prolonged experiments under the most onerous conditions of use.

Mr. Anderson gives no reasons why the "danger" signal *must* be mechanical, and qualifies his requirement for different "danger" and "all-right" audible signals by "if audible signals were to be given," which, I take it, means, "if audible signals only were to be employed." There are many reasons why purely audible signals are not the best, some of which are given in the paper. Mr. Anderson's remarks respecting the normal position of Mr. Raven's indicator and the normal position of the line signals are indicative of thought carried out on too straight a line. It is true that the normal position of line signals is "danger," and that the position of Mr. Raven's indicator *between signalling zones* is "off," but there are no line signals between such zones. What was aimed at in the design of Mr. Raven's apparatus was a permanent indication of the last signal received until the next signalling point is reached. Moreover, Mr. Raven's apparatus complies with the "normal danger" principle before the train arrives at the signalling zone. Mr. Anderson's instance of drawing a train within the "home" signal is dealt with in the paper, where a distinct cautionary signal is referred to. If Mr. Anderson refers to the drawings he will find a small, normally closed switch, the opening of which will release the indicator. Personally I do not think that there is likely to be any difficulty in the operation of the switch for single line working, any more than there is in getting the engine driver to attend to the numerous other duties that his calling entails upon him.

I do not quite clearly comprehend Mr. Anderson's last point. If the "home" and "advance" signals are "off," and the driver gets that intimation at the distant signalling point, he cares nothing for the

position of the "distant." As a matter of fact, such a "conflict of signals" may appear any day if the signalman has not a clear run for a train. Mr. Raven's system is intended to give the "all-right" signal for the "home and advance" without the aid of the "distant." If a driver can see the line "home," and "advance" signals are "off" he will not bother himself about the "distant," since he knows that the operation of the two former are not dependent upon the operation of the latter. Mr. Pigg.

I may point out that the omission of an indicator in the cabin in the Great Western system leaves the signalman without knowledge of the working of his appliances and is liable to lead to delay.

Mr. H. Raynar Wilson is in error in thinking that the bell continues to ring when an "off" signal has been obtained at the first energised bar. The bell instantly stops ringing, and is only momentarily rung at the moments when the brushes are on the succeeding bars. It may be pointed out that the ringing of the bell in this case is from the cabin battery. If the signals are thrown in the driver's face after the receipt of the "off" signal the bell will re-commence ringing precisely as was done at the non-energised bar. The indications, therefore, are clearly distinctive. With regard to the running of engines over water troughs, I may say that the engines equipped with Mr. Raven's apparatus have regularly run over such troughs at Northallerton and Belford, and in no case has any damage been done. These sections of the line are not equipped for such signalling, but, for experimental purposes, the engine circuits are maintained, and in no case has an indication of any kind been given when passing over such troughs.

Mr. F. W. Cooke's suggestion to dispense with the "route indicators" in the initial stages of adoption would, I am afraid, lead to a good deal of expense, and would mean the scrapping of the first indicators obtained. I do not quite follow Mr. Cooke's suggestion to use an insulated section of track rail from which to pick up the current required to give the signals. The time during which such currents could be picked up would be extremely short as, as soon as one wheel of the engine got upon the insulated rail, it would no longer be insulated and no current would be available. In such a system, moreover, the brushes would be constantly in use and the wear would be enormous.

I am well acquainted with the "Croco" system of the Chemin de Fer du Nord, particulars of which appeared in the technical papers at the time. Personally I do not believe that copper wire brushes on a brass bar is the best combination for ensuring contact. Copper bars have been used in connection with the experimental work on Mr. Raven's system, but were abandoned on the score of expense when the present galvanised iron bars were found to be equally efficient.

Mr. Bowman's remarks on the desirability of point detectors deals entirely with another phase of signalling than that under notice. Point detection is, of course, a matter of great importance, but I submit that it refers to the means of actuation. The present detectors being sufficient for the control of the levers actuating the present line

Mr. Pigg. signals, will necessarily be sufficient for the signals given under Mr. Raven's system, since these signals are only given by the operation of the levers by which the line signals are themselves operated. Facing points detection should and is of such a character that unless all the movements necessary have been effectively carried out the signal levers are inoperative. The fact that the latter can be moved is sufficient to show that the locking and detecting movements have been effected. I regret that I am unable to give Mr. Bowman any figures with regard to the cost of the system. Reliable figures can only be given when the present extension of the system has been in use some time.

With reference to Mr. Waite's communication and the question of whether automatic signalling will enhance reliability in the working of trains, I can only say that where deficiencies are admitted to exist as in railway signalling as at present carried on, the provision of additional facilities, having as their object the obviation of the deficiencies, can only result in good. As regards the operation of the train brakes in connection with the system I have already pointed out that this can be readily done by Mr. Raven's apparatus, but with that or any other apparatus the application must be regulated by the driver.

It has been somewhat of a surprise to me to hear the term "complicated" used during the discussion to describe Mr. Raven's apparatus. Surely nothing could be simpler than the arrangements by which the effects were produced. The whole arrangement of the indicator consists of—

1. A main operating circuit on the engine which can lock itself in the operated position.
2. Two circuits (only one of which is used at a time) which control the main circuit.
3. A separate failure indicating circuit.

Practically there are therefore only three circuits on the engine, and two of them belong to the engine circuit, and the other is operated from the line.

On the line for all signalling points up to 2-way junctions there is a single circuit, to which the bars are connected in parallel. For 3-way or 4-way junctions there would be two independent circuits.

Another point in the design which has been kept in view is to arrange the parts, so as to reduce to a minimum the mechanical work which has to be done by the electromagnets. Still another point has been the recognition of the fact that metallic circuits, and circuits using earth, can be arranged in conjunction without interference. Where does the complication come in? It is not in the apparatus, and it certainly does not come in with any necessity for continual adjustment. Beyond the trifling wear of the brushes, there is not an adjustable point in the apparatus. Looking to the general trend of the criticism, it would seem that Mr. Raven is to be congratulated on the fact that practically no fault has been found with the principles underlying his design, or the accuracy of the reasoning upon which the results aimed at and obtained were based.

Professor W. M. Thornton asked if there would not be a danger of the driver paying attention to the visual signals on the engine and not paying sufficient attention to the bell signals. The latter, I may say, are simply "call attention" signals. The visual indications are the significant signals. No need for an identification signal for the various cabins, etc., such as is suggested by Professor Thornton, has ever been found. Drivers of trains know from the "feel of the road" to within comparatively short distances where they are under any circumstances, and under the ordinary conditions of weather their exact position is known. Mr. Pigg.

Mr. Ralph's suggestion that it might be desirable to put the line wires underground to prevent breakages in stormy or snowy weather, is one that time only will show. The other wires for signalling purposes are run on the ordinary poles, and it cannot be denied that they sometimes suffer damage. In such cases, however, if the block system was suspended, cautionary running would be imposed, and the conditions become greatly different from those ordinarily obtaining. Still the subject is one of great importance, and requires keeping under observation.

One speaker has suggested that a considerable staff will be required to keep the apparatus in order. I cannot, of course, reply to this definitely, as the equipment so far is meagre. I do not, however, anticipate that it will be necessary to engage a large *additional* staff—the railway company already has a "large staff"—as there will be no difficulty in training some of the present staff in all that is required.

No tampering with the apparatus, such as Mr. Cross suggests would seem to be possible, has yet been experienced.

Mr. Clague asks what has been done to guard against the effects of vibration. In the indicator little has been specially done. Arrangements were made by which the movement of the armatures of the electro-magnets is as great as possible, and the pole-pieces are shaped to get as long a pull as possible. Hence, when the armature is released the vertical component of the parallelogram, of which the armature is the diagonal, is comparatively large. Besides this the free end of the armature is hung to the mechanism which it actuates, and is not held by a back stop. The mechanism, again, is carried in guides which, whilst allowing free motion in the required direction, do not allow of much motion in other directions. No trouble from the effects of vibration is now experienced. With the earlier home-made instruments vibration was rather troublesome at times, if the engine was at all lively.

Replying to Mr. Andrews, it may be said that the current required for the operation of the rotary indicators, and to release the semaphore indicator, is about half an ampere. The question of contact with other wires, such as the Post Office wires carried on railways, is akin to the question raised by Mr. Ralph, and will require to be kept under observation.

LEEDS LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN.

Professor G. D. ASPINALL PARR, Member.

(*ABSTRACT.*)

October 24, 1907.

Gentlemen,—Allow me, first of all, to express my sincere thanks for, and also my sense of appreciation of, the honour you have done me in electing me Chairman of this Section of the Institution of Electrical Engineers for the ensuing Session 1907–8. It will be my earnest endeavour to merit the confidence which you are thus good enough to place in me, and in conjunction with my Committee and yourselves to further the best interests of the Section.

As a spectator, if I may use the phrase, "behind the scenes," I can assure you that we owe a deep debt of gratitude to the members of former committees and their chairmen; to our late Secretary, whose services we have already suitably and gratefully acknowledged, and to our present Honorary Secretary, Mr. Dickinson, for the time and labour spent in making the Section the success that it is.

With the time at my disposal I would like to make a few brief remarks on the training of professional engineers, particularly with reference to those specialising in electrical engineering.

Much has appeared in print on this important and widely debated subject, but instead of absorbing valuable time in the recapitulation of the main points at stake, I will merely refer you to the very exhaustive report of a special Committee, representing *all* branches of engineering, appointed by the Council of the Institution of Civil Engineers, and dated April 7, 1906, on the "Education and Training of Engineers," and which is reprinted in the *Journal of the Institution of Electrical Engineers*, No. 180, vol. 37, 1906.

While I fully concur in general with what is therein stated, namely, that a youth should leave school at about the age of seventeen, then take a year's training in works, and afterwards take his three years' college training, subsequently taking a further one or two years in works, I would emphasise the fact that there is often considerable difficulty in gaining admittance to works for the one year after leaving school, due presumably to the youth not being of much use to the

employer at this stage and during so short an apprenticeship. We know well that commercial and business instincts must necessarily predominate in such places, but if the difficulty is to be removed the employer, for the sake of the coming generation and in the advancement of the industry, should show a philanthropic spirit in the matter. If the youth is to be an electrical engineer I feel convinced that he will derive the most benefit by taking the year in a purely mechanical engineering works.

The very nature of electrical engineering necessitates an intimate knowledge of the theory of applied electricity—especially with alternating currents—before he can understand the whys and wherefores of the many special features connected with the manufacture of electrical engineering appliances in electrical works. It would therefore be much more to the advantage of the student to enter electrical works after his college training.

I venture to think there can be no two opinions that the training of a good all-round electrical engineer at the present day is more arduous and difficult than that of his good all-round brother mechanical engineer, for the former must know a considerable amount of ordinary mechanical engineering work in addition to his electrical work, which in itself covers a vast field, while the mechanical engineer can manage to get along in his own line without appreciable electrical knowledge, although even this is becoming more essential to his existence each year. This being so, it would seem only right and in the best interests of our profession if manufacturers and station engineers would afford somewhat special facilities to young men for entering their works after a college training. It is only reasonable to suppose that they would like to be first assured that the individual was reasonably intelligent and worthy to be helped on in this way, which information can easily be obtained from his former instructor.

I am glad to say that a few members of this Section, holding high positions, and able to help in this way, are very good in doing so with regard to our own students, but I would again say that I sincerely hope many more will follow their example in the future, and I should then feel that I had accomplished a good object if I have been the means of suggesting to some, to-night for the first time, such a course. The principal of a department naturally feels more than ordinary interest in the welfare of the students whose work he has watched for three or more years. I venture to suggest that it would be of the greatest help to such a principal if the manager or chief engineer would apply to him when a vacancy occurred in his works for an improver.

Unity is strength, and I should be more than gratified to find my colleagues of this Leeds Local Section writing to me on this subject, and giving my students the first opportunity of improving themselves at least in works within the boundary of Yorkshire.

DUBLIN LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN.

THE PRODUCTION OF POWER FROM PEAT.

T. TOMLINSON, Member.

(*ABSTRACT.*)

November 21, 1907.

Successive failures in attempts to deal with peat as fuel on a commercial scale have been so discredited that there is great danger that Irish engineers and capitalists—remembering these failures and not inquiring into the causes—may allow the exploitation of our great industrial asset to pass into the hands of outsiders—to their profit and not to our credit.

I am, however, firmly persuaded that through work already done and advances already made it is a very ordinary bit of engineering work, and that it can be undertaken whenever the capitalists will find the money. Lest it should be supposed that this is an over-statement, let me put the matter in a way which will appeal to engineers. In *Engineering*, vol. 83, p. 650, 1907, an account of a recent test of a peat gas and ammonia recovery plant was given as follows, the gas being used to drive a gas engine: "The method adopted in the experiments at Winnington was Dr. Caro's process for the gasification of inferior fuels by means of air and superheated steam. The experiments were made in the Mond gas plant, and the resulting gas utilised in the gas engines; the superintending engineer was not informed that he was burning a special mixture, and he did not notice any difference. The peat came from Italy, the special machinery having been constructed for that country. Altogether 650 tons of this peat were gasified. The peat contained about 40 per cent. of water—the percentage varied—and in the dry state 15·2 per cent. of ashes, volatile substances 43·8 per cent., nitrogen 1·6 per cent., and 34 per cent. of carbon. A ton of dry peat yielded 1,780 cubic metres of gas of 1,360 large calories (about 150 B.Th.U. per cubic foot), and 118 lbs. of ammonium sulphate; this amount of ammonia was really obtained, and not only calculated. The gas generated was partly utilised for raising the steam required for the process, and also for evaporating the ammonium sulphate, and it gave, in addition, 480 horse-power hours in

the internal-combustion engine—that is to say, the ton of dry peat yielded 480 horse-power hours, in addition to 66 per cent. of its contents of nitrogen as ammonium sulphate. An estimate of the cost shows that under such conditions peat and producer gas has a good prospect of being financially successful. Not reckoning the value of the ammonium sulphate, the horse-power hour cost less than 0·06d."

A fortnight later Mr. E. S. Mond wrote, referring to the above quoted article: "These tests were carried out solely by my company at its engineering works at Stockton-on-Tees, where the Mond plant is manufactured, and it is quite correct that the peat gas was turned into the gas engine driving the works by ordinary Mond gas without the change being noticed by those in charge of the engines. In consequence of this, we have now concluded a contract with the Società per l'Utilizzazione dei Combustibili Italiana, Milan, for a 2,000-H.P. ammonia-recovery peat-gas plant for a central electrical distribution station to be driven by gas engines consuming peat gas. This plant is now being constructed at our works, and we expect that it will be operating at the end of this year. We have also utilised successfully Swedish and other peats."

It does not appear from the extract (which is all the information I have) that the waste heat is to be utilised in the Italian case to dry the peat, and under the climatic conditions of Italy this may not be necessary; but I am perfectly satisfied that it cannot be relied on in Ireland, where we have 2,500 million tons of easily accessible fuel, but water-logged to the extent of 90 per cent.

The one thing which distinguishes the scheme of utilisation from all others tried here is this, that it makes available for the absolutely necessary drying 60 per cent. of the total heat value of the peat. After it has thus been made possible from an engineering point of view, the recovery of the sulphate of ammonia makes it profitable as a commercial undertaking, or perhaps it would be more accurate to say that the value of the sulphate of ammonia per ton of dry peat covers, and, in my opinion, more than covers, the cost of getting the peat and reducing it to such a state of dryness as enables the rest of the drying process to be carried out in the producer. Let us first consider the question of the value of the sulphate of ammonia per ton of dry peat, which, it is scarcely necessary to say, is recovered by passing the gas through sulphuric acid. On this point there is a fair agreement.

Engineering, in the extract quoted, gives 118 lbs. "really obtained and not only calculated." Mr. Rigby, in a paper read in March, 1906, before the Engineering and Scientific Association of Ireland, gave 130 lbs., and I have a recent analysis of peat samples, taken by myself from three different levels in an Irish bog from which an expert in producer plant states 85 lbs. per ton of dry peat is recoverable; the figure varies according to the percentage of nitrogen in the peat and the manner of working the plant. Taking the last figure, we have 1 ton of sulphate of ammonia for every 26·5 tons of dry peat. Now, a ton of sulphate of ammonia is worth £12, and the sulphuric acid is worth

about £1 10s., leaving a net value (exclusive of labour, interest, and depreciation of plant) of £10 10s., or 7s. 11d. per ton dry peat—say, 7s. Now 7s. per ton of dry peat is 3s. per ton of peat containing 40 per cent. of moisture (air-dried peat), so that virtually the problem of peat utilisation is narrrowed down to the getting of peat equal to air-dried at a cost as near 3s. per ton as possible. If it can be got at that price we have fuel free.

It is not possible in this country to get air-dried peat at 3s. per ton with manual labour and open-air drying, as is the universal practice in Ireland; but there is no reason why it should not be got and dried at this price if machine getting and drying by waste heat be adopted. Of machine getting and treatment we know nothing in this country, where the cheapness of seaborne English coal makes peat valueless except to supply local requirements, but in other countries where fuel is dearer machinery is extensively used. The following particulars give an idea of the cost of operation of some of the systems. The first (Brosowsky) is a simple hand-operated machine, of which 13,000 were made for Pomerania and Mecklenburg during the first five years of its introduction. It is also the simplest. With this machine the yield, reduced to dry peat, is said to be 2 tons cut and laid to dry per man per day, and the cost for labour would, therefore, be 1s. 6d. per ton of dry peat. A much more elaborate Swedish machine (the "Anrep") is stated to deal with the equivalent of 42 tons of dry peat per day of 10 hours at a cost of 3s. 4d. per ton of dry peat, and takes as power 10 brake-horse-power hours per ton of dry peat, which seems a very excessive allowance. A still more elaborate German machine, the Schlickeysen, or Dolberg, is said to do only 3·8 tons of dry peat in a 10 hours' day, at a cost, inclusive of hand digging of peat, of 2s. 6d. per ton of dry peat, with an expenditure of 5 B.H.P. per ton of dry peat. The first machine gives peat in sods of the wetness of the bog (say 90 per cent.), and it would probably be used for getting a summer harvest of air-dried peat when the season favoured, for no man wants to do by machinery and artificial heat what Nature, when in the mood, can do more cheaply. The second machine is one of a very numerous class in which the peat is macerated, forced through nozzles, and cut into briquettes. The briquettes contain about 80 per cent. moisture. The third employs maceration and pressing in a modified sewage sludge press; it delivers the peat with 50 to 60 per cent. moisture. It is then most effective, but it is slow, and the necessity of using bags to contain the peat during compression, which appears to be absolutely necessary where a high degree of compression is attained, is not desirable; but, on the other hand, it is effective in the extraction of moisture, it necessitates no atmospheric drying—with the inevitable handling—it delivers the peat in a suitable form for artificial drying, and its stated cost of operation is well within our requirements.

That the work can be done at or about this price of 2s. 6d. per ton of dry peat I have been assured by Mr. Lennox, an engineer and expert in this matter of peat getting and drying, and the inventor of drying apparatus now in successful operation.

The vital question then is: Is there enough waste heat available to deal with the moisture contained in the peat after the surplus moisture has been pressed out by power, or drained and dried out by exposure? First let us clearly know what we have to do. Turf *in situ* may contain 90 per cent. of moisture: it can be fed into the producer with 40 to 45 per cent. of moisture, the heat of the escaping gases (which must in any case be cooled before use) extracting that amount of moisture from the peat as it approaches the combustion zone. By pressure, or drainage, and exposure, the 90 per cent. of moisture can be reduced to 70 per cent., so that the problem reduces itself to this: Is it possible to reduce peat from 70 per cent. of moisture to 45 per cent. of moisture by the waste heat of the gas engines at a realisable efficiency of utilisation of the waste heat? To fix our idea, let us consider 100 lbs. of dry peat (chemically dry). At 90 per cent. moisture this will be associated with 900 lbs. of water; at 70 per cent. with 233 lbs.; at 50 per cent. with 100 lbs.; at 45 per cent. with 82 lbs. We have, therefore, to evaporate $233 - 82 = 151$ lbs. of water, and, taking 1,100 B.Th.U. per pound of water, we shall require 166,100 B.Th.U. If drying by compression or exposure to 60 per cent. moisture were resorted to, we should require to evaporate $150 - 82 = 68$ lbs., requiring 74,800 B.Th.U. Now, 100 lbs. of dry peat contains 864,400 B.Th.U. (from the analysis already referred to), and of this 648,000 is realisable as gas—the Mond trial gave 675,000. Assuming this gas to be used in a gas-engine with a thermal efficiency of 30 per cent., 194,400 B.Th.U. will be given as power ($= 76$ indicated-horse-power hours), and 454,900 B.Th.U. will appear as waste heat in exhaust and cooling water. The required efficiency of utilisation will then be $\frac{166,100}{454,000} = 36$ per cent. if peat be dried to 70 per cent. moisture, and $\frac{74,800}{454,000} = 16$ per cent. if peat be dried to 60 per cent. moisture.

The question then arises, Is that a realisable efficiency in actual practice? I have but one recorded trial from which it is possible to estimate the efficiency. In a 10 hours' test of the "Dobson" dryer as recorded in the "Report of the Bureau of Mines," Toronto, 1903, the following results were obtained: "Weight of air-dried peat charged into the dryer, 29,300 lbs., containing 34·21 per cent. water; weight of peat discharged from dryer, 23,000 lbs., containing 16·61 per cent. water. The weight of water evaporated was 6,300 lbs. Air-dried peat containing 34 per cent. water was used as fuel at the rate of 3,145 lbs. per day (of 10 hours)." From this data we can estimate the efficiency so: 3,145 lbs. peat, 34 per cent. moisture, consists of 2,096 lbs. dry peat and 1,048 lbs. moisture; adding to this latter the 6,300 lbs. of water evaporated from the peat we have a total of 7,348 lbs. of water evaporated by the combustion of 2,096 lbs. of dry peat. The efficiency, therefore, was $\frac{7,348 \times 1,100}{2,096 \times 8,644} = 45$ per cent. As we require only an efficiency of 36 per cent., we are evidently well

within the limits of practical efficiency. This is the more certain because the heat available was not in this experiment used to the best advantage, as the heat carried away in the escaping gases and evaporated moisture was allowed to escape to the atmosphere without further utilisation, which certainly would not be done if it were sought to use a fixed quantity of heat to the best advantage. Note also that peat was dried from 34 per cent. of moisture down to 16 per cent. moisture, within which range it is reasonable to suppose the efficiency would be less than from 70 per cent. to 45 per cent. Of power to get pressure assistance in the drying of the peat there is ample—76 I.H.P. per 100 lbs. dry peat, or, say, over 1,000 brake-horse-power hours per ton, 10 per cent. of which would certainly provide all the power necessary for the getting, conveying, pressing, and drying of the peat.

Two matters have not yet been dealt with: the heat necessary to raise steam for the producer and the heat necessary to evaporate the sulphate of ammonia. The first amounts to that necessary to convert about 25 lbs. of water at the temperature of cooling water of engine to steam (= 27,500 B.Th.U. about) per 100 lbs. of peat. For the second I have no data, but as the amount of sulphate of ammonia is only about 3·3 lbs. per 100 lbs. of peat it should not amount to much. It is, of course, a waste of heat to use the gas before being used in the engine to generate this heat, as apparently was done in the trial referred to in *Engineering*. My belief is that the difference between the 36 per cent. shown to be necessary with the compression of the peat to 70 per cent. moisture and the 45 per cent. shown to be attained in practice, is ample to cover contingencies; but if it were not, then drying by compression or exposure to 60 per cent. moisture, increased efficiency of heat utilisation, air drying under cover after pressing, and, finally, storage of air-dried peat harvested in good summers, put it, I submit, beyond the possibility of doubt that there is obtainable from 1 ton of dried peat, or 10 tons of peat, *in situ*, about 1,000 brake-horse-power hours as power, and that the fuel for this power will cost nothing, the cost of getting the peat and extracting the moisture being covered by the value of the sulphate of ammonia recovered.

One is, however, often met by the objection, What market is there for cheap power generated on the bogs of Ireland? I need not tell you that with one-seventh of its limited area under bogs, very little of Ireland is beyond the reach of economical electrical transmission of cheap power from these bogs; that Ireland is not such an industrial wilderness that there is no demand for cheap power and light in her towns and cities; nor, even if it were so, is Ireland so far from the industrial and distributing centres of Europe that cheap power, cheap sites, cheap labour, and cheap carriage (for her bogs are traversed by canals and navigable rivers) should not attract industries to which these are essential.

A further objection may be raised. If this can be done with peat, it can be done as cheap or cheaper with coal, and Ireland for cheap power production will be in no better position as compared with

England. This is not so for the following reasons: (1) peat is very much more easily got than coal, and is not dependent on specialised and highly organised labour for its mining; (2) peat has no exportable calorific value as compared with coal, and its value is, therefore, not regulated by an enormous demand for purposes other than power production; (3) peat has a very much higher percentage of fixable nitrogen per thermal unit than coal, so that the return in sulphate of ammonia for a given horse-power is greater. Peat has about 98 lbs. and coal 60 lbs. to 70 lbs. per 10,000 B.Th.U.

I have confined myself simply to the demonstration of the possibility of a power scheme from peat in which the fuel cost shall be at least covered by the by-product value. If this be possible, I need not point out to you the economic value to Ireland of her peat nor the industrial possibilities for Ireland involved in its exploitation.

NEWCASTLE LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN.

JAMES PIGG, Member.

(ABSTRACT.)

November 18, 1907.

Gentlemen,—Education is a subject which occupies a very prominent position at the present time, and one which we are said to be neglecting, and for our alleged neglect it is being prophesied we shall suffer severely in the near future. As all our meetings are more or less of an educational character, there can be no great incongruity in devoting a little attention to the subject.

There are some points in our system of education for the career of an engineer—say, at once, an electrical engineer—upon which something may be said. One takes it as granted that the object of his education is the turning out of a product as fully equipped for its purpose as is possible. That is to say, his teachers, besides grounding him in the principles of his work, should also be able to guide him in its applications. Speaking, perhaps, as one less wise, it would seem as if our teaching tended too much in one direction—that of the manufacturer's designing office—and that it also had a tendency to make the student too precise in little things to the exclusion of a better view of the actual relationship of his work to the end for which it is required. No one is more ready to admit the importance of design or the necessity for accuracy than myself. But, just as much as the buyer is the objective of manufacture, use is the objective of design; only in so far as precision brings an adequate return is it of service. Care in design has enabled the electrical engineer to produce machines of unrivalled efficiency, and precision has given him methods and the means of measurement beside which the data for measurement employed in other branches of engineering seem to him almost empirical. But these are means to an end; the fact that they are but the tools by which the product is more highly finished should not be lost sight of. When the designer has exhausted all his skill upon the product it has to be put into use for its object, and the chances are very great that it will not be used with the care or skill displayed by the designer. In economic science there is a law known as that of "diminishing returns," which in its ultimate application may be reduced to "Is it worth while?" Precision carried to excess becomes pedantic; an extra 0·1

per cent. efficiency may be obtained at the expense of something more valuable.

The different branches of electrical engineering are numerous, but for my purpose may be divided into two—manufacture and utilisation. When the manufacturer has produced something capable of being put to useful service, the utilising engineer steps forward. As a matter of fact he also is a designer of a somewhat different kind, and with, perhaps, a wider experience. The utilising designer necessarily combines many functions. He must be something of a specialist in the construction of each piece of apparatus that goes to make up his complete scheme, and must have a close knowledge of the latest practice and the best results obtained with each, so that he can specify the results required. He must also be something of a commercial man, and know how to combine the technical results offered with their commercial value. He must be clear-minded and broad-minded, or he will get into a groove, and find, perhaps, some day that he has unknowingly and unconsciously become a "loose peg," liable to fall out owing either to its own shrinking or to the widening of its environment.

All this is but the prelude to the statement that the engineer must develop the faculties of observation and imagination. The first enables him to collect and collate facts with regard to his business; the latter enables him to apply the results of his observation to useful and, it may be, novel ends.

Another essential qualification of the engineer is the power of organisation. Organisation can only be effectively carried out when a clear view of the end to be attained is combined with a thorough knowledge of the means available. Effective organisation implies a knowledge of the tools; the aptitude to judge a good tool from a bad one, or the ability to improve it. These tools are for the most part men. The engineer must therefore be a good judge of men, and know in what way they may be most effectively used for his purpose; how, in fact, they may each be fitted properly into the mosaic which it is his business to form.

There is probably no more important indication of the development of character than the ability to deal with men, and there is probably no point in which the individual is more left to his own devices than in this acquirement of knowledge of his fellows. Whatever the type of man, the engineer must be able readily to recognise it and act accordingly; and as he acts he will show whether he himself has the greatest of all the qualities necessary for a director of men—tact—or the ability to deal with the various types as to enhance their good qualities and to diminish or eradicate the bad.

An important item of the engineer's education, which seems hardly to obtain the attention its importance demands, is the cultivation of language, and the ordering of ideas. In certain stages of the engineer's career, it is his business to make or receive reports on various matters, and in other stages he may have to discuss and decide questions in

which slight variations of language may have important results. Conciseness and order of arrangement are of some importance, but from an exaggerated appreciation of a well-known aphorism, much more importance has been given to the former than it deserves. Conciseness, like other things, can be carried too far. Brevity, if it has to be supplemented by detailed additions or explanations, is not brief. It is not so much the question of literary style—so called—that is being referred to here, but, as already stated, the just appreciation of the interpretations that *may* arise under certain subsequent circumstances. Literary style, however, has an undoubtedly great value, even in the engineer's work. It is much easier and more pleasant to read anything that has been carefully compiled than the same thing written in a slipshod fashion. The chances are also very much in favour of the first being more readily understood, other things being equal.

There is yet another most important item of the engineer's education to allude to, and that refers to the fact that he must at the present time be a good deal of an accountant. It is the fate of electrical engineering to find the latest and most important fields of use already occupied, and in order to gain its footing it has necessarily to justify its use much more clearly than its rivals had to do at their inception. The advantages of steam over previous methods of obtaining power were so great that its engineering efficiency became a mere bagatelle, and remained unrecognised until a comparatively recent period. Similarly the advantages of gas over previous methods of lighting were so great that the efficiency of production, or the utilisation of by-products, was neglected until quite recent times. The competition of electricity in both these fields has stimulated the older methods to improvements, with the result that it is becoming more and more difficult to justify the newer methods, and the electrical engineer has necessarily to know something of the methods of arriving at the costs of production and utilisation of any system he desires to supplant in order to prepare statistics to justify his proposals. It is not sufficient to prove that electrical apparatus is sufficient or better for operating purposes for any special work. If the nett cost is to be more the business man will not—and rightly will not—look at it.

From the fact that the field, as has been stated, is already occupied, the consumer is faced with other comparisons. He has not only to compile comparative estimates of the costs of running in the two cases, but he has also to face the fact that he will not only have to spend a certain amount of capital on the new plant, upon which certain annual charges will have to be debited to the new system of working before an advantage can be shown, but he will also have to displace existing plant and dispose of it for, perhaps, a tithe of its book value, and on the difference between its realised and book values he may have to debit the new system with an annual charge. Under certain conditions such charges become prohibitive.

I do not propose to take you through the intricate maze connected with capital charges. Accountants themselves are by no means con-

sistent, and I am not an accountant. I merely wish to direct attention to some of the items.

If capital is expended in, say, electrical undertakings, it is usual to debit the undertaking with a charge for interest on that capital and a depreciation or sinking fund charges. I am not going into the question of what the latter should comprise or cover. Electrical engineers can read sufficient for themselves on that point, especially if the undertaking should be under municipal control. Whether the latter fund should cover ultimate replacement to the extent of its original cost, in addition to replacements of parts as becomes necessary, is outside the question at present, but it may be pointed out that electrical engineering is itself the most potent argument on the subject that can be adduced.

To undertakings, the capital of which is related in the ordinary way of joint companies, these charges are necessary, and the interest charge is the remuneration of those who have advanced the capital. With other industrial undertakings, in which the use of electrical energy is incidental to the prime object of their existence, the position seems different. The prime object of a railway company is the carriage of goods and passengers. Incidentally thereto it carries on many businesses, but they are subordinate to the principal object, and are only profitable in so far as they contribute to the efficiency with which the principal object is carried out. Other industrial concerns use electrical or other power for many purposes connected with their manufactures, and the inference is that such use of power is necessary for those manufactures. In such cases, it may be contended, that the advantage to the capital employed is in the user, that the use of that power is a profitable factor in the main industry to which it is applied. If these premises are admitted, the interest charge on the capital expended on subsidiary contributory appliances is obtained from the greater facilities for carrying out the prime object of the undertaking and the profit resulting therefrom. The imposition of the interest charge on the subsidiary industry has the effect of enhancing the apparent cost of that industry on the one hand, and, where the increased facilities are not employed to cheapen the prime object to the consumer, has the effect of enhancing the apparent profit resulting from the prime object.

To fix our ideas more closely, it may be remarked that the design of the universe is such that we cannot carry on our ordinary avocations continuously without the aid of artificial light. The production of artificial light enables other work to be continuously carried on, and in some cases it is resorted to for that purpose. As a result there should follow an increased profit on the total work done, and the increase is the result of the additional facilities provided by the use of the artificial light. In just the same way, as has been remarked in the case of capital used directly for the prime object, where the interest charge is the remuneration of the capital, it would seem that the interest charge on capital used for a subsidiary purpose, necessary for the prime object

of the undertaking, should be borne by the latter, rather than appear as an increased working charge on the subsidiary appliance, by the use of which the greater profit is obtained.

There is one other phase of the engineer as an accountant that may be briefly referred to. Whatever the results of his comparative estimates may be, the engineer must take into account with them the tendency of the times and the probable development that is likely to take place within the period which the work he is estimating for is likely to last. If practice in that particular branch of engineering is in a state of flux, if the tendency of the time is such as to promise a change of practice within a comparatively short period compared with the total period, his work is likely to last, and if such developments are likely to result in changes that will render his estimates out of date, however accurate they may be at the time when his work is contemplated, he must then judge whether it is not better to sacrifice a little at the first to reap a benefit later. Upon the truth of his judgment grave issues may hang in the future. We have in this neighbourhood an example of foresight on the part of the engineer that is probably unique. If we consider the changes that have occurred in the railway practice since the High Level Bridge was erected, if we consider that notwithstanding these numerous changes that bridge has been sufficient for something like sixty years, we have an instance of foresight of the most profitable kind to the proprietors. Many more instances of the same kind in progress to-day may be found. Who will be hardy enough to say that the prices for electrical energy now being charged by the small isolated electrical plants scattered about the country, and in some cases about a large manufacturer's works, are typical of the prices that will be obtained from undertakings having a wider scope, as the result of better judgment on the part of the promoters or of the prices that will be obtainable in, say, another ten years? We also have instances of this kind in this neighbourhood, and one looks with amazement at the apathy and something worse which is now preventing the first city in the kingdom from benefiting from similar undertakings.

I am afraid that these remarks have been much more lengthy, perhaps, than is desirable. That the subject is inexhaustible must be my excuse. I have endeavoured to put before you some of the characteristics that it is desirable the electrical engineer should possess, but which are not included in the ordinary curriculum. Purely technical work tends towards the stereotyped, when the experimental stage has been passed, and the results obtained by the investigator have crystallised into practical applications. Some one has said that we are the heirs of all the ages; heirs to the achievements of our forebears. We are not "heirs" in the sense that the term is ordinarily used, inasmuch as we cannot start where they left off. We must first achieve the same results; it may be in a less laborious manner, as the result of their labours, and we may be able to co-ordinate our efforts more towards a given end, but the impulse for the achievement must come from ourselves.

BIRMINGHAM LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

Professor GISBERT KAPP, Member.

November 20, 1907.

Gentlemen,—Let me first of all thank you for the honour you have conferred upon me by electing me as your local Chairman, an honour which I value all the more as this Section of our Institution comprises some of the most important centres of electrical engineering, namely, Birmingham, Stafford, Rugby, Coventry, and Wolverhampton. I think it will be difficult to find in the provinces another district so strongly charged with electrical talent and industry. This circumstance is of course highly gratifying to us all as members of the Birmingham Local Section, but to your new Chairman this feeling is to some extent marred by the consciousness that he personally is not a representative of the industrial side of the profession. I am neither financially, nor as a manufacturer, nor even as a designer connected with the trade of the district. My connection with the industry is indirect; my duty is to teach the rising generation to become useful members of the profession. If, then, you have elected a professor to the chair, I take it as a proof that you appreciate the part which science plays in the modern development of industry, and that you take an interest in technical education. It is for this reason that I invite you to look over the laboratories and that I ask your permission to make a few remarks on the question of technical education. Not in its general aspect. This question has been debated in the press, the committee-room, and on the platform for many years, and yet no agreement has been reached. The reason is obvious. The conditions of the various technical trades are so different that no general formula will suit them all. Even in one particular branch (say engineering manufacture) there is great diversity of opinion. Should the men go through the works before going to college or after? Should they go as workmen, apprentices, or premium pupils? Should a workshop course be part of the college teaching?—and so on. A committee of the German Institution of Engineers, after long deliberation, recommended the practical training at some works to precede the study at a technical high school. The Committee of our Engineering Societies recommend one year's

"introductory workshop course" before college, and two or three years' practical work after. This difference of opinion may be due to the fact that the intermediate education in Germany takes longer than here, so that the men would be too old if they deferred their shop training until they have completely finished their studies. The English Committee do not favour workshop courses. We in this university think otherwise. I merely mention these few points to show that it would be a hopeless task if I were to attempt to treat the subject from a general point of view. All I can attempt to discuss is what is the best method for this particular district (by which I mean the district of our Local Section) and this university.

The inspection of the laboratories, which I hope you will make, will show you how far we have up to the present been able to go in a practical way to meet the requirements of the industry for men, and how, on the other hand, the district has rendered us valuable assistance. I do not propose to give a description of our arrangements, since you will find that already in print in the "Electrical Handbook" which was issued by the Institution in 1906. Your inspection will merely be a kind of supplement to this description. The machinery is at work and various tests have been set up, so that you may see in what way the work is done.

I have already said that the district, by which I mean the leading firms of the district, has given us generous help. No finance committee, be it ever so liberal, could afford continually to buy new machinery and apparatus as it is developed and put on the market. Yet it is important that students should become acquainted with the latest type of plant from all leading manufacturers as it comes on the market, for the student of the present time must in the natural order of things become either the designer, the user, or the buyer of plant at some future time. It would be a disqualification for him, and unfair to most manufacturers, if his practical experience were restricted to a few samples of plant (which will then be old) made by a few manufacturers. To meet this difficulty Mr. Chamberlain has suggested that the manufacturers in the district should be asked to lend machines and other apparatus of their latest type from time to time. This suggestion has met with a generous response, and I am glad to take this opportunity of thanking those firms who have lent us plant for their assistance. Some firms have not only lent, but also presented plant, and I am glad to say that among the donors are not only firms in the district, but also outside the district and outside of England. Loans are generally for an indefinite time, but on the understanding that if, on account of a sale or for some other reason—for instance, exchange for a more recent type—the firm wishes to have the article back, it is immediately returned. When you go through the electrical department you will see that we have ample facility as regards switchboards, test-beds, measuring and regulating appliances, to put into work in a proper manner any machine or apparatus of moderate power, so that loans are properly treated, and when returned are in as good a con-

dition as when they reach us. In this manner the firms are working hand in hand with us, and are materially furthering the cause of technical education. I wish, however, to enlist their help in yet another direction, and this brings me to the main point of my address, namely, a suggestion for their assistance in the training of our men in a still more direct way. My suggestion is not new, but I make it in this place for two reasons. First, because I wish to have the benefit of your criticism on it, and secondly, because I hope that it and your criticism in the discussion may reach the leaders of industry in the district and receive their consideration.

My suggestion, briefly stated, is that firms should allow students to work in the shop during vacation time, no premium being required nor pay given. The privilege would, of course, be restricted by each firm not only to a certain number, but also to those students who are recommended for it by their professors, or by a committee of their professors.

There is nothing new in this suggestion ; it is in fact well known under the term "sandwich system," and is in use in various institutions both in Scotland and in England. It has also been in use, to a limited extent, in this university, inasmuch as I have been able to persuade a few of my manufacturing friends to take some of my best students on for three months during the summer vacation, but I should like to see the system so far appreciated by the manufacturers that it shall not be a matter on which they require to be persuaded, but a matter into which they enter heartily of their own free will.

The most formidable obstacle in the way of the more general introduction of the "sandwich system" is the "premium pupil system," brought down to us from the good old times when technical instruction could not be acquired in any other way. If carried through in the original spirit it would even now be a very efficient method of acquiring scientific knowledge and technical skill. The original spirit I define as that which prompts the master to teach his pupil personally. But such a personal intercourse between master and pupil is only possible where the master is personally attending to the details of his business ; it is only possible in a very small office or works, and it is certainly quite impossible in the big engineering works of our present time. The master, in this case the manager of the works or the head of the particular department where the premium pupil happens to be at the time, has other things to do than teaching science and handicraft ; he has so to conduct the works or the department that the shareholders may get a dividend. Thus the instruction of the pupil must be left to the foremen and workmen. This instruction cannot be of a scientific character, but it may nevertheless be very valuable so far as it relates to what may be called the mere handicraft of the business. The only drawback is that the pupil gets very little of it under ordinary circumstances. Neither the foremen nor the workmen have under modern conditions of hard driving and piece work time to give instruction to pupils, and what they learn they

have to pick up as best they can by exerting their own powers of observation.

Thus it comes that the premium pupil system must on the whole fail to produce really efficient engineers, and it is this failure of an old system which has led to the establishment of technical universities and colleges. Our Continental rivals have recognised this failure sooner than we, and hence they got the start of us in the establishment of technical high schools, but we are now doing our best to catch them up, and in one particular to surpass them. This is in the introduction of workshop courses into the curriculum of scientific instruction.

On this question of workshop courses opinions are divided. I personally do not deny the utility of such courses, and I think it was right that the authorities, after due investigation of what was done in this direction in American colleges, have adopted them, but such courses alone are not sufficient to supply the practical side of technical education. A student may learn in a general way how patterns are made and moulded, how work is lined out and tooled and fitted, but the knowledge thus acquired is only part of what he needs in order to become an efficient member of the staff in a large engineering works. I do not mean to say that for this purpose it is necessary that he should be a perfect patternmaker or moulder or turner or fitter; indeed, it would not be possible for a man to become efficient in all these trades, even if he spent his whole life at them. What I mean is, that in addition to the general idea of the handicraft which the student can get by attending the workshop courses at the university, he requires some experience in shop routine, that is, in the way in which engineering work is done commercially. Such experience the university cannot give him; he can only get it in a works producing machinery in the way of business. The student must learn to appreciate the value of organisation, he must observe how work goes through the shop, how experienced workmen tackle the various jobs, what may and what may not be expected from any particular grade of workman, and many other things, some of them peculiar to the work carried on in any particular shop. All this the student can only get to know by working alongside of the men and as one of them.

The question then is, How can this opportunity of learning shop routine be best given to the student? Some firms say, Let him graduate and then come as a premium pupil on somewhat better terms than are offered to a boy from school. By better terms they mean not a smaller premium per annum, but a reduction in the pupilage period, say two years instead of three. The general objections against the premium pupil system apply also in this case, though in one particular not with quite as much force as in the case of a boy coming straight from school. The university man has already had his science teaching, and the failure of the master to give him such instruction does not count so heavily; the other objection, that the pupil, having

paid a heavy premium, considers himself as a sort of privileged person who need not take the work seriously, counts for more, as the man is older than the pupil coming straight from school. But most of all there is the objection on the ground of loss of time and expense. When a man finds that after spending four years in the university he is expected to pay a heavy premium and spend two years more as a pupil at some works, he will consider that he is worse off than the boy from school who goes as a pupil for three or at the most four years. This must act as a deterrent to scientific training, and if there were nothing but the premium pupil system as a completion for the university training, we should very soon see these establishments, which have been started at great cost, denuded of all but the most wealthy and not very energetic scholars, and we should be back to the condition of finding the best posts in our big firms occupied by foreigners and only the subordinate positions open to Englishmen. Such a state of things is advantageous neither for the rising generation nor for the firms. The moral of all this is that the premium pupil system, in whatever form it may be applied, does not fit our modern conditions of manufacture and is bound to go. Luckily we have something better to put in its place, namely, the sandwich system. By alternating scientific and practical tuition each will be more effective than where they are separated by a long period. If a graduate has to work for two years in the shop after he has completely finished his studies he is in danger of forgetting much of his science. The idea of keeping it up by private reading or evening classes is out of the question, for no man can do mental work after a hard day's toil in the shop. Either one or the other must suffer, probably both. If on the other hand the student can devote himself entirely during three or four months in the summer to shop work and need not open a book during all this time, he will certainly do his shop work well, and he will come back to his studies mentally refreshed. Having seen something of practical work, he will be in a better position for profiting by scientific instruction.

If students were permitted to work during the summer vacation for three years in engineering shops they would benefit more than if they worked as premium pupils for two years straight on. Remember that our students take workshop courses, and are therefore more or less familiar with what I have called the mere handicraft side of the trade. They will be more useful to the manufacturer than the ordinary apprentice, who has to be taught the very rudiments of the craft.

The suggestion which I put before the leaders of the electrical industry in our district is that they should assist us in our task of educating efficient engineers by giving facilities for work in the vacation; these facilities to be offered to students who have during the session's work shown that they would profit by them and are, in fact, worthy of the privilege. This point might be dealt with by a committee of professors. As a general rule each student would year

after year go back to the same firm and thus go through various departments. There might, perhaps, be a finishing term of six months after the fourth year, where the man would get a nominal salary, but these are details which would have to be arranged according to the particular circumstances in each case. Here I am only concerned with the general principle. The advantage of the sandwich system to the student is obvious, but I venture to think that it also has advantages for the firm. First and foremost, the advantage which the industry as a whole must derive from the better practical training of the rising generation. Then the advantage to each particular firm. The firm will have an opportunity of watching the men and seeing how they shape. If they are not satisfied with any particular man, they can send him down, but if they are satisfied they will take him during subsequent vacations and gradually become acquainted with what he can do. Thus, if the firm should at any future time require to replenish their staff, they will not have to take a stranger by advertisement, but they will have the pick of the best men out of a group they have themselves helped to train.

Finally there is the advantage to the university itself. In all such establishments there is a natural tendency of laying more stress on the purely scientific as distinguished from the purely practical side of the work, so that in course of time, if no corrective influence from outside be applied, the latter must suffer. In the sandwich system we have such a corrective influence; the students themselves supply it.

As I have already mentioned, and as is probably well known to most of my audience, the system I suggest as particularly applicable to so important a centre of electrical industry as the Midlands is no new thing. It has been introduced in other districts with success. Thus King's College, London, has worked on these lines for some years. In the syllabus of this institution we read: "In order to carry out this arrangement a number of places have been put at the disposal of the college by well-known engineering firms." The Northampton Polytechnic Institute has also adopted the sandwich system, and its Principal, Dr. Walmsley, writes me as follows: "I enter each year into correspondence with leading manufacturers for the purpose of inducing them to take our second and third year students into their works. No premium is offered, but on the other hand no wages are asked for, but as a matter of fact very few works care to take the students without putting them on the wages list, even though the amount may be small. The students conform entirely to works' regulations, and it is my particular request in every case that they be not treated as 'gentlemen apprentices.' On the other hand, it is an essential detail of the system that I carefully weed out from the classes at the end of the first and of the second year students who do not show promise of engineering ability or who are undesirable in other respects. In this way I endeavour to insure that the men sent to the works are well worth having, and so far I think I may claim that I have

been successful in this respect, as evidenced by the fact that the manufacturers who have once had our students desire to take them again." There are other places in the kingdom where the system is at work. In America it is fairly general, and it has also been introduced in our colonies, notably in New Zealand, with beneficial results. I do not wish to weary you by multiplying examples, and the object of my address will have been attained if I have succeeded in drawing your attention to a matter which, if properly taken in hand, will benefit not only our students, but the whole industry of our district.

GLASGOW LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN.

APPRENTICESHIP AND EDUCATION.

Professor FRANCIS G. BAILY, Member.

(*November* 10, 1907.)

The subject of education has occupied in one or other of its aspects the minds of every class and almost every individual during a good many years, and the particular branch called technical education has probably received the largest share of attention. The methods of Germany, the procedure in America, the old school of British engineering, all have their adherents and their critics. Scientific training, technological instruction, workshop practice, each has had its value upheld, until now some concensus of opinion has been attained, and differences, where they still exist, are confined chiefly to details of time and place, or to apportionment of importance of the different parts.

But all this battle has been confined to the education of a particular class—the class of masters, managers, and professional men. That one of the subjects in debate is the value of a college training connotes that the individual is in a position to attend a college course. He will not earn his daily bread by the skill of his hands. In short, we have not been discussing the artisan at all. Only a stray voice here and there has called attention to the wider range of the question, or at times its importance has been pointed out during a discussion of an altogether different question, such as the problem of the unemployed and the condition of the unskilled labourer.

Though the matter is at present largely ignored, no one will deny that the vast body of men included under the term "skilled labour" form one of the chief factors in the success of an industry. The question of the training of the rank and file is as vital as the training of the officer, and it demands its own individual solution. The same training, even if it were possible, is not necessary or desirable for both. One works with the hand, the other with the head, and these must receive respectively the bulk of the training; but just as we demand a practical training for the manager, lest he become a doctrinaire and a paper

strategist, so also must we develop the understanding of the workman, lest he turn into a mere machine.

The problem is not confined to the electrical industry, and indeed the whole engineering industry occupies but a small part. There is no trade or manufacture that is not concerned. But probably there are few where the importance of the matter is more obvious than in electrical engineering, in that the agent, with which the craftsman deals, possesses properties less obvious than those met with in other trades. Hence the ordinary teachings of mother wit and the common senses are less valid, and a technical instruction becomes necessary. No man, for instance, of ordinary sense would use a thin piece of wood as a blank flange on a high-pressure steam pipe, or plug a hole in a boiler with a cork. But plenty will employ a wisp of paper to insulate a wire, or after carefully supporting a live copper rod on porcelain, will let it rest on a piece of wood or plaster wall. There are, unfortunately, innumerable opportunities for disregarding the properties of an electric current, all of which mean an ultimate expenditure of time and money and loss of reputation.

That every one would like his workmen to be more intelligent, more appreciative of their work, and more alive to its requirements, may be taken for granted, but trained intelligence will scarcely come of itself. Let us examine the process of the usual education of the skilled artisan, and consider in what degree it is organised to the required end.

The ordinary board school education may now be taken for granted as substantially universal, and forms the lower limit. In most cases it will also form the upper limit, since the board schools have replaced in most towns the lower grade private schools. Though the attainments of the boys naturally vary with their intelligences, the bulk of the scholars do not differ very much, and a moderate elementary education may be safely assumed at the leaving age of thirteen or fourteen. The next definite step is the apprenticeship, and in a well-organised scheme of things it would be imagined that the compulsory regularity of the school should be succeeded by a similar obligatory occupation. This is, however, not the case. The apprenticeship age has risen, and is possibly still rising, so that there is a hiatus of one or even two years in the development of the educational scheme, to be filled in by occupations which may not be even remotely connected with the ultimate aim of the boy. There is an optional path through the higher grade schools, where arithmetic, mathematics, the elements of science, and other useful subjects are taught, and time spent in these studies before apprenticeship could hardly be better employed. But a comparison of the numbers in the higher-grade schools, and of those leaving the board schools, shows how few adopt this line. The great majority go to be errand boys, handy boys, helpers, all reputable occupations in their way, but from their very nature desultory and off the line of development. The boy leaves his place at the best with a scrappy knowledge of something quite useless to him, with much of his school

work forgotten, while too often he has contracted idle irregular habits, by being free from close supervision just at the age when external control becomes more difficult and before self-control can be expected. It is true that night-school classes give him some opportunities for continuing his education, but a small boy cannot be expected to work very hard at the end of a long day, and in any case his attendance there is not obligatory. In short, his education may stop abruptly when he leaves the board school, and it can scarcely be doubted that in a large proportion of cases this is what happens.

Here in Scotland the school training continues to the age of fourteen, whatever the ability of the boy, and the more able among them have the advantage of the more liberal and varied range of subjects taught in the supplementary classes, leading on to the higher-grade schools. In the English school system the evil is greater, since, as I understand, a boy can leave even at the age of twelve, if he has reached the sixth standard. Whatever economic advantages this rule may have, educationally it can scarcely be commended, for by its action the discovery that a boy has brains is regarded as a reason for excusing him from further education, and for turning him out into a world which has no proper and suitable accommodation for him at that early age. At fifteen, or even sixteen, the boy enters an apprenticeship, and though this is not the binding and solemn agreement of former days, still there is an obvious and clear line before him, so far as the shop work goes. Here he will learn the routine of handicraft, and will gradually attain manual skill. In other words, he will become something of an efficient machine. More than that he can scarcely get in the shops. He learns his trade from other workmen, who possess none of the main attributes of a teacher—desire to teach, ability to teach, and thorough knowledge of their subject. They also are in large measure machines, and are gradually becoming more so, as work and workmen tend to increased specialisation.

That this is not adequate has been felt for many years, but our only remedy, our sole suggestion for a real education, is a course of study in evening classes.

Evening classes are held up as the road to industrial success and as the proper course for a good apprentice who wishes to succeed. Technical schools are equipped in every town to supply the recommended courses of study, and on all speech-days and public gatherings the opportunities that are now offered for self-improvement are the subject of envious comment from comfortable gentlemen, who are well removed from their sphere of application. But after an experience of many years as a teacher and one year as a pupil, the most that I can say for them is that they are certainly better than nothing. They have certain fairly well-defined limits of usefulness, which are independent of the perfection of equipment and ability of the staff of the school—limits which are drawn by the human capacity of the evening student, limits which are drawn at a much more humble line than is usually supposed.

They are not a satisfactory education for lads of any class, high or low. We all of us publish in our calendars a scheme of work by which a boy can in his five years' apprenticeship obtain by unremitting effort what I must call only an imperfect imitation of an education ; yet even with this meagre goal I defy any boy to have the energy, the patience, the endurance, and the constitution to work through it. They do not. They begin at the beginning and stop half-way up, omitting classes here and there, or dropping classes half-way through the session, out of sheer inability to stand the strain. Finally they withdraw even from their special subject, for their imperfect general education and poor knowledge of cognate subjects blocks any further advance. It is true that the more advanced classes show a good attendance and a good quality of work, but an examination of the students reveals that they are, with scarcely any exceptions, lads or men of a good education, derived certainly from a secondary school, and frequently from college courses. These students are supplementing a good education, not building on a poor one, and though such classes are obviously of great advantage and utility, they are not for the class of student which we are considering.

Is it reasonable to expect any other result? For any adequate course, a lad who is serving his apprenticeship and spending the whole day in the shops, must attend classes for at least two hours in two or three evenings out of a week. If he further spends two nights in going over his notes, working problems, and reading, an allowance which is certainly not excessive, we find that from Monday morning to Saturday afternoon he is continuously at work, with some intervals for sleeping, eating, and travelling, through the autumn, winter, and spring. Such a strain is not only morally improbable, but is physically undesirable for a growing lad, and I have observed again and again the positive exhaustion of the more strenuous students by the end of the session, and with the less strenuous or those more careful of health, a cessation of attendance before the end of the session.

But even this heroic endeavour to learn their trade thoroughly does not meet with fair treatment. We strictly forbid parents to interfere with the proper course of school training, but permit a ruthless destruction of subsequent studies. With the most intense desire for conscientious and regular study, many find their work broken and spoilt for a session by compulsion to work overtime in their shops, or to take part in work at a distance.

Not being an employer of labour on a large scale I may, perhaps, give too little sympathy to the point of view of the employer, but as an outsider regarding an apprenticeship as primarily a period of education, it seems to me that the working of apprentices overtime is indefensible, and is a not unfit subject for legislative interdict. It may be argued in justification that overtime is optional ; but taking shop customs, foremen, and apprentices as they are, it is an option very much akin to the historic choice offered by Hobson.

But at the best there cannot be expected much from evening classes

during the apprenticeship. The lads are exhausted before they come by a long day's work, and though they are in many cases keen and diligent to a degree which I admire profoundly, they are attempting to learn in remnants of the day what their more favoured companions devote their whole time to. Our own sons are given time—time to learn and study, time to think, time to relax and recover—all through the years of youth. Surely some small modicum of time can be spared for the others, some little portion of the good daylight hours! When we have squeezed a long day's work out of them, when eye and hand and brain are exhausted, we tell them to go home and study mathematics, mechanics, the theory of steam, valve gears, and alternating currents!

I submit that we have gradually come to ignore the primary reason for apprenticeship, and while preserving the form and letter, to refuse recognition of its true spirit. The apprenticeship is to the craftsman what the college course is to the professional man. It is his education for his life's work. The school education fits him to be a member of society suitable to his station, but it makes no claim to afford a mode of livelihood, except of the poorest description. It is during his apprenticeship, if ever, that a lad learns his work, and if that learning is in any degree to be called an education, it must include more than a mere manual dexterity and a routine procedure of operations. Education involves teaching; that is, teachers and opportunities. Teachers there are, but the time offered is no more than a poor remnant of an already well-filled day, a time filched too often from needful rest, and always from well-earned relaxation and leisure.

From fourteen to twenty is the important educational age. Up to fourteen the boy is learning rudiments, and learning how to learn. He is preparing his brain for the work of learning. After twenty he has to set to and apply his knowledge. His leisure time then becomes rapidly filled up by small and varied calls, so that the steady application necessary for laying foundations is increasingly difficult to maintain. If we are to have intelligent educated workmen, something higher than human machines tending material machinery, there must be a space allotted during apprenticeship for the requisite scientific and technological training. We do not want a wearied product full of undigested information with tired and inelastic brain. We hope to turn out "a young man rejoicing in his strength"; but for this result, to quote somewhat inaccurately from the same Source, "as his strength, so must his day be."

Such a demand is not, however, anything very alarming when it is examined. The standard of education for the mass of artisans has a modest limit, beyond which its utility becomes doubtful. Moreover, if the school elements have been mastered previously, the time required for learning the elements and principles of the technical sciences in question is not great. These may be mastered more quickly than the multitudinous details of practice, and the brain, if properly treated, learns more rapidly than the hand. But a proper treatment is essential

for the brain, for an improper treatment is practically useless, and may even be deleterious.

It will be well at this point to suggest some course of procedure as a basis of thought, though the magnitude of the subject will naturally prevent any single scheme from being sufficient and adequate for all conditions. Starting with the board school, it seems most desirable that the minimum age limit should be the same for all. If this is taken at fourteen, then it is further desirable that the boy should not be allowed to drift even for a short time. The apprenticeship age should be definitely fixed. It is obviously to the advantage of the masters to fix this as high as possible, but this matter lies above their immediate and temporary gain. Probably the best course would be a year in a continuation or higher grade school for mathematics of an elementary kind, the elements of chemistry, physics, and mechanics, drawing, and the beginnings of handicraft. Apprenticeship would follow at fifteen. A cheaper and inferior course would substitute a night-school for the day-school, leaving the day for an occupation; but this is so much inferior, for reasons already fully given, that I am unwilling to mention it as a possibility. Needless to say, whatever school attendance is laid down should be compulsory, for at fourteen years of age optional mental work has little chance of general fulfilment.

Another possibility would be the commencement of apprenticeship at fourteen, but, in view of a sacrifice to be asked from the employer during the apprenticeship, the ratepayer and the parent may be called upon for their share, and there can be little doubt that the preliminary year of pre-technical training would benefit the boy.

To pass to the apprenticeship period, it is clear that under modern conditions any teaching that is given must take place in a technical school or some similar institution. At all events, proper teachers must be employed who not only understand their subject but also know how to teach it. The time to be devoted to mental work, and the period at which such time should be given, require more careful discussion. Assuming the preliminary year in the continuation classes, I propose as a reasonable amount of time a complete course of day classes for some six or eight months, or the like time spread intermittently over a period of years. Thus the time might be made up by a course of one day a week, or two half-days a week spread over three years. In some trades the latter would be the more suitable, whereas in others, especially in machine shops, intermittent work would be very inconvenient, and a complete absence for a space of time would cause the least disturbance to the shop work.

The period at which this training should come may be varied within limits. If set too early in the apprenticeship, the lad would scarcely have grasped the general ideas of the work with which he is occupied. Hence much of the teaching would be absorbed in matter which he would learn better in the shops, and he would often fail to observe the object of the instruction, while the young brain would also be less capable of understanding. But if delayed too long he would be in

danger of forgetting much of his school training, which would involve waste of time in refreshing his memory concerning elementary parts. Moreover, as it may reasonably be supposed that his work in the shops will be improved by his training in principles and theory, it is only fair to allow the employer a good share of apprenticeship time after the course is concluded. Probably the latter half of the second year, or the first half of the third year, would satisfy all the conditions most fully. And as even a single year may cause an astonishing obliteration of school work from a boy's memory, during the intervening time a modest amount of evening classes, even a single evening a week, could be required of each one, to afford him some intellectual exercise and to keep him in the habits of study.

The other alternative is a daily sandwich system. By appropriating two afternoons in each week through the first three or four years of apprenticeship, an equivalent to the single collected course could be obtained, while with no evenings occupied in classes a sufficient expenditure of time could be properly enforced for home work and digestion of instruction. Other things being disregarded, I believe that this method has many advantages to the boy. His studies can advance with his growing powers, his mental interests are kept awake continuously, and the slower assimilation of knowledge allows of its more thorough appreciation, for it is very easy to choke a young mind. Moreover, as the class work would be a change from the shops, the one would serve to some extent as a relief to the other, and we should get more work done than under the first scheme, where the whole day is devoted to class work, and relaxation can only take the form of play. Still, there is much to be said for both methods, and the conditions of the particular trade would necessarily require full consideration. These schemes are, in any case, only put forward as illustrations, and not as the only solutions.

It must be borne in mind that such a scheme of education is not ambitious or superfluously extended. The courses could succeed only in giving a workman an intelligent knowledge of the objects of his work, an appreciation of the vital points, and the reasons for the processes or operations involved. He would also be in a position to extend his knowledge by subsequent reading of technical books and periodicals, if he was unusually gifted and interested. But the education would only be, and could only be, appropriate to an artisan of the skilled class, though doubtless some would use it as a stepping-stone to higher things.

The interests at stake are of such magnitude as to justify some disregard for a possible reduction in the profitableness of an apprentice. Indeed, there is little justification for undue profitableness. The employers are in the position of teachers, training lads for subsequent employment as skilled workmen, and to regard the question primarily from the immediate profit-earning point of view approaches to a breach of moral responsibility. But in some such procedure as is indicated above, the community and the apprentice already pay a

heavy share in money and time, with the result that the new apprentice will clearly be of considerably more value, even during his apprenticeship, than he is now. I am only demanding that this "unearned increment" to the employers shall be given back to the apprentice, and still some increment will directly accrue to the employer during the latter half of the time, while the general improvement of the full-fledged workman will conduce to the employers' gain. The amount of time, money, and material wasted by the incompetence, the stupidity, and the ignorance of workmen is too notorious to require illustration; while the loss of reputation of a firm due to mistakes of its workmen, the expense of rectifying such mistakes, and the maintenance of numerous examiners, foremen, testers, and similar officials, all engaged in looking out for workmen's blunders, amount to so large a cost that even a considerable sacrifice in the apprentice stage would be ultimately a wise economy.

It will no doubt occur to many that these facilities could only be enjoyed by lads whose work or home was not too far removed from the place of instruction, and there are doubtless conditions in which such an arrangement could not be made. But there is no need to hold classes of this character exclusively in an elaborately equipped college. The numerous technical schools, which are being founded in the smaller towns, would have ample equipment for the work, for we are not aiming at advanced education and high scientific attainments. In fact, these schools are for the most part empty in the day-time, and could with ease find accommodation for large classes, more easily indeed than could the elaborately equipped colleges aforesaid. No doubt certain of the students of marked capacity would find their way ultimately to the technical college by means of bursaries and scholarships. Though not devised for picking out lads worthy of better things, such a course affords excellent opportunities for helping on the lowly born genius.

Within the last three or four years there have been attempts made in several towns to deal with this subject. The schemes proposed differ considerably in detail, and a close examination shows that in few instances is the work intended as a training for the prospective artisan, but is directed largely towards picking out lads for further advancement. This, however worthy an object, is not the main question, and should properly be an adjunct rather than the principal aim. Inquiry among the teaching establishments of the larger towns in the country has been made, and the results may be briefly epitomised.

In Birmingham apprentices attend usually two afternoons a week, and evening classes are optional. The arrangements seem tentative as to the length of the course. There are about fifty students.

In Bradford certain apprentices are picked out by employers and are given a five-year course, consisting of one afternoon and two evenings a week. There are some forty or fifty students.

In Coventry they have one morning and one afternoon a week for three years. There are some eighty students.

At Derby the Midland Railway allow pupils to attend two mornings a week, but this is not quite artisan work.

In London the Battersea Technical College gives classes in the morning for a three-year course, and most students take evening classes as well. These are well attended.

In Manchester a two-year course is held with one complete day of eight hours each week. They have about sixty students, and the numbers are not increasing.

In Middlesbrough the class is held one afternoon a week for a two-year course, a preliminary evening class course being taken.

At Rochdale two afternoons a week are given to apprentice classes.

At Sheffield apprentice classes were tried, but it is now found better to admit apprentices to parts of the ordinary day courses.

At Swindon the Great Western Railway allow special apprentices to attend classes for two afternoons a week for a three-year course, but this is a little above artisan work.

At Woolwich a course of part day and part evening class work is arranged for apprentices.

The above list is fairly complete of all towns in which apprentice training is attempted, and in some of those it is more a training of special apprentices than a general application. In other towns, *e.g.*, Darlington, London, Glasgow, Sunderland, Edinburgh, and others, there are many instances of particular apprentices being allowed off for whole winter courses, but this is rather a different class of education. In a few other places there has been no success with these apprentice classes, partly from difficulty about agreement with the masters, who have declined to agree independently, and among whom combined agreement has failed. Mention may also be made of an arrangement in Edinburgh for the house-painting trade, which has just been organised. A special class for trade apprentices is held twice weekly in the Heriot-Watt College, to which all apprentices are permitted to come, spending two whole days a week, and receiving full pay for the time thus spent.

Thus, although some small attempts are being made in several places, the movement can scarcely be regarded even as having made a promising and healthy start. The replies on the whole do not indicate very much satisfaction with the work done, but there are two possible reasons for this disappointing result. First, the previous training of these lads has not been organised, so that they are imperfectly prepared, and much of their previous knowledge has probably become rusty. Secondly, there appears to be insufficient recognition that these lads are going to be artisans, and are not on a par with the more pushing evening-class student who has his eye on advancement, and does not intend to become a journeyman. That it has been found preferable, and even possible, to draft the students into the ordinary day classes is a proof that the style of education aimed at in these places differs greatly from that which we have been discussing.

We must therefore acknowledge that of the definite training of the ordinary apprentice, who is to be an ordinary workman, there is as yet little sign. He is brought up to the stage on which education may be based, the foundations are laid down, but the superstructure—the useful building—is left to chance. He may force his own way if he has unusual energy and ability, but the average lad has not unusual ability, and rarely has the energy to continue his studies seriously after a long day's work.

Nevertheless, it is the average lad, the lad in the bulk, that we ought to get hold of and improve. We ought to prevent the submergence of those foundations in the intellectual idleness which is too apt to follow release from school, until not only the wish, but also the power, to pick up again the threads of study have been lost.

This is not a matter for tentative and puny individual enterprise, though that may begin the movement. Between the natural preference of the boy for play rather than work, and his sociological insignificance on the one hand, and on the other the disinclination of the master to part with a small personal gain for the sake of a larger advantage—partly reaped, possibly, by some one else—there is small chance of development.

Federations of employers may succeed, if they can control a large enough area, but if these refuse to recognise the claims of the apprentice there are yet two other forces, both within the bounds of possibility. The Government has but to lengthen the educational course—which has gradually grown from the barest elements to the present very fair training of the higher grade schools—has but to keep its compulsory hand over the lad for a little longer. It is already following the boy near to the very portals of apprenticeship, and leaves him probably with reluctance. Another move, and the sacred precincts will be invaded. If continuation classes are found to be good, they may be enforced with a due regard for the pupil's welfare and health; or in other words, they may be made compulsory, or almost compulsory, as a day course. A universal compulsory higher grade course might possibly be wasteful, but as a preliminary to entering the skilled trades it might well be obligatory. And from that to the inception of regulations concerning apprenticeship is not a large step. It would be no more, indeed, than a natural desire to ensure that the money spent in the lad's education should produce its proper fruit.

Another powerful and interested body are the trades unions. Let them realise that their sons are being unfairly worked, and that the heavy school rates have been spent in a result which is thrown away as soon as acquired, and they may raise a demand that the educational building shall be completed. Collectively, no doubt, a trades union keeps a sharp eye on the admission of apprentices, and the older men look with suspicion on a better training for young ones, for obvious personal reasons; but the members individually are fathers of sons whose welfare is dear to them. They may demand that facilities for instruction shall be included in the indentures, and their

demand will be difficult to combat. An apprenticeship is a technical education. That is its primary object, whatever other ends it may also serve, and if an obvious and great improvement can be indicated in this education, it will be well-nigh impossible to refuse its fulfilment.

It is, no doubt, a large question that we are now considering, and it will not come in a day. As an official arrangement there can be no temporising with a few picked scholars, arbitrarily chosen, and enjoying their privilege with an uncertain tenure. It means technical education for all skilled labour, and the cost, the staff, the buildings and equipment will be on a colossal scale. But whether reckoning as individuals or as a nation, no money is dearer than that earned in boyhood at the expense of education, while no money is better invested than that devoted to training the young mind. A few pounds spent on a lad's education means that his wage is higher, and equally that his work is better. In a year or two he has won back all the sunk capital, with his equipment not only undepreciated, but increasing in a way impossible to the present ignorant craftsman. He may not actually pay back a definite sum to the people who have borne the cost. That is immaterial to the argument. The money spent is earning enormous dividends, and the community is the richer.

There may be a doubt among employers of labour whether this picture is not too rose-coloured, and they will recall proverbs about a sow's ear, or a horse and a well. The average apprentice in their experience does not burn for education; but has he ever had a fair chance of it? That evening classes are held at all proves that a good number show unusual eagerness and capacity for sacrifice. Remove the need of sacrifice and many more will be anxious to improve. Even if there are some wasters, shall we refuse to sow the corn because some of the grains will not germinate? And do we sow it in stony ground in order to point to the success of the few exceptional specimens that have managed to grow at all?

But however beneficial the improved training of a few special lads may be to the firm, some may object that a universal training, if it is to benefit the workman, must result in higher wages, and they may regard this as a loss, or at all events no great gain to the masters. One might reply brutally that if the trades unions saw their way to increased wages, they would not stay their hand out of consideration for the masters. But this is a needless fear. If there is one thing about the skilled artisan which differentiates him as a class from the black-coated workers, it is his contentment with small pecuniary advantage. One man earns 30s., another 36s., at the same job, and the 20 per cent. difference in wages is often no representation at all of the difference between the two men. In black coats the second man would demand and get two or three times the first one's salary. The master gets the lion's share of the advantages of his workman's ability, and I believe under the new conditions he would do so even more signally. If all are educated it is indeed the master who would gain more certainly than the workman. His profit comes from the outside world—from

enlarged trade and wider markets. His rivals are ultimately in other countries, and he wins or loses against them according to his own power of ideas and his workmen's capacity for carrying them out.

Competent workmen are the mainstay, the *sine quâ non* of an industry. How much time and money is spent in overseeing the men, in examining and checking their work? How much time is wasted by a dull-brained workman in adjusting himself to a small alteration in procedure? With what painful caution does he approach a problem of the slightest novelty! Other nations are recognising this, and on the Continent are springing up trade schools for teaching the apprentice the principles of his craft, and while we are justly proud of our artisan's skill and general honesty of work, we cannot afford to rest in the assurance of supremacy without effort. Elsewhere the cry is to increase the productiveness of the worker, from highest to lowest, by education. But with ourselves the one piece in the vast machine of manufacture that we utterly neglect to render efficient is the human. We pour out money on improvements in machines, we spend capital that running costs may be reduced, while the vast running cost of human labour streams away every week without a thought to its reduction except the thought, "Can I make a machine do the work instead?" We pay a man to look after the machine because it cannot think for itself, and we also pay men to look after the human machines because they have not been taught to think. We are not Frankensteins to create thinking organisms, but we can at all events improve those that we have to the best of their powers. We do not grudge carefully trained teachers for subjects like history, geography, literature, and nature study, all pleasant and enlightening subjects, no doubt, but not sufficient in themselves to a handicraftsman; while the principles and practice of a man's main occupation in life, his trade and means of livelihood, we leave to the chance instruction of teachers often as ignorant as the lads themselves—teachers, at all events, who have no special capacity to teach, no obligation to teach, no incentive to teach.

There must be doubtless many who can be nothing higher than labourers, whose abilities are too limited for cultivation. But I think that all who have had to deal personally with skilled workmen, and have studied them in a sympathetic spirit, will admit their ingenuity and resource within their narrow limits of thought. The ingenuity is due to their own mother wit, and the narrowness of its sphere of action is caused by their training—a mere repetition of workshop lore, tips, and even shibboleths, handed down from craftsman to apprentice, generation after generation. The larger the shop grows the less the manager and even the foreman hold personal intercourse with the men and apprentices, so that the lore becomes more and more stereotyped and empiric.

Here we have, I submit, one chief reason for the present outcry against the modern apprenticeship system, that it is no longer like the old intimate relationship. And that is quite true. In the old-time small shop the master was with and about his workmen, taught them

what he knew himself, and being well versed in the knowledge of his time, he was a teacher of value. The apprentice learnt his trade with this teacher, and gained breadth of view and insight from contact with a higher mind. But the advent of huge shops has changed all that. The masters no longer discharge this duty to their apprentices themselves, for I call it a duty; it is deliberately neglected, and the apprentice may justly demand that this duty shall be discharged by some one. Apprenticeship must again become an education. It is now too often degraded to a form of cheap labour. We are convicted of sweating the helpless. We say to the working man, "Your son shall spend the years of his youth to our profit, or we condemn him to the hopeless depths of unskilled labour." It is a crime. And it is a blunder, for it is short-sighted and suicidal.

LEEDS LOCAL SECTION.

THE MAGNETIC TESTING OF IRON.

By W. H. F. MURDOCH, B.Sc., Associate Member.

(*Paper received from the* LEEDS LOCAL SECTION, *August* 20, 1907, *read at Sheffield November* 25, 1907.)

Some years ago, whilst reading over that very classical paper of Professor Ewing's on "Contributions to the Theory of Induced Magnetism," I was impressed by the following paragraph referring to his magnetic model:—

"The model shows equally well other magnetic phenomena, which presumably depend on the inertia of the molecules, such as the fact that a given force causes more magnetic induction when suddenly applied, and leaves less residual magnetism when suddenly removed than when gradually removed."

And, again, referring to groups of small magnets:—

"When the impressed force **H** reaches a critical value, one of the outer members of the group becomes unstable, and swings slowly round, its next neighbours finding their stability weakened follow suit, and the disturbance spreads through the group in a way eminently suggestive of those phenomena of time lag in magnetisation which I have described in a former paper."

Professor Ewing also points out in his book "Magnetic Induction in Iron and other Metals," 3rd edition, pp. 128–135:—

"That the magnetic viscosity is most noticeable when we have to deal with feeble forces, or with small changes of force, or when the specimen is of considerable size. In such cases the ballistic method . . . is not properly applicable."

Further on he states that with hard iron and steel the effect is less, although with a moderately strong magnetising force, and then a small step-up, the value of $\frac{d\,\mathrm{I}}{d\,\mathbf{H}}$ was 5·3. Small specimens show much less of this "creeping" than large ones, and the cause of this appears to be obscure.

With very soft iron the time required to reach a maximum may be twenty seconds, so that a ballistic galvanometer of ordinary type would hardly take account of this.

E. Gumlich has also shown that the shape of the magnetisation curve varies in different materials with the mode of magnetisation, and depends greatly on the magnitude of the steps in the magnetising process. It is much less in sheet metal than in rods of the same

quality. The method of experimenting was with the magnetometer, the materials tested being in the form of ellipsoids and rods. The effect is much greater in soft materials than in hard ones.

E. Wilson, testing pure iron, found large discrepancies between the step-by-step method, and the reversal method of ballistic testing. Hoyt Taylor has recently given a method whereby these errors can be reduced or eliminated. The errors themselves are due to the time constant of the circuit, perhaps, being large, and to the viscosity referred to above, and in soft specimens they may amount to as much as 17 per cent.

It would appear, therefore, that the ballistic method of testing when applied to soft materials, or to large specimens, has to be used with caution.

In practice, the best-known permeameters are those of the Hopkinson, or S. P. Thompson type, Drysdale's permeameter, and the Picou permeameter. Time does not permit me to touch on the more elaborate instruments of Professor Ewing, the interesting direct-reading instrument of Professor F. G. Baily, and the Grassot fluxmeter.

Dr. Hopkinson's permeameter, if used as a traction instrument, is essentially of a workshop type. My objection to permeameters of this description is that: (1) The specimen has to be turned or machined to a certain size; (2) this size is small; (3) in the act of testing the whole magnetic circuit is broken; (4) the material itself may be subjected to stresses or shocks.

In the first place, cutting or machining a specimen should be reduced to a minimum, as undoubtedly its magnetic properties are affected thereby, and all stresses on the specimen itself should be avoided as much as possible. The engineer also wishes a test of the material as nearly as possible under conditions realised in practice, and this is not possible with small specimens.

Dr. Drysdale's permeameter of the plug type is a most ingenious instrument, but it involves the use of a galvanometer of the ballistic type, and besides the size of the specimen tested is extremely small. Since this has been cut out of a solid block, the stresses in it can hardly be allowed for, as the sharpness of the tool will affect these to some extent.

Referring to the Picou permeameter, a description of which, with results of tests, was published by Mr. A. Campbell,* a ballistic method of testing is used. Although the results obtained appear very satisfactory, this permeameter does not seem to me to offer any distinct advantages over Professor Ewing's double-yoke method. In both of these cases the specimens may be large bars, and in the case of the Picou permeameter there appears to be the additional trouble due to the fact that the "time lag" in magnetisation differs in different parts of the magnetic circuits, which affects the balance to an unknown extent. It also involves three distinct operations for each single reading.

* See Bibliography at end of the paper.

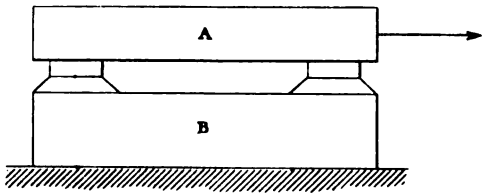
It occurred to me some years ago that a permeameter of a workshop type might be constructed, if instead of pulling the ends of the specimen apart, as in the S. P. Thompson or Hopkinson type, the specimen were made to slide for a very short distance on the yoke.

The present paper is based on the results of some experiments made for the purpose of finding if this was practicable.

If two magnetised plane surfaces each of area A square centimetres are in contact, and **H** lines per square centimetre pass between them, then the attraction between them is—

$$\frac{\mu \mathbf{H}^2 A}{8\pi} \text{ dynes.}$$

Since for air $\mu = 1$, and if the lines of induction in the iron pass



Workshop type of instrument.

FIG. 1.

A = Bar to be tested.
B = Yokes.

from one face to the other without spreading, then $\mathbf{B} = \mathbf{H}$, and the force is—

$$\frac{\mathbf{B}^2 A}{8\pi} \text{ dynes.}$$

The action of Dr. S. P. Thompson's traction permeameter, and of Dr. Hopkinson's permeameter used as a traction instrument, depend on this or a similar formula.

This Law of Traction has been confirmed by Bosanquet and Dr. Taylor Jones for a very wide range of inductions (*see* Ewing's Magnetic Induction).

Referring now to Fig. 1, it is easy to calculate the friction pull:—

Let—

W = weight of the moving portion of the specimen being tested.

$\frac{\mathbf{B}^2 A}{8\pi}$ = force due to magnetic attraction.

f_0 = friction pull when the specimen is demagnetised.

f = total friction pull required to keep the specimen in motion when magnetised.

γ = coefficient of friction for the sliding surfaces.

Then we have—

$$f=(F+W)\gamma,$$

or—

$$f=\frac{B^2 A}{8\pi}\gamma+W\gamma.$$

Since $W\gamma=f_0$ we obtain—

$$B=k\sqrt{f-f_0},$$

where—

$$k=\sqrt{\frac{8\pi}{A.\gamma}}.$$

This, of course, can be expressed in any unit we please, the value of the constant merely requiring alteration.

In this paper $f-f_0$ is measured in 16-oz. lbs., and **B** in lines per square centimetre.

With regard to the errors in using this method, it is easily seen that a considerable error in $f-f_0$ affects the value of **B** to half the extent only—

$$dB=\tfrac{1}{2}k(f-f_0)^{-\frac{1}{2}}df,$$

$$\therefore \frac{dB}{B}=\frac{df}{2(f-f_0)},$$

and the percentage error in **B** is—

$$\frac{100\,\delta f}{2(f-f_0)}.$$

Suppose $f-f_0$ is 12 lbs., $\delta f=1$ oz., then the error in **B** is only—

$$\frac{100}{2\times 12\times 16}=\frac{100}{384}=0{\cdot}26 \text{ per cent.}$$

If $f-f_0=1$ lb., then it is about 3 per cent.

The coefficient of friction is easily determined by finding the pull required to slide the specimen over the surface when it is demagnetised, and then loading it up mechanically either by a spring, or by putting weights on it. Here the difficulty of starting friction comes in. This appears to be generally greater than the normal sliding friction, and the coefficients of friction show little agreement amongst themselves, as has been pointed out by Professor Veitch and the other early experimenters on friction (Gregory's "Mechanics," vol. i.).

The results I obtained for starting friction coefficients between the cast-iron bar and wrought-iron yoke are given in the following table, which shows clearly how erratic they appear to be :—

TABLE I.

Load.	Pull.	Friction Coefficient.
5·25	0·757	0·144
7·25	1·135	0·157
10·25	2·030	0·196
12·25	2·250	0·184
15·25	3·060	0·200
19·25	6·060	0·315
26·25	6·750	0·256

When, however, an initial pressure is applied by hand to the bar much more consistent results are obtained. These are given in Table II. below, and are the results of experiment with the same bar and yoke, no special precautions being taken beyond seeing that the surfaces were clean :—

TABLE II.

Load.	Pull.	Friction Coefficient.
7·25	1·180	0·163
12·25	1·875	0·153
15·25	2·430	0·159
19·25	2·875	0·149
26·25	4·065	0·154
33·25	5·375	0·162
40·25	6·210	0·154
47·25	6·750	0·143
61·25	9·520	0·155

Average value of $\gamma = 0·155$.

The maximum departures from the mean values are, therefore, 5·16 per cent. too high, and 7·4 per cent. too low. With care the pull can be measured to within an ounce, so that the maximum possible error in the first experiment would be 5·3 per cent., and in the last 0·63 per cent.

It is seen that the errors are not very great and the sign of the errors in γ and $f-f_0$ are of opposite characters, so that they may neutralise to some extent.

Other values for γ are as follows:—

Cast iron on cast iron	... ...	0·143	Hele-Shaw
" " "	... ...	0·150	Perry.

The value obtained above seems sufficiently accurate.

It is interesting to note that the coefficient of friction might be determined electrically. In this case **B**, the magnetic induction for a given pull $f-f_0$, is measured by means of a ballistic galvanometer.

The equation can be written—

$$f = k' \mathbf{B}^2 + f_0,$$

where k' is a constant, so that if values of f are plotted vertically and $\mathbf{B}^2$ horizontally a straight line is obtained. The intercept obtained for $\mathbf{B}^2 = 0$ gives f_0 the friction pull, and hence the weight of the sliding mass being known the coefficient of friction can be determined. Since k' involves γ, this coefficient could perhaps more accurately be determined by measuring the slope of the line.

In the equation above a term involving the force necessary to accelerate the moving specimen is omitted, so that the expression is not strictly accurate. More strictly—

$$f = k' \mathbf{B}^2 + f_0 + \mathrm{P},$$

where P is the force causing acceleration.

Since—

$$- k' \mathbf{B}^2 - f_0 = \frac{f + \mathrm{W}}{g} a,$$

where a is the acceleration, we may write the ratio—

$$\frac{a}{g} = \frac{f}{f + \mathrm{W}} - \frac{k' \mathbf{B}^2}{f + \mathrm{W}} - \frac{f_0}{f + \mathrm{W}}.$$

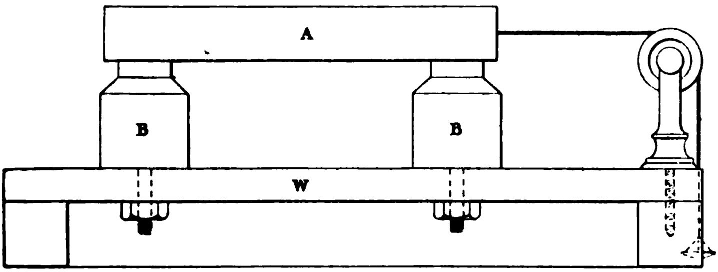
In the expression previously given for magnetic induction, $\mathbf{B} = k\sqrt{f - f_0}$, the acceleration term is not included. This is because it is very small, and in many cases zero.

The coefficient of friction does not vary for the comparatively small loads occurring in practice. At 8,000 lines per square centimetre there is only 60 lbs. per square inch pressure. The bearing surfaces, however, must be kept perfectly clean and free from lubricants.

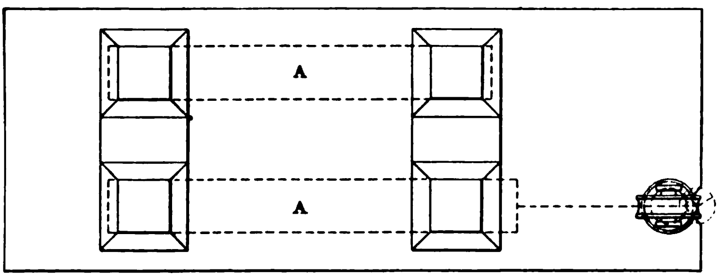
The rough instrument shown in Fig. 2 was made purely for experimental purposes. It consists of two bars and two yokes. The bars are of cast iron, and the yokes of soft wrought iron, the surfaces in contact being carefully trued. One of these bars remained fixed, the other being movable, a string being put round it and passing over the pulley, the string carrying the necessary scale pan and dead weights.

The method of working was as follows; the no-load friction pull having been determined and the constant for the formula calculated, current was then passed through the coils on the bars to be tested, and was kept constant by means of a suitable resistance, and the pull f was measured. Then **B** can be calculated. A series of readings is then taken.

The object of the two bars in the instrument in Fig. 2 is twofold. In the first place it lengthens the specimen under test, thereby reducing the effect of the gaps, which are equivalent to a length of iron of $\delta l\ (\mu - 1)$, where δl is the width of the gap, μ the permeability.



Elevation.



Plan - Double-yoke type of instrument.

FIG. 2.

A = Bars to be tested.
B = Yokes.
W = Wood-base.

Again, to allow for magnetic resistance of the yokes, I used a double-yoke method of testing. The bars were first tested for a length of 16 cm., and then for 8 cm. If **H** represents the true magnetising force, **H**′ the apparent magnetising force, L_1 the first length, and **H**″ and L_2 the second magnetising force and lengths, then, e being the error due to yokes, etc.—

$$\mathbf{H} = \mathbf{H}' - \frac{e}{L},$$

$$\mathbf{H} = \mathbf{H}'' - \frac{e}{L_2} \quad \text{or} \quad \mathbf{H}'' - \mathbf{H}' = \frac{e}{L_2} - \frac{e}{L_1}.$$

Hence, if $L_1 = 2 L_2$ we have—

$$H'' - H' = \frac{e}{L_1},$$

as used by Ewing in his double-yoke method of testing with ballistic galvanometer. By taking two curves, one for a long length and the

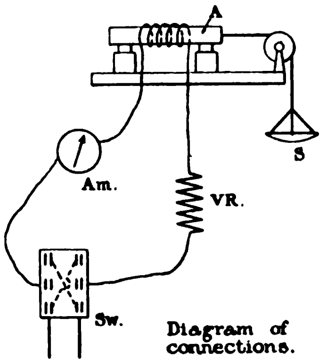


FIG. 3.

A = Bar to be tested.
S = Scalepan.
VR = Variable resistance.

other for a half of that length, the true **H** is found by shifting the values along horizontally a distance **H**″ — **H**′. The following is a table of results :—

TABLE III.

Double-yoke Arrangement—Long Length.

$-f_0$.	$B = 1{,}744\sqrt{f - f_0}$.	Current.	$H' = 4{\cdot}47\,C$.
4·25	3,630	2·22	10·00
5·25	4,030	2·70	12·00
6·75	4,550	3·20	14·25
8·75	5,200	4·00	17·85
10·13	5,600	5·10	22·80
11·50	5,970	5·60	25·00
12·75	6,300	6·76	30·20

Double-yoke Arrangement—Short Length.

$f - f_0$.	$B = 1{,}744 \sqrt{f - f_0}$.	Current.	$H'' = 9C$.
6·75	4,220	1·6	14·4
7·25	4,700	1·9	17·1
7·75	4,890	2·2	19·8
10·25	5,440	2·6	23·4
11·25	5,850	3·1	27·9
12·13	6,140	3·6	32·4
13·50	6,450	4·2	36·8

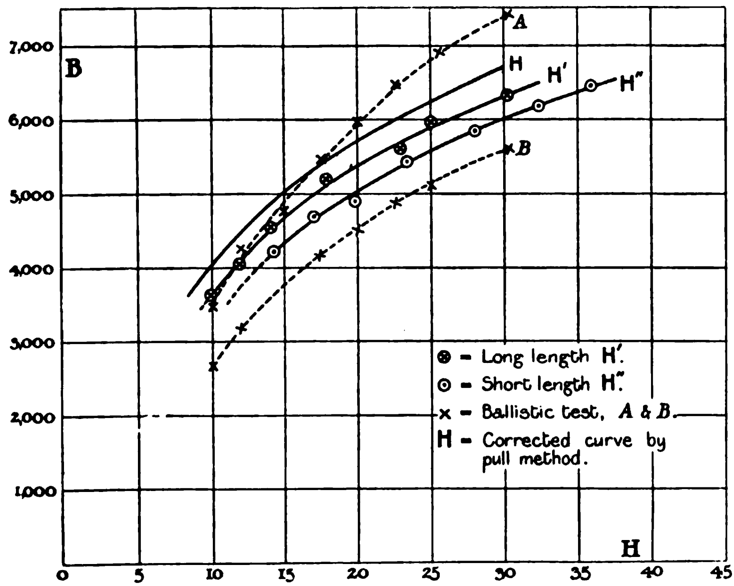


FIG. 4.—Results of Tests—Double Yoke Type of Instrument.

Regarding the curves in Fig. 4, the ballistic curve A was obtained by plotting the results of the test for long and for short lengths, and treating these in the same way as for the traction method. It will be noticed the curve A lies partly above the corrected curve H for the traction method. This is because the ballistic search coil was placed

at the centre of one of the test bars. The coefficients of leakage between centre of test bar and the opposite side of the gaps were tested for both long and short lengths and with varying ampere-turns. It was found to have a practically constant value of about 1·3.

The ballistic curve A was now corrected to curve B, representing the induction at the gaps, by multiplying its ordinates by 0·75. This curve lies about 1,000 lines below the corrected traction curve H. The reason for this is that the leakage flux adds about one-third of a pound to the traction pull.

It will be seen that the corrected ballistic curve B is of exactly similar form to the corrected traction curve H.

The double-yoke instrument is chiefly of theoretical interest, and suitable only for laboratory work. The chief trouble with it appears to arise from imperfect demagnetisation of the bars and yokes between the experiments at long and short distances respectively. It is also more difficult to get correct alignment of the four surfaces in contact than would be the case if there were only two as in Fig. 1.

An instrument of the single-yoke type was constructed, and several sets of experiments were made with it. Two of these sets of experiments are given in Tables IV. and V.

The coefficient of friction was measured and taken as $=0·163$, and the bar tested was of cast iron.

The instrument was also tested ballistically, and the results of this are given in Table VI. The agreement with the traction method seems very fair.

Workshop Permeameter.

Table IV.

Results of Test.

Current.	H′.	$f - f_0$.	$B = 1{,}700\sqrt{f - f_0}$
1·85	8·0	3·150	3,020
2·40	10·4	4·875	3,750
2·80	12·1	6·150	4,220
3·50	15·1	9·275	5,175
4·30	19·0	10·775	5,575
5·35	23·2	15·275	6,550
6·35	27·4	17·020	7,000
7·85	34·0	20·520	7,700

A second test gave the following values :—

TABLE V.

Current.	H'.	$f - f_0$.	$B = 1{,}700 \sqrt{f - f_0}$.
1·93	8·3	3·000	2,940
2·48	10·6	4·750	3,710
2·92	12·6	6·525	4,240
4·45	19·1	13·275	6,200
5·60	24·1	16·750	6,950
6·65	28·6	18·150	7,220
8·25	35·6	21·775	7,920

The results given in Tables IV. and V. show that the instrument is working very consistently, and these results are plotted on the following curve :—

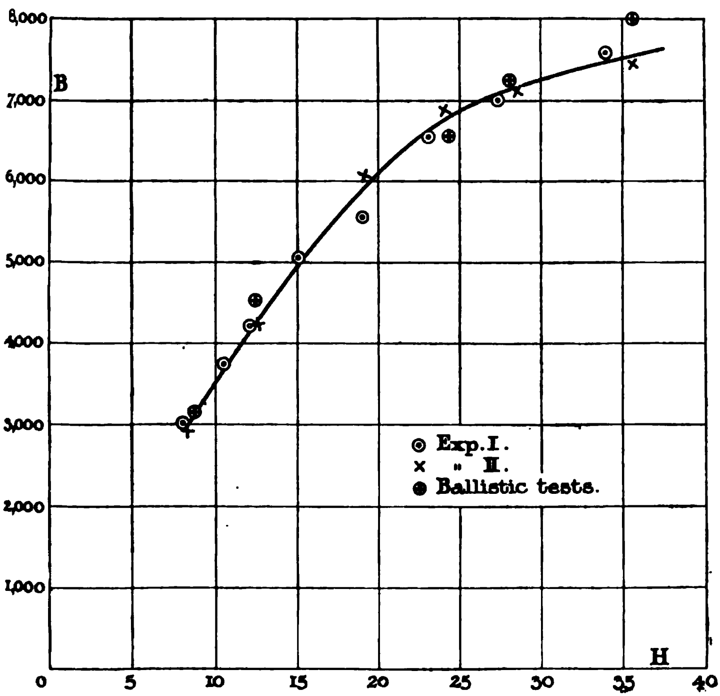


FIG 5.—Results of Test.—Workshop Type of Instrument.

A ballistic test gave the following set of values for **H** and **B** :—

Table VI.

Results of Ballistic Test.

H.	B.	Residual Magnetism.
8·65	3,150	2,210
12·50	4,550	3,060
24·30	6,550	4,100
28·10	7,225	4,250
35·70	8,000	4,420

The leakage factors were determined by winding three turns of double silk-covered wire in the following places :—

(*a*) Centre of magnetising coil.
(*b*) End of magnetising coil.
(*c*) On neck of gaps.
(*d*) On middle of yoke.

A current of 5·6 amperes was passed through the magnetising coil, and the kicks on ballistic galvanometer noted.

The following are the mean values :—

a.	*b.*	*c.*	*d.*
90	80	75	80

The leakage coefficient for crossing a gap is therefore $\lambda = \frac{80}{75} = 1·07$.

As, however, the leakage lines pass through the yoke, this probably does not influence the pull to such a degree as one would at first expect, if you regard them as lost. To a certain extent the lines will add to the pull, and be taken into account in $f - f_0$.

A sample of wrought iron was next tested. The particulars are as follows :—

Friction pull $f_0 = 0·75$ lbs.
$\gamma = 0·15$ „
Constant $= 1,700$ „

When the current reached 8·3 the number of turns, originally three, was altered to six, doubling the constant.

The values appear to agree very well with other tests of wrought iron when the correction for gap and yoke resistance is made.

It would appear that an instrument of this type affords a ready means of testing iron, especially the grades with lower permeability.

TABLE VII.

Test of Wrought-iron Bar.

Current.	$H = 0{\cdot}314$ C.	$f - f_0$	$B = 1{,}700\sqrt{f - f_0}$
2·0	0·62	0·75	1,470
2·9	0·94	1·00	1,700
4·4	1·38	1·37	1,990
5·6	1·76	2·12	2,500
6·7	2·10	2·62	2,750
8·3	2·60	3·91	3,360
5·6	3·52	10·62	5,550
6·7	4·22	14·62	6,500
8·6	5·40	23·12	8,160

The material under test may be left almost untouched by a tool, and may be in very large pieces. The magnetic circuit remains absolutely unbroken throughout the tests, and the molecular magnets are not subjected to abrupt shocks, but may be carried gradually through a complete cycle. The test is of the simplest description, requiring only the reading of an ammeter, and adjusting of a dead weight, or spring balance.

A simple permeameter would be one as in Fig. 1. This would be used for ordinary workshop tests, the curve obtained from the material being compared with that of a standard bar, instead of using a double-yoke method of testing. A few readings need only be taken to settle the magnetic properties sufficiently. The accuracy aimed at in all the foregoing experiments was about 5 per cent., this being sufficient for practical purposes. I have endeavoured throughout to show that the results obtained are consistent, and for purposes of comparison that is all that is necessary.

I have to express my indebtedness to Professor Francis G. Baily, to Mr. W. Mansergh Varley, Mr. W. H. Eccles, and Mr. Charles F. Smith, for their kindly criticism and valuable suggestions.

I wish also to say that I probably would not have made any experiments but for the assistance of my colleague, Mr. A. T. J. Kersey, who made the instruments for me, and who has suggested many improvements, which will probably be embodied in a newer type of instrument than those at present shown.

Data regarding Constants.

Calculation of the Constant for the Double-yoke Type of Instrument.

From formula on page 140 we have—

$$\mathbf{B}=\sqrt{\frac{11,183,000}{A\,.\,\gamma}(f-f_0)},$$

$$A=24\cdot5, \qquad \gamma=0\cdot15,$$

$$\sqrt{\frac{11,183,000}{24\cdot5\times0\cdot15}}=1,744 \text{ approximately.}$$

Hence—

$$\mathbf{B}=1,744\sqrt{f-f_0}$$

For the single-yoke instrument the constant was 1,690, and was taken as 1,700.

The dimensions of the bars tested were 8, 12, and 16 cm. long by about 12 sq. cm. They were machined on one face only, the rest being untouched by a file or tool of any sort.

Magnetic Resistance of Air-gaps and Yokes.

If N is the total flux, then—

$$N=\frac{4\pi n C}{\frac{l_1}{\mu_1 A_1}+\frac{2 l_2}{A_2}+\frac{l_3}{\mu_2 A_3}}.$$

Where μ_1 is the permeability of the specimen for the given induction—

μ_2 is the permeability of the yokes,
A_1, A_2, A_3 the cross-sections of specimen air-gaps and yoke respectively,
l_1 is length of specimen,
l_2 is equivalent length of air-gap,
l_3 is length of path of lines through the yoke.

Now N, the flux in the bar, is $\mathbf{B}\,A_1$—

$$\therefore\ \mathbf{B}=\frac{4\pi n C}{\frac{l_1}{\mu_1}+\frac{4 l_2 A_1}{A_2}+\frac{l_3 A_1}{\mu_2 A_3}},$$

$$\mathbf{B}=\frac{4\pi n C}{\frac{l_1}{\mu_1}\left\{1+\frac{4 l_2 A_1 \mu_1}{l_1 A_2}+\frac{l_3 A_1 \mu_1}{l_1 \mu_2 A_3}\right\}}$$

The apparent magnetising force, then **H**′ or **H**″—

$$\mathbf{H} = \frac{4\pi n C}{l\left\{1 + \frac{4 l_2 A_1 \mu_1}{l_1 A_2} + \frac{l_3 A_1 \mu_1}{l_1 \mu_2 A_3}\right\}},$$

or—

$$\mathbf{H} = \frac{\mathbf{H}'}{(1 + \alpha + \beta)},$$

α and β being the values of the terms above.

Order of Values of Yoke and Gap Resistance for a given Induction.

Taking the terms in the denominator above, we have—

$$\frac{4 l_2 A_1 \mu_1}{l_1 A_2} = \frac{4 . 0\cdot0033 . 12\cdot25 . 200}{18 . 8} = 0\cdot23,$$

$$\frac{l_3 A_1 \mu_1}{l_1 A_3 \mu_2} = \frac{8 . 12\cdot5 . 1}{16 . 18 . 5} = 0\cdot06.$$

Hence $\mathbf{H} = \mathbf{H}'(1 - 0\cdot3)$ approximately for the double-yoke instruments.

The value for l_2, namely, 0·0033, is that given by Ewing for the magnetic equivalent of the air-gap between two plane surfaces. This value appears large, but there is, as he points out, probably a lowering of permeability owing to the machining of the surfaces.

For the instrument constructed as in Fig. 1 we have only two gaps, and the cross-section of the yoke is much greater.

In this case—

$$\mathbf{H} = \frac{\mathbf{H}'}{1 + \frac{2 l_2 \mu_1 A_1}{l_1 A_2} + \frac{l_3 A_1 \mu_1}{\mu_2 l_1 A_3}}.$$

The value of—

$$\frac{2 . l_2 \mu_1 A_1}{l_1 A_2} \text{ is } \frac{2 . 200 . 12 . 0\cdot0033}{12 . 50}$$

or 0·024.

And—

$$\frac{l_3 A_1 \mu_1}{\mu_2 l_1 A_3} \text{ is } \frac{12}{5 \times 50}$$

or 0·048.

The total is 0·072.

$$\therefore \mathbf{H} = \mathbf{H}'(1 - 0\cdot07) \text{ approximately.}$$

This refers to the case where the specimen is of cast iron. When the specimen is of wrought iron the correction is much greater. The above figures are merely to indicate the order of error due to gaps and yokes, as, of course, this varies for each point on the curve.

List of Symbols Used.

B = magnetic induction per square centimetre.
H = true magnetising force.
H′, **H″** = apparent magnetising force.
A = area of cross-section.
f_0 = friction pull at no magnetisation.
f = pull when magnetised.
γ = friction coefficient.
μ = permeability coefficient.
W = weight of moving bar.
a = acceleration.
g = acceleration due to earth.

BIBLIOGRAPHY.

Ewing's "Magnetic Induction in Iron and other Metals." Electrician Publishing Company, London, 1900.

"Contributions to the Molecular Theory of Induced Magnetism." Ewing. *Phil. Mag.*, 5th Ser., vol. 30, p. 208, 1890.

"On the Testing of Cast Iron and other Materials by the Ewing Permeability Bridge." A. Campbell. *Journ. Inst. of Elect Engin.*, vol. 36, p. 220, 1906.

"Dependence of Coercive Force on the Mode of Magnetisation." E. Gumlich. *Electrician*, vol. 58, p. 377, 1906.

"On the Use of Chilled Cast Iron for Permanent Magnets" A. Campbell. *Electrician*, vol. 58, p. 333, 1906.

"Limitations of the Ballistic Method for Magnetic Induction.' A. Hoyt Taylor. *Electrician*, vol. 57, p. 968, 1906.

"The Picou Permeameter." A. Campbell. *Electrician*, vol. 58, p. 123, 1906.

"Hysteresis and Permeability of Iron in Relation to Frequency." L. Schames. *Electrician*, vol. 59, p. 234, 1907.

"The Electromagnet." Cantor Lectures by Professor S. P. Thompson. *Journal of the Society of Arts*, vol. 38, 1890.

"Friction." Cantor Lectures by Professor Hele-Shaw. *Journal of the Society of Arts*, vol. 34, 1886.

"A New Form of Permeameter." Francis G. Baily. *Transactions Royal Scottish Society of Arts*, May 13, 1907.

"The Magnetic Properties of Almost Pure Iron." E. Wilson. *Proceedings of the Royal Society*, vol. 62, p. 369, 1898.

"Grassot Fluxmeter." *Electrician*, vol. 56, p. 560, 1906.

"Measurement of Magnetic Induction." H. Armagnat. *Revue Electrique*, vol. 7, p. 113, 1907.

Discussion.

Mr. Beauchamp.

Mr. J. W. Beauchamp: I would like to ask the author if he has made any experiments with sheet iron such as is used for transformer work, and, if so, if it is necessary to put them edgewise on the pole-pieces.

Mr. King.

Mr. W. N. Y. King: I have been very much interested in the excellent paper from the point of view of the central station engineer. The size and correspondingly the cost of electric generators of given output have of late years been much reduced by the increase in the

speed of driving by the adoption of high-speed engines and turbines. The use of high-permeability steel in the construction of electrical machines has produced an effect in the same direction, as well as an economy in the power required for magnetisation. No one appreciates the value of details such as these, with a view to keeping down costs, more than the producer of electrical energy. In 1898 I had occasion to test a sample of cast steel to determine the hysteresis loss for maximum values of **B** varying from 4,000 lines per sq. cm. to 16,000, and I used, with satisfactory results, the ballistic method as described by Professor Ewing in a paper read before the Institution of Civil Engineers in 1896. I think some further interesting results would have been obtained if the author had continued his tests with higher value of **B** than the maximum value mentioned, namely, 8,000 lines per sq. cm. I would like to ask the author if there are any reasons detrimental to the higher values of **B** being used. Mr. King.

Mr. J. R. Williams: With regard to the yoke and bar to be tested, I should like to know whether these were scraped up to a true plane surface previous to the experiments being carried out. If not, I should consider that the area of contact would vary within wide limits, and that would undoubtedly tend to vary the no-load friction between the bar and yoke; again, the starting friction would be greater than the sliding friction, and I do not consider that the method adopted by the author, namely, "starting the movement of the bar by pressing against it with his fingers," a satisfactory one. No two tests will be alike, and the results will therefore only be approximately correct. On page 149 of the paper the author states that the material under test may be left almost untouched by a tool, and may be in very large pieces. I am of the opinion that this will increase the liability to error due to varying area of contact, and that a test carried out on these lines will result in errors of rather large magnitude. As a general rule the material has to be tooled before use, and therefore the comparison between a tooled specimen and the material used in the actual machine will be more correct than one between a rough untooled specimen and the tooled material in actual use. Mr. Williams.

In Table I the author gives some erratic data obtained for the coefficient of friction between the yoke and the test-bar, and if the surfaces are not true planes this will probably account for the varying results observed.

I think a better method of measuring the starting friction of the test-bar would be by means of a cord passing over a pulley and attached to a spring balance, which would register the pull. In my opinion this would be an infinitely better method than the one shown by the author.

Mr. W. T. Wardale: I should like to know what method the author adopts for determining the starting friction, as I think a push with the finger would hardly be definite enough. Mr. Wardale.

Mr. T. W. Sampson: With reference to tooling as mentioned on page 149 of the paper, I notice that the specimen on the table has been filed, properly surfaced, and scraped very carefully; and it seems to me Mr. Sampson.

Mr. Sampson. that, although one may gain to some extent in the simplicity of the test, yet as regards the machining one would not gain much advantage, at any rate in reduction of the time occupied in the preparation of the specimen. I don't know whether any of the members present are familiar with the bar and yoke, but in this method there is a constant number of turns, and the bar is turned to a definite size. In that case the magnetic force is always the constant quantity, and the effect simply depends on getting the proper diameter of a piece of steel, which can be easily done in the lathe to any degree of accuracy. In winding the specimen and calculating the value, it seems to me that considerable error may be made, especially in a rough specimen. The value of **B** depends to some extent upon the area of the specimen. On a rough specimen this cannot be obtained very accurately, and it has not been taken into consideration so far as I have noticed it. I would like to ask Mr. Murdoch if he has got any relative values of the effect of machining a turned specimen, and on a specimen such as that on the table; and also as to what degree of accuracy the value of **B** could be calculated, as it seems to me that it is possible in trying to get the simplicity to that extreme to lose a good deal in accuracy, and consequently to lose more than is gained as regards practical purposes. I would like to ask if there is not a slight error in the formula given on page 139, and whether it should be $\frac{\mu^2 \mathbf{H}^2 A}{8\pi}$ instead of $\frac{\mu \mathbf{H}^2 A}{8\pi}$.

Mr. Marsh. Mr. E. J. MARSH : I should like to ask if it is necessary to have all the test pieces uniform in length and cross-section, when using the workshop type of this instrument for taking comparative tests, for if this was not required, and the results obtained were approximately the value of the larger specimen, with the area of contact on the poles remaining the same, it would largely extend the use of such an instrument in the workshop, where often series of rapid tests are necessary, in which case the cost and trouble of mounting the specimens to a uniform cross-section would probably be prohibitive. Regarding the curve, it would be interesting to know at what point the author obtains the best results.

Mr. Lovell. Mr. R. P. LOVELL : With regard to the question of starting friction, it struck me that this need not be considered if the weight put into the scale pan is just sufficient to cause a slow movement of the bar in the direction of its length. The times taken for the bar to move over a given length would be a measure of the flux with various values of the magnetising current. In order to eliminate the disturbing effect of the ends of the bar as it is moved along, the ends should project well beyond the yoke. I think the current might be led into the coil through mercury troughs placed parallel to the direction of motion, and the arrangement might be calibrated by placing known weights on the top of the bar under test, the bar being in a demagnetised condition.

Mr. Burnand. Mr. W. E. BURNAND : I think there is something in the remark with regard to determining the starting friction, and I agree with the former speaker that the bar, being in motion, will get over the difficulty of

starting friction. Also, the pressure exerted between the contact surfaces depends not only upon the total flux, but on the distribution of the flux (being greater with an uneven and concentrated distribution than with an even distribution of the flux), and as this distribution will vary at different parts of the **B**/**H** curve, some error, I think, will naturally be introduced. Mr. Burnand.

Mr. T. E. HERBERT: I have been very much interested in the points raised in the discussion, but the criticism seems to have been directed towards showing possible errors, and I am of opinion that the method could not be considered absolutely trustworthy, and will, I think, require a considerable amount of practice. No doubt in the hands of a skilled experimenter it will be possible to feel that the amount of starting push required to set the bar moving is approximately the same in any two or more cases, and experience will enable them to obtain results which will agree with a fair amount of accuracy, but if the instrument is placed in the hands of a person using it for the first time large errors will probably result. Mr. Herbert.

Mr. W. H. F. MURDOCH (*in reply*): In reply to Mr. Beauchamp's question, I have not yet made any experiments on sheet iron. I see no reason, however, why it could not be tested if clamped into a block and allowed to slide edgewise, as suggested. Regarding the scraping of surfaces, referred to by Mr. Williams, these were scraped as carefully as possible, and after the lapse of one and a half years I have not found any departure from the original value of the friction coefficient. That is to say, I still obtain 0·155, or 0·156, from the average values. With reference to his remarks regarding the machining, or tooling, of the specimen under test, those before the members were certainly surfaced for sliding, but otherwise only filed. It seems to me, if one considers a small cylindrical specimen, the mass of the material acted upon by the stresses due to tooling, such as turning in a lathe, has a much greater ratio to the total mass of the specimen than in the large specimens I use. In the case of a dynamo-field magnet the material inside is certainly almost free from effects of machining, and is in a large mass. I consider, therefore, the engineer wishes his material tested under nearly similar conditions. In reply to Mr. King, there is no reason why higher values of **B** were not used. The manufacturer, however, in using cast iron, works at inductions of 4,000 to 6,000 lines, and my tests were under similar conditions. I was limited also by time and the duties in a Technical College. Mr. Murdoch.

Referring to Mr. Wardale, he thinks the starting-push is not satisfactory. A method of eliminating this is to use the screw starting arrangement shown. In this way the specimen is always given an equal starting-push. At the same time, I do not consider this at all necessary. One reason for this is that in the formula for induction we have—

$$\mathbf{B} = K\sqrt{f - f_0}.$$

In the denominator of the constant K it will be noticed that we

Mr. Murdoch.

have the coefficient of friction, which is itself measured by observing the pull. If e_1 is the error made in measuring the induction pull, and e_2 the error in the friction coefficient, then we have the ratio—

$$\frac{\sqrt{1 \pm e_1}}{\sqrt{1 \pm e_2}},$$

or $\left(1 \pm \frac{e_1}{2} \mp \frac{e_2}{2}\right)$ approximately, expanding by the Binomial Theorem.

Now, it appears the errors are of the same magnitude, and they are opposite in sign, so that they cancel out completely. An absolute determination of the coefficient of friction is unnecessary, merely an accurate measurement of the *ratio* of the pulls being required. Mr. Sampson refers to the tooling of the specimen with which I have already dealt. With reference to determining the value of **H** in the double-yoke method of testing, the value is almost completely free from error. In the single-yoke instrument the orders of the error are indicated, and there is no reason why these should be excessive. On a large specimen any error due to area of coil, being different to that of the specimen itself, would have a less percentage effect than in the case of a small specimen. I am not responsible for the formula, $\frac{\mu \mathbf{H}^2}{8\pi}$, to which Mr. Sampson takes exception. I consider it perfectly correct, and proofs of it will be found in Clerk Maxwell's *Electricity and Magnetism*, vol. ii. pp. 641–646, also in J. J. Thomson's *Elements of Electricity and Magnetism*, 1st ed. p. 266.

The value of the permeability, 200, given on page 151, is merely an average value for cast iron.

Mr. Marsh refers to the size of specimens. This may vary within wide limits, according to the method of testing double or single yoke and the permeability of the specimen. I have reasons, however, for keeping them a certain shape, to which I shall allude later. He also asks from which part of the curve **B-H** I get the most accurate values. This is rather difficult to reply to offhand. The greater the pull the less the error in measuring it becomes; the accuracy of **H** depends on instrumental errors to some extent, and to the circumstances connected with the design of the apparatus. I should think it possible with a properly designed instrument to obtain an accuracy of 1 per cent., and I hope to manage this with an improved type. With reference to the remark of one speaker, regarding the measurement of friction by observing the spaces passed over, this is perfectly sound, and was the method used by Morin and other early experimenters on this subject, to whom I have referred in the paper. It really does not matter whether the coefficient of friction is exact or not, so long as the tests are made by the same experimenter. The errors cancel, as already pointed out. For instance, one always makes an error with scale-pan weights, owing to the friction of the pulley—that is, the weight in the pan is always rather greater than it should be. However, the pulls and true weight here follow a straight-line law (passing through zero), and

since this merely multiplies $(f - f_0)$ and γ by the same constant, it is entirely eliminated. Every one finding the value of the coefficient of friction will obtain a slightly different value, depending on the sensitiveness to which they can measure the sliding. This also deals with the point raised by Mr. Herbert. He stated that the instrument could only be used by skilled experimenters. Now, I can detect pulls to within an ounce, whereas a person using it for the first time would only get to within two or three, but, as already pointed out, the errors cancel out, so that the values of **B** would be the same. Referring to Mr. Burnand's remarks regarding the distribution of flux over the gaps, it is of course essential, in order that the law of traction is obeyed, that this should be uniform; I have had this in view very carefully. If **B** was not constant, then the pull would be— Mr. Murdoch.

$$P = \frac{1}{8\pi}\int \mathbf{B}^2\, dA \text{ dynes,}$$

where **B** is an unknown function and dA an element of area. It appears from ballistic tests I have made over gaps that, so long as the pieces form a well-faced gap, the distribution is fairly constant. The difficulty is more a question of correct facing of the gap surfaces than of flux distribution, since the use of a narrow gap would eliminate any errors due to flux distribution. Neither do I find the flux altering perceptibly as the bar is allowed to slide—that is to say, to within 1 in 150 or so, the limit of accuracy of my experiments. Another speaker suggested a method of measuring the coefficient of friction which, no doubt, is quite correct, but this does not interest me. Regarding the shape of the specimen, there is an advantage in having this of such a shape that it demagnetises itself quickly. In making an experiment on iron of this sort one wishes to leave the iron at the end of the experiment pretty much in the same condition as it was when one started. Where a closed magnetic circuit is used it is necessary to demagnetise the iron through a hysteresis loop gradually approaching a point, at which value we stop, and the magnetism is then taken entirely out of the bar. In my case I do not go to that extreme at all, but simply reduce the magnetism by reversing the current and inserting resistance in the circuit.

Sometimes I used alternating current, and reduced it to a small value. I chiefly used the reversing switch and continuous current. In this way I reduced the current to, say, half an ampere or so. That left the bar on the yoke with a certain amount of residual magnetism, and when the bar is pulled off the yoke the magnetism goes down to a small value. A few experiments were made on cast-iron and wrought-iron bars. In Fig. D the ordinates represent the deflection of the ballistic galvanometer and the abscissæ the number of times the bar was lifted off the yoke. The galvanometer was damped in each case, so that the intervals were about the same, namely, 25 seconds.

It will be noticed that the wrought iron demagnetises much faster than cast iron, and sinks to a lower final value. Of course, the pull

Mr. Murdoch.

corresponding to an induction of about 200 lines per square centimetre in the cast-iron bar is hardly measured by this instrument, so that there is no necessity to demagnetise it further. In order to obtain the magnetisation of the yoke, this may be simply done by placing on it the wrought-iron bar referred to above, which has a feeble residual value, and connecting the ballistic galvanometer to a few turns wound on the

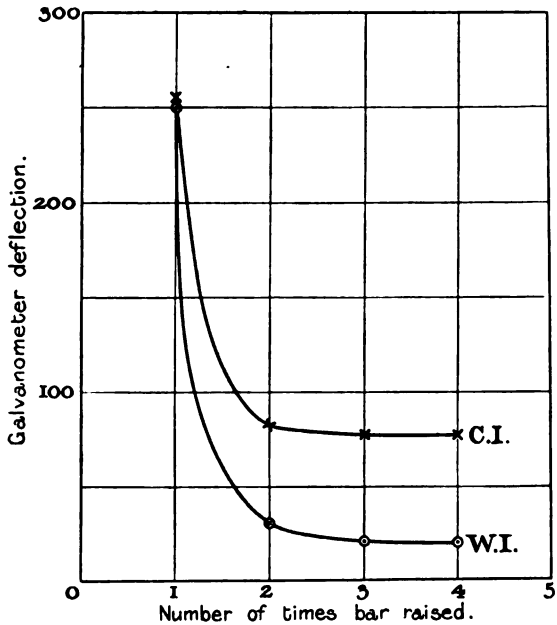


FIG. D.

yoke. On removing the bar a deflection is obtained, say δ_1, proportional to $N_1 + N_2$, if N_1, N_2 are the lines of induction in bar and yoke respectively. Now, reversing the bar, we obtain $N_1 - N_2 = \delta_2$. From these equations,

$$N_1 + N_2 = \delta_1,$$

$$N_1 - N_2 = \delta_2,$$

the magnetisation of the yoke and wrought-iron bar is estimated easily. My experiments show that the yoke residual induction was only $\frac{1}{18}$ of that of the wrought-iron bar, so that it is quite negligible.

MANCHESTER LOCAL SECTION.

FURTHER NOTES ON THE ELECTRICAL DRIVING OF TEXTILE FACTORIES.

By H. W. WILSON, Associate Member.

(*Received from the* MANCHESTER LOCAL SECTION, *November* 18, *and read at Manchester, November* 26, 1907.)

Nearly three years ago I had the honour of reading a paper * before this Institution on the same subject, but as since that date distinct progress has been made, it was considered that a further paper would be likely to promote a useful and instructive discussion.

Three years ago the actual horse-power of motors installed for driving textile factories in Great Britain was limited to a few hundreds, practically all in one mill, and only a very small fraction of the then total was actually in cotton mills.

So far as it has been possible to estimate from answers to inquiries sent by the various firms interested, the total horse-power which will be installed and running by about the end of March, 1908, will be about 28,000, and this is almost entirely in cotton spinning mills. These figures show, of course, that a very distinct progress has been made, and that the conservatism of the textile manufacturers with regard to the driving equipment of their factories is being slowly broken down.

When, however, it is considered that in the United States and Canada there is something like 250,000 H.P. of motors installed in textile factories, and that the orders for such machines received by one firm during the past half-year, for supplying various mills in the United States, amounted to about 25,000 H.P., or practically as much as has been done in this country in three years, and when besides it is remembered how very much the total power requirements of the British mills exceed those of the United States, it will be recognised what immense room there is for further development here.

There has been almost endless discussion in various newspapers concerning the advantages or disadvantages of the electrical driving of textile machinery, and some very exaggerated statements have been made on both sides. This is a circumstance greatly to be deprecated

* *Journal of the Institution of Electrical Engineers*, vol. 34, p. 757, 1905.

as it tends to prevent fair consideration of a question that is worthy of the most careful investigation. There is also an unfortunate tendency on the part of the various electrical manufacturing firms who are carrying out work of this class to try and impress upon the minds of their customers that by some special system of their own they are inevitably bound to get better results than any of their competitors. Statements of this kind are hardly justifiable, as it is, of course, apparent that there cannot possibly be anything particularly novel in the design of the machines which are installed.

Every one agrees that for work of the class under consideration a 3-phase equipment has decided advantages over a direct-current, and in the absence of a satisfactory single-phase motor for anything except very low periodicities, there is no doubt that the present general course of action will continue.

It is quite obvious that an induction motor, either squirrel-cage or slip-ring, made by one manufacturer does not differ very materially in its performance from a machine of the same class made by another, and that provided the same guarantees as to the behaviour of the machines are given by any two firms the results which will be obtained when in operation will be the same. It may appear unnecessary to emphasise this point so much, but it must be remembered that persons who are not electrical experts, and having very little knowledge of the actual design of electrical machinery are apt to be misled by manufacturers who state theirs is the one and only system which will give perfect results in operation. There is no special system in the driving of a textile factory which will inevitably give the best results, but there is scope for the exercise of an immense amount of judgment and knowledge of the conditions to be met in the arrangement for the driving of the various textile machines. To this point we will refer later, but at the moment it may be of advantage to repeat from my previous paper the statement of the advantages and objections to the electrical driving of the average factory.

The advantages which the advocates of electrical driving urge for the operation of large textile factories are as follows:—

1. The mill and the engine house can be placed each in its most convenient situation without any regard to the relative positions.
2. The internal arrangements of the mill as regards shafting, gearing, belt, and rope drives, etc., are greatly simplified and their costs reduced. The flexibility as regards extensions is, of course, obvious.
3. The grouping of the machines is much less arbitrary than in a mechanically driven mill, as the motors and the comparatively light shafting required by them can be placed where most convenient.
4. The reduction of the chance of a breakdown, which would stop the whole mill, to a minimum.

5. The ease of running one section for overtime or on special work.
6. The reduction of the maintenance and depreciation charges.
7. The greater steadiness of drive which can be obtained under suitable conditions, with a subsequent permissible higher speed and increased output.
8. The reduction in the total capital cost of the mill per spindle or per loom with a factory of above a given size.
9. The possibility of keeping a constant check upon the results obtained in each department of the factory.

In addition to these advantages it may now be claimed that a better price can be obtained for the product of an electrically driven factory than for that of a mechanically driven one, and as regards this point I have definite information that this statement is correct, though I am not at liberty to give the exact figures or to mention any names.

It is also a fact that Clause 7 in the above list has now been proved conclusively, but in this instance also the manufacturers display considerable disinclination to allow the publication of definite figures, though some of them are prepared to admit that they would continue electrical driving even at a greatly increased cost on account of the much better results which are being obtained. It may be mentioned, however, in the case of one large installation which has recently started work that the management in making their calculations admitted that from the evidence before them they were certain to obtain an increase of output when the mill was being driven electrically, and they took this fact into their calculations.

The objections referred to previously are as follows, and so far as I am aware no further serious points have been raised by the opposition side.

The first objection is that the reliability of electrical driving has not been sufficiently proved.

Secondly, that the capital expenditure involved in the adoption of the electrical system is so great in comparison with mechanical driving as to put it out of court.

Thirdly, that it is only advantageous in special cases, or where the average load factor is poor.

Fourthly, that the efficiency of a mechanical drive is considerably higher than that of an electrical one.

The first objection may now be regarded as finally demolished, as of recent times I have never found that even the most bigoted objectors tried to contend that electrical machinery was not reliable in operation, and when it is considered that at the present time 3-phase motors are working with perfect satisfaction under the very severe conditions of mining and rolling mill work, it is fairly obvious that the com-

paratively light duty of a textile factory presents no difficulty whatever.

The second objection, which is almost answered by the eighth clause of advantages, may now be fairly definitely withdrawn from the list. The capital expenditure is, of course, high when a small factory is being considered, but with a large spinning mill of, say, 100,000 spindles, after making allowance for all the structural alterations which can be effected, there is absolutely no increased capital expenditure, and if the factory increases in size beyond this limit the electrical equipment will work out more cheaply than the mechanical. The important items, of course, are the total abolition of the rope race, which under ordinary conditions is an expensive structure, and the reduced cost of gearing.* There is one example of a factory in this neighbourhood which is electrically driven, but where the rope race was built in order to allow of mechanical driving if the electrical proved unsatisfactory; this course is not likely to be followed in the future.

The fourth objection is still brought forward with commendable regularity, and it is in the nature of the case a very difficult thing to disprove. The actual efficiency of an electrical system of driving can be ascertained easily, but the efficiency of a mechanical system under running conditions it is impossible to obtain accurately. It may, however, be of interest to note that where a mill has been converted from a mechanical drive to an electrical one, the I.H.P. of the prime movers has been reduced with the same machinery running, and this certainly tends to show that the efficiency of mechanical drives is not usually as great as stated by its advocates. The probability is, however, that as between a well laid out electrical drive and a perfectly modern mechanical drive there is very little difference indeed in the actual efficiency of transmission—that is to say, as regards the total power delivered to the machines by the driving mechanism. This does not, however, dispose of the fact that the motors deliver the power in a rather better manner—that is, at a more constant speed.

It is, however, hardly necessary before an Institution such as this to labour these points unnecessarily, and the subjects which seem to me to be most worthy of discussion are these:—

First, the advantages and disadvantages of grouped and individual driving for the various classes of textile machines;

Secondly, the advantages and disadvantages of squirrel-cage and slip-ring motors respectively; and

Thirdly, the consideration of the best type of prime-mover, where the factory has its own power plant.

Arising out of this latter point is the consideration of the advantages of the centralisation of the power plant for a number of factories, which was a subject briefly referred to in the previous paper.

Three years ago it was stated that for the preparation machinery,

* See Schedule I., p. 167.

including bale breakers, blowing machines, scutchers, cards, combers, roving and intermediate frames, group driving was the correct course to pursue, and when considerations of the capital cost are taken into account, there is still no doubt that this is the best practice.

It was also then stated that for mule driving the grouping of the machines was essential, and this statement held good until very recently. My own experience certainly points strongly to the advantages of a group drive for this class of machine, but recently satisfactory results have been obtained from a motor driving a single mule with a suitable arrangement of flywheel effect in the system. It would seem doubtful even yet whether as good results can be obtained from this as from a group drive, although the users appear to be perfectly satisfied; but it is certainly worthy of interest to note that successful results have been obtained at all, considering the exceedingly variable load that a single mule spinning frame gives.

The keenest argument over group or individual driving has arisen in connection with ring-spinning and ring-doubling frames. These machines individually give a practically constant load, and it is a very simple matter to couple directly a motor of, say, from 5 to 10 H.P., according to the size of the frame, to the machine and thus do away with all possible slip or transmission losses. It is, however, to be remembered that in a number of factories the ring frames are not always spinning the same class of counts, and, in consequence, a speed variation of the spindles becomes necessary. To effect this variation, in a large number of cases slip-ring machines with wound rotors have been installed, and the variation is obtained by inserting resistance in the rotor circuit.

There are three objections to this arrangement, which is, of course, in other respects a most convenient one, the first being the initial capital outlay upon the installation; the second, the low efficiency of the motors when running with the resistance in circuit; and, thirdly, the objection which has several times been expressed to the heating of the resistances, which has sometimes been the cause of considerable annoyance. Textile manufacturers are generally apprehensive of the heating of resistances to a temperature which, from an electrical standpoint, is perfectly safe, but which they regard as dangerous; and as the ordinary factory is always at a fairly high temperature, it is difficult to get efficient cooling without building rheostats of abnormal size. It would therefore appear as if in future installations of this character special cooling arrangements would have to be considered.

In cases where individual driving is adopted, but where speed variation on the spindles is not necessary, squirrel-cage motors with some form of a friction clutch as a coupling have frequently been used, and have given satisfactory results. The capital outlay on these machines, including the clutches and auto-transformer starters, is distinctly less than upon the wound-rotor machines with regulating resistances, even although in this case the clutch coupling is not necessary.

The alternative arrangement for the driving of frames where variable speed is essential is to group the machines and drive with a single motor, changing the pulleys for speed variation, as is customarily done with the mechanical arrangement.

Arising from this question of the method of driving is the possibility of direct coupling the motors to the line shafts in nearly all cases for group driving, and this course of action has now been found to be possible to a greater extent than the manufacturers were at one time prepared to admit.

Having become accustomed to certain shaft and pulley speeds as being on the whole the most convenient for a mechanical transmission, considerable difficulty was found in inducing them to depart from what they regarded as a standard arrangement; but it is now, in nearly all cases, found possible to speed line shafts up to an extent that will permit of direct coupling to standard speed induction motors without any objectionable results arising. This arrangement materially reduces the transmission losses between the prime-movers and the textile machines, and also eliminates some possibilities of slip.

In several installations in this district, however, the mechanical arrangements, and, in fact, the placing of the motors generally, do not appear to have been given sufficient consideration; and the assumption seems to have been that, provided one is using motors, the method of fixing and the arrangements of the drives from them were of small importance. This has naturally led to a certain amount of dissatisfaction. These cases, however, when contrasted with the installations which have given exceedingly good results in other factories, merely emphasise the necessity for the careful consideration of the actual requirements of the machinery to be driven before the work is carried out.

In other cases those responsible for the lay-out of the installation appear to have overlooked the fact that it is almost as inadvisable to have the motors too large as too small, and this is a fault which exists in some recent instances.

Electrical manufacturers generally do not appear to realise that the actual powers required need very careful investigation before the machines are actually installed, and it is by no means a safe rule to assume that the horse-power is so much per one hundred spindles of the mill, as this may lead to considerable error. If a large mill engine is being put down to drive the whole of the plant, it may be perfectly safe to use such general assumptions; but when what is practically equivalent to a subdivision of the prime-mover is made, the most careful consideration of the power taken by each section of the installation is essential.

In considering this it is necessary to know the class of material which is to be produced, as figures from one mill only would be very misleading applied to another mill with the same class of machines but manufacturing a different grade of material.

Regarding the question of the prime-mover, if the mill has its own installation, one approaches a subject which is open to practically endless discussion. Installations either in operation or approaching

completion comprise standard mill engines driving generators through ropes, mill engines direct coupled to generators, quick-revolution generating sets, and turbo-generator sets. It is only needful for a gas-driven set to be installed to have complete examples of almost every type of prime-mover which is possible in this country.

From the point of view of perfect angular velocity the turbine offers obvious advantages, and, in addition to this, for large sizes the capital cost per kilowatt is fairly reasonable.

Mill-owners generally, however, seem to have a prejudice against steam turbines, apparently on account of one or two unfortunate experiences; but these do not appear to justify the sweeping condemnation sometimes indulged in. At the same time, it does appear to be a fact that a steam turbine is not altogether as reliable a machine as a reciprocating engine, and recent practice seems to be in favour of installing two turbines, either of which is of sufficient size to run the mill by utilising to the full its overload capacity.

My own preference for a mill of, say, less than 1,000 H.P., would certainly be in favour of a quick-revolution reciprocating set, but the slow-speed engine makers are now prepared to guarantee such favourable results in steam consumption that in spite of the extra capital cost their claims demand careful consideration. There is, of course, the added advantage that the average mill engineer is more familiar with the slow-speed engine than any other type, and under ordinary conditions repairs and maintenance will be a small item.

There is, however, ample room for a most instructive discussion upon this part of the subject, and I trust that some of the members present will be prepared to express their views.

It must be remembered that the ordinary mill engine runs about 55 or 56 hours per week under practically full load, and that it is usually of 700 to 1,500 H.P. in size. It is manifest also that with the distinct advances which are being made in gas-engine design gas-driven generating sets will in the future demand consideration. Particularly is this the case as the makers are now prepared to guarantee very constant angular velocity, which in the past has been one of the principal drawbacks of gas engines for the class of work under discussion, where constant speed is of the utmost importance.

It might be mentioned, in passing, that in the case of one mill supplied from a central station the mill authorities stated they were able to tell by the behaviour of the machinery when they were being run off the steam turbines and when off the reciprocating engines, and very much preferred the former.

The other point which I would suggest as one that might be usefully discussed is that of power supply from a central generating station. Of the total horse-power in motors now installed in textile factories in this country some 25 per cent. to 30 per cent. are driven from central generating plants, and this proportion shows every sign of increasing rapidly.

As a matter of fact, upon the occasion of reading the previous paper

I did not feel that the central supply authorities would be able to induce proprietors of spinning mills to take a supply at a price much above 0·35d. per unit in the case of large mills in the Lancashire district, where coal is cheap. This, however, is merely another case showing the unwisdom of prophecy, as it is common knowledge that some large factories are now paying considerably above this price, and are perfectly satisfied so to do; and, as regards mill-power costs, I would refer you to Schedule II.

The advantages of taking a supply of energy, which were fairly fully discussed before, hold good in an intensified degree to-day. In the first place, if the mill-owners make a contract for power at a fixed price they become largely independent of the fluctuations in the price of fuel and also of strikes; they are freed almost entirely from all anxiety as regards stoppage from breakdown, and, in addition, the capital expenditure saved on the generating plant can be very much more usefully employed in productive machinery. This is a matter of very great importance in times like the present, when mill profits have been very satisfactory and every spindle that could be run has been in useful employment.

Mill-owners are coming more and more largely to recognise that a supply from a central authority is a good thing from their point of view. In the case of a new mill, they save a large amount of capital expenditure; in the case of an old mill, whose power plant requires overhaul, they get all the advantages of an electrical drive without any further large capital expenditure.

There is therefore little doubt that developments along this line are bound to be fairly considerable, and it would seem to be likely that eventually the generating stations of nearly all the cotton manufacturing towns of Lancashire will have a considerable demand from the textile factories within their areas.

In negotiating for a supply of power with the mill authorities it is necessary to remind them that when due allowance is made for interest, depreciation, maintenance, rates, insurance, etc., the actual power cost in Lancashire, where coal is fairly cheap, is from three to three and a half times the fuel bill. The example given in Schedule II. showing how this works out may be of interest.

A number of the mill-owners would, of course, contend that 7 per cent. depreciation is too high an amount to allow, but when times are good they usually allow more than this themselves, though when trade is not satisfactory depreciation amounts are much lower.

I regret that there is a considerable absence of definite figures, which it would be desirable to give in a paper of this character, but it will be understood that while individual manufacturers are prepared to give figures in confidence, they object to having them generally disclosed, and although this objection will in some cases cease to exist before long, it is at the present moment impossible to give detailed statements publicly.

There is, however, in this subject scope for a very instructive dis-

SCHEDULE I.

COMPARISON OF CAPITAL OUTLAY FOR A SPINNING MILL OF 100,000 MULE SPINDLES.

Driven: (*a*) By Ordinary Mechanical Means.
(*b*) By Three-phase Electrical Transmission.

(*a*) MECHANICAL.				(*b*) ELECTRICAL.			
	£	s.	d.		£	s.	d.
Engines, piping, etc., erected	6,000	0	0	850-k.w. turbo-generator and auxiliary comp....	6,600	0	0
Boilers	2,000	0	0	Boilers	2,000	0	0
Chimney and boiler seatings	1,000	0	0	Chimney and boiler seatings	1,000	0	0
Reservoir	1,500	0	0	Reservoir	1,500	0	0
Engine house	600	0	0	Engine house	400	0	0
Engine bed	600	0	0	Engine bed	300	0	0
Rope race	500	0	0	Motors, cables, and switchboard	2,982	0	0
Boiler house	500	0	0	Boiler house	500	0	0
Economiser and house	750	0	0	Economiser and house	750	0	0
Lighting generator and seating	1,200	0	0				
	£14,650	0	0		£16,032	0	0
Gearing	3,000	0	0	Gearing, £550; pulleys, £250	800	0	0
Total	£17,650	0	0	Total	£16,832	0	0

SCHEDULE II.

COMPARISON OF RUNNING COSTS FOR A SPINNING MILL OF 100,000 MULE SPINDLES.

Driven: (*a*) By Ordinary Mechanical Means.
(*b*) By Three-phase Electrical Transmission.

(*a*) MECHANICAL.	£	s.	d.	(*b*) ELECTRICAL.	£	s.	d.
Coal, 1,500 I.H.P.	1,359	7	6	Coal, 1,400 I.H.P.	1,268	15	0
Wages—Engineer, 55s.; assistant, 30s.; 2 stokers, 28s.	352	10	0	Wages (no assistant)	277	10	0
Oil, etc.	125	0	0	Oil, etc.	125	0	0
Insurance—E, £100; B, £22; gear, £60; buildings, £20	202	0	0	Insurance—E, £100; B, £22; gear, £16; buildings, £10	148	0	0
Rates, 7s. on 5 per cent. of £14,650	256	7	6	Rates, 7s. on 5 per cent. of £16,032	280	15	0
Interest and depreciation, 12 per cent. on £17,650...	2,118	0	0	Interest and Depreciation, 12 per cent. on £16,832...	2,019	15	0
Total...	£4,413	5	0	Total	£4,119	15	0

Cost per kilowatt-hour for 850 kilowatts, 56 hours per week and 50 weeks per year.

(*a*) $= 445$d. (*b*) $= 415$d.

NOTE.—Coal cost based on 2 lb. per I.H.P., 7s. 3d. per ton, 56 hours per week, 50 weeks per year.

cussion, and the paper has purposely been shortened in order to allow sufficient time for this. Moreover, the number of controversial points which have been raised should be sufficient to satisfy the most exacting.

In conclusion, I would draw the attention of the electrical manufacturers to the fact that for the expansion of the home trade in electrical machinery they are bound to look to developments in large industries such as the textile.

Supply stations are not being built with the rapidity of previous years, traction work does not offer anything like the demand for machinery that it used to, and for a steady home trade every attention will have to be given to the electrical equipment of factories of all classes. It is therefore absolutely necessary that a subject such as this should be approached as far as possible in the spirit of research, and it should also be recognised that a very great responsibility rests upon those engineers who, through rashness in approaching a subject on which they have not much information, or through carelessness in the laying out of an installation, get electrical driving a bad name. One unsatisfactory installation does more damage than twenty good ones can rectify, and there are bound to be more or less hostile critics who will be only too pleased to expatiate upon any unsatisfactory results that can be pointed to.

There is still a great deal to be learnt as regards the subject, but if due care is taken over the carrying out of future schemes there is little doubt that it will not be many years before every new mill built in Lancashire will be electrically equipped, and it is also equally certain that these mills will be able to make a profit when a good many others cannot do so.

Discussion.

Mr. M. B. Field (Chairman): I think all will agree with me that we have listened to an interesting paper on a very important subject to-night, and our thanks are due to Mr. Wilson for bringing this matter again before us for discussion. The point at issue is whether there can be a great development in the near future in this country in the use of electricity for driving textile factories and what general principles will be most conducive to the best results. Mr. Field.

Mr. G. D. Seaton: I agree that it is very important that all of us connected with the electrical industry should do our best to further the electrical driving of factories, and I am very much pleased to say that there seems a strong prospect that Mr. Wilson's hopes may be realised in the near future, though not exactly in the way that we expected when we discussed this subject three years ago. To my mind, our chief hope of getting cheap current lies in the blatant hypocrisy that is now rampant in the land. It may sound very curious when I state that the great free trading municipalities of Lancashire and Yorkshire are now committed to a thoroughly up-to-date system of dumping, and I am pleased to say that I believe Manchester stands pre-eminent in this respect. Other people have started this policy of dumping, but they Mr. Seaton.

Mr. Seaton. have not been quite so successful, inasmuch as I understand the power companies are getting into their districts as well, and setting up a sort of free trade, but in Manchester so far no power company has appeared nor is one likely to.

Mr. Taite. Mr. C. D. TAITE: I hoped to have had the advantage of hearing some criticisms from several other speakers before making any remarks on this paper of Mr. Wilson's which has been put before us to-night. I would, however, point out that Mr. Wilson has to a very large extent confined his remarks to spinning mills. Personally I think that probably there is a still larger scope for electrical driving in weaving sheds. The difficulty hitherto has been that in weaving sheds the shafting has usually been arranged to run at rather a slow speed, and motors direct coupled to the main shafting have been out of the question. It seems to me that where electrical driving in weaving sheds has been tried, very excellent results have accrued. I know a manufacturer who has two sheds, one of which is driven electrically, while the other is driven by steam, and although it is to his interest in this particular case rather to depreciate the electrical driving, he has given me, as his opinion, that the electrical driving stands far ahead of the steam drive. He states that he gets an increased output, that his cloth is better, that there is better demand for it on 'Change, and that there is reduced upkeep of his looms. I think those are three advantages which are very important and which are worth emphasising. With regard to spinning mills, Mr. Wilson has given a list of nine advantages in favour of electrical driving. In No. 4 he says, "Reduction to a minimum of the chance of breakdown, which would stop the whole mill." Where power is obtained from some exterior source, I thoroughly agree that that is the case, but where a mill-owner puts down a plant of his own for driving his mill, and then converts that power again to mechanical power through motors, I cannot see that this is reducing the risk of breakdown; on the contrary, I should rather say there is an increased risk of stoppage. But where his power is purchased from an exterior source, it is always the case that standby machines are kept ready for every emergency; standby mains, of course, are also provided, and in that way I think Mr. Wilson's claim is established. I do not admit that I am at all an advocate of electrical driving under all circumstances. If a man simply replaced his mill engine by a turbine without any standby at all, I think he is running very great risks. Any one who considers this subject at all, and knows anything about the careful attention which turbine plant requires—and I think that the great majority of cases where mill-owners put down their own plant they have been turbine plants—must agree that the risk in such a case is very great, unless there is a complete standby provided; and if a standby has to be provided, either the plant must run inefficiently, because it will consist of small sets with increased coal consumption, or two big sets must be put down, one of which will take the load, and therefore the capital cost is very largely increased. The solution of the problem—and I do not wish to be thought to be

speaking from a biassed point of view—seems to rest upon the supply of power from an exterior source entirely. As to the advantages which electrical driving offers: Of course, if the power is obtained from an outside source, a very considerable reduction in capital expenditure results, and we believe, and we have every reason to believe, that there is a very considerably increased output from the mill. I have been working out what an increase of 5 per cent. in the output from a mill really means to the mill-owner, and if my calculations are correct, 5 per cent. increase would mean 1½ per cent. on the total capital employed. If one takes a mill of 80,000 or 100,000 spindles, with a capital of £120,000, that is very nearly £2,000 additional profit, or if the additional profit is divided between the supply authority and the mill-owner, it would be about £1,000 each—that is, a millowner can pay £1,000 more for his supply from outside, and yet net £1,000 additional profit over what he would get if he had a slow-speed engine. With regard to the purely electrical point of view: The company with which I am connected have both examples of individual drive and collective drive, and at the present time I do not know that I am in a position to say which our consumers appreciate the most; those who have the collective drive are very much pleased with it, and those who have the individual drive are the same. With regard to squirrel-cage and wound rotors, we employ wherever we possibly can the squirrel-cage machines, and the biggest machines of this type that we have on our mains at the present time are 100 H.P. We have no difficulty in starting up with an auto-transformer, and they give very excellent results. There is one point in Mr. Wilson's schedule to which I take exception. He makes his calculations as regards coal consumption on the basis of 2 lbs. of coal per I.H.P. at 7s. 3d. per ton. I do not think this can really be substantiated. If the price is taken at 7s. 3d. per ton, then a greater weight of coal will be required, and of course at the present time there are very few people who can buy coal at that price. I think really if one put the coal cost at about 2½ lbs. per I.H.P. and about 8s. 6d. per ton, that would be nearer what the average mill has to pay, having due regard to the fact that the fires have to be banked about 112 hours per week. Mr. Taite.

Mr. W. B. Woodhouse: I am pleased to see that the three years that have passed since Mr. Wilson's previous paper have brought him more into line with the public supply people. Mr. Wilson thought then that power could be produced in an isolated plant much more cheaply than now, even though in those days he took his capital charges on the plant at 15 per cent. for interest and depreciation, and now he reduces them to 12 per cent. He points out the very large use of electric power, principally in cotton mills. In the Yorkshire woollen mills, although there is something like 240,000 H.P. of textile machinery running, yet they are rather slow to move. The Yorkshire Power Company has something like twelve mills on its mains and contracts pending for a good deal more. The Bradford Corporation are launching out in a public supply to textile mills, and have just made a big contract with Mr. Woodhouse.

Mr. Woodhouse.

the Bradford Dyers' Association, who use something like three-fifths of their coal for steam raising, apart from power, and yet find it advantageous to take a public supply. The Huddersfield Corporation are doing the same, and have just made a contract for 1,000 H.P., and have put down steam turbines, and are going into the power supply business. In fact, I think there is no doubt at all that the advantage of power supply from a central public authority is pretty well established. I am glad to note that Mr. Wilson thinks with me on that point. With regard to 3-phase being the best for driving textile machinery, and direct current being not quite the right thing, I have gone into the question of driving textile machinery with direct-current motors. It is quite simple to design a shunt motor to run at constant speed at all loads, providing that load does not vary rapidly; but when the load does vary rapidly, most distressing results occur in the shape of speed variation. In an extreme case I met with a little while ago they had an installation of their own, with direct-current plant driven from their own engine, and direct-current motors. The speed variation on the mule countershaft ran up to 30 per cent., although the speed variation on the main engine was only 3 per cent. We changed that drive over to mechanical transmission from the engine, and the speed variation was reduced to 18 per cent.—that is, mechanical driving gave a better result than the direct-current motor. With a 3-phase induction motor the variation is reduced to between 3 and 4 per cent. The fact of the matter is, with the shunt motor the conditions which determine a particular speed do not all act immediately. There is the drop in the windings, and there is the armature reaction giving a compensating effect. This action takes some little time to come into play, and although it may be a very excellent shunt motor for a steady load, yet for rapidly varying loads, like looms or mules, a direct-current motor is most unsatisfactory. One is forced to conclude that the induction motor is the right thing for practically all textile machinery. There is, however, a field open for a motor which will vary the speed of spinning frames as the cop is being wound. The diameter on which the thread is wound is varied, varying as the layers are conical. The speed on the wind can be increased as one travels towards the centre and decreased as one travels outwards, and in such a case as that it seems to me highly probable that the single-phase motor with an arrangement for varying the speed as the bar which guides the thread on the cop rises and falls, would be most successful. We are hoping to try it in Yorkshire on spinning worsted, and we expect to get a very considerable increase in output from the spinning frames; the output is, of course, everything. Mr. Taite mentioned the vital importance of output. If machines can be kept running to the top speed, then one gets a bigger profit on one's capital. It is all a matter, from the mill-owner's point of view, of what return he can get from his capital. I should like to quote a few cases of mills taking a supply from the Yorkshire Power Company. I was talking to one mill-owner, who has had part of his mill converted to electrical driving, namely, the weaving shed. He says they are quite

Mr. Woodhouse.

the best running set of looms he has got in his mill, and produce more than the mechanically driven looms. I said, "Will you please write to the newspapers and say so?" He said "No, I won't; I am not going to tell my competitors what a good thing it is." Another case of a worsted spinning mill. In their carding department they had carding machines that used to card a pack of wool per machine per day. They increased the output to a pack and a quarter, or a 25 per cent. increase. In a woollen mill they increased the output of the carding machines by an amount which paid the cost of the power. Roughly, it took 2d. an hour to drive each set; the extra output due to the electric drive gave an extra profit of 2d. per hour, and they thus get their power for nothing. In the same mill they converted some mules. In woollen mills the usual practice is to drive two, three, or four in a group, but usually two. They converted about half their mules to electric driving, and they left the other half on the steam drive, and the men who were looking after the mules on the steam drive formed a deputation and complained to the mill-owner. They had a lot more broken ends and more work to do than the people who had got the electric drive. They got what they wanted, and that mill is now equipped completely and running, every part of the mill being electrically driven.

Mr. Wilson mentions the comparative horse-power of mechanically and electrically driven mills; that is, of course, a very interesting point, and a very difficult one to get at. My own experience is that the horse-power is not reduced by converting to electrical driving, but the output is increased, and so for the same horse-power more work is done, and that is the important thing. We try to deal with our mill-owning friends, not from the point of estimating the I.H.P. or B.H.P., but how many units it takes to drive a certain amount of machinery, that is the real point. The results of driving various mills come out pretty closely, and we can say now with considerable accuracy just how many units per annum it will take to drive certain machinery. Mr. Wilson, in talking of the equipment of mills, raises another point, which seems to me a most important one—that is, the very elementary point of millwrighting. A good mechanical drive can be spoilt, and so may a drive from a good electrical supply, by bad millwrighting. It is not always the fault of the engineers; it is very frequently the fault of the mill-owners, who have got prejudices in favour of running their shafting at speeds at which their grandfathers ran them. I have seen cases of electrical driving where one big motor has been put on to the old rope race, and they supposed they were testing electrical driving fairly. The first element of successful conversion is that the millwrighting should be done on good, sound, up-to-date lines. As to the question of cost, I am glad to see that Mr. Wilson's views as to the likely cost of an isolated plant are rather higher than they were before. I think that in estimating running costs the capital charge he allows is low. It seems to me that a very strong point indeed can be made of the productive use of capital. If instead of putting the capital into an

Mr. Woodhouse. engine, boiler, chimney, etc., the mill-owner spends that capital on actual productive machinery, spinning frames, or looms, then for the same capital expenditure he is going to get a considerably larger output, and presuming his standing charges are constant, he will also get a considerably higher profit. Therefore even an allowance of 15 per cent. for interest and depreciation on generating plant is quite insufficient, for if he employs his capital on productive machinery, the figure is something much more like 30 per cent.

Dr. Bowman. Dr. F. H. BOWMAN: This is a subject to which I have given a great deal of attention. I have all the information that it was possible to get privately, but some I obtained only on the condition that I would not make it public. Now, I do not think there is any doubt whatever that if electricity could be supplied at one-third of a 1d. per unit, it would not pay to put down one's own power-plant, but unless it is delivered into the mill at that price, direct driving will be cheaper, and it will pay to put down a plant if the power is, say, over 250 B.H.P. There is a mistake with regard to the example of cost given. I only wish that somebody would give me one or two orders for a mill at the same figure, as I would soon make a big fortune. I could put down the whole mechanical power of 100,000 mule spindles, and I can save from £3,000 to £4,000 on what is given for a mechanical drive, because I have estimates in my possession for a mill of about that size, and it is much under that shown. Then, when I look at the electrical side, I find that if a turbo-generator is put down it would certainly be necessary to put down a double set, and one that will enable the whole or the greater part of the power to be taken. As one generator will generally be standing, there will be a great increase in the capital charge. Some people imagine that mill engines in large cotton mills and worsted mills are always breaking down. I have had mills running, and in five years I do not think we ever stopped two minutes in consequence of anything going wrong with them. They are so simple that there is nothing to go wrong, and a reserve engine is not required, which must always be taken into consideration. My electrical friends say, "See what safety you have; you cannot have a breakdown if you are on a public supply"; but some of us know that occasionally when we want to get home at night there is not a tramcar running. I have made the most careful calculations for a large mill when none of the machinery was running, when it was simply running on the loose pulleys, and have obtained the friction diagram. I am not referring to an old mill, but to a new one. In the case of a rope-driven cotton mill with the speeds of the shafts sufficiently high we calculated the friction, and took the indicated-horse-power with no machinery on, and made a comparison with an electrically driven mill, where we were told what the friction was, as judged by the output of the electricity. We took out the friction diagram as well as we could, so as to bring both to the same common denominator, and the direct-driven plant showed 3½ per cent. less friction than the electrically driven plant. I then went to a turbine and found that the direct-driven plant was 9 per cent. better

than it was with the turbine and electric driving. I have worked as an operative in a worsted mill as well as in a cotton mill, and I can judge perfectly well when a machine is doing its duty. In one place where there was a motor fixed to a ring spinning frame we took the output of the frame and compared it with that of the frame next to it, which was driven by a belt; the output from the electrically driven frame was about 6 per cent. higher, and we were all delighted with it. I pointed out that the driving was by means of straps, and suggested the use of a rope instead, to see what the result would be. A rope was accordingly put on, and we obtained 2 per cent. more work than we did out of the electrical drive. In most mills the straps are driven straight down, and there is always much loss, but with a rope-drive, arranged with a pulley at the other end of the frame furthest from the shaft, so that the rope is perfectly slack, and using two pulleys, fast and loose (not a friction clutch), the difference between running with the strap and running with the rope is very marked indeed. I thoroughly agree that in the case of old mills, where there are small rooms, the electrical drive is of great advantage to the owner, but where there are small shafts running straight down a mill at the proper high speed, and in a modern cotton mill with the machinery properly arranged and the heavy machinery close to the engine, I am certain that there would be no gain in substituting an electrical drive for a rope drive, if one had to provide one's own power, but if power is obtainable at one-third of a 1d. per unit, delivered into the mill, the electrical drive would be more advantageous, as the capital required for the prime-mover plant would be eliminated. Dr. Bowman.

Mr. E. L. Hill: With regard to the 28,000 H.P. for textile mills, this means we should be driving 3,000,000 spindles, which is something like 6 per cent. of the spindles in Lancashire, and seeing that that has been done in two and a half years' work, I think electrical engineers can congratulate themselves on that result. Of course the question of cost is the chief thing in determining whether a mill will go in for electrical driving or not. If we take the case of a mill of 1,300 H.P. with 100,000 spindles, the average load would be 880 or 900 k.w., the output will be about 2½ million units per year, and I think if we drive that with turbines with a steam consumption of, say, 18·5 or 19 lbs. per kilowatt, the works cost should not exceed 0·175d. per unit. If to that is added 13 per cent. on capital cost, that is to say 5 per cent. for interest, 5 per cent. depreciation, 2 per cent. accident insurance, and 1 per cent. repairs, we shall find that cost of 0·175d. is raised to 0·3 or 0·325d. per unit, and I therefore do not think it will pay a mill to take power from an outside source unless they can get it at something below ½d. per unit. The difference between 0·325d. and 0·45d. per unit with an output of 2½ million units per annum will be roughly £1,300. If the money, instead of being spent on a generating station, is put into productive machinery, then I think there is no doubt that the mill-owner would be wiser in buying his current Mr. Hill.

Mr. Hill. from a supply authority, but only if he does not pay more than ½d. per unit.

Mr. Mallinson. Mr. A. B. MALLINSON: I wish to take exception to some of the figures given in the schedules. Turning to Schedule I., why is the mechanical drive handicapped by having a lighting generator at £1,200? Does that include wiring? If so, why is it not on the electrical side? Turning to the electrical items, the same amount is allowed for the reservoir with the electrical as for that with the mechanical drive, whereas the extra 1½ to 2½ in. vacuum required for maximum efficiency with the turbo-generator would necessitate either a much bigger cooling reservoir or a cooling tower. Coming to Schedule II., it seems rather strange, after Mr. Wilson's admission that the efficiency of an electrical drive can at the best only be the same as the efficiency of a mechanical drive, that the coal for the electrical side is 1,400 I.H.P. compared with 1,500 I.H.P. for the mechanical drive. He says wages (no assistant) for electrical drive; I am afraid Mr. Wilson will get no cotton-mill engineer to run an electrically driven mill with all the motors, etc., without an assistant. Probably two more will be required. For the insurance of gear £60 is allowed in the case of the mechanical drive, and £16 for electrical drive. What is going to be put in for the insurance of the motors? How is it that the buildings only require half the insurance with electrical as with a mechanical drive? I think, if things are balanced out, it will be found that the advantages of the electrical drive do not come out as favourably as they are shown in the paper.

Mr. Blackmore. Mr. D. R. BLACKMORE: With reference to the author's statement that the question of reliability was sufficiently proved, I am afraid that this cannot be said in every case, owing to the fact that some users have unfortunately been troubled with badly designed plant and inefficient gearing, and great care must be taken in the design of textile mill plants to consider each scheme very carefully on its own merits, and err on the safe side in every case.

From my own experience I differ from the author with regard to his comparison between a rolling-mill motor and one installed in a textile mill, as I consider the latter has the most difficult duty to perform owing to the high initial temperature of the room in which it is installed, namely, about 85°.

The full-load temperature of such motors would approximate 165° Fahr., and owing to the humidity of the atmosphere in the spinning-room when running at high temperatures I have found that insulation troubles have occurred unless special precautions be taken to provide against such chance of breakdown.

I think electrical engineers have taken too much to their own credit up to the present time for the advantages of electricity in cotton mills, as, in my opinion, in order to get any increase of production compared with an up-to-date mechanically driven mill we must use a steam turbine as the prime mover, and electric driving is the only practical method of getting the resultant even turning moment of the turbine.

on to the shafts driving the various machines. The author refers to a certain case where the customer knows whether a reciprocating engine or turbine was on the load at certain times, and I think this must refer to my own system, as I have such a case where we are driving mules, spinning fine counts. Mr. Blackmore.

Some doubt seems to have arisen as to the reliability of turbine plant, and the question of installing two complete units is suggested where a mill generates its own current. In my opinion this is wrong and shows weakness, and should this advice be continued we shall find that our textile manufacturers will fight shy of electricity on the score of lack of reliability alone. I may mention that owing to the carelessness of a fitter a piece of steel about 6 in. long stripped sixteen rows of high-pressure blades from one of our turbines, but the set was kept on load during that day, and at night the casting was opened up, the fragments removed, and the set again on load the following morning.

In the Stalybridge district electricity is gaining ground very fast, and we shall shortly have 6,000 H.P. connected for textile work.

I think that some of the author's figures in the schedules are open to criticism. For instance in Schedule I. the amount for motors, cables, etc., is very low, and in Schedule II. the indicated-horse-power of the electrically driven mill will certainly not be less than that of the mechanical. It will also be necessary to have quite as much spent in wages in either case.

Referring to the question of individual *versus* group driving, I am strongly in favour of the group system for textile mills, especially for mule driving, and with the exception of very rare and special cases, group driving will be found to be the only one commercially feasible.

A comparison of squirrel-cage and slip-ring motors has been made, and I think that squirrel-cage motors up to 150 H.P. are quite reliable and suitable for textile work as they have so many advantages over those of the slip-ring type.

With regard to production I do not think there is any doubt that with good design and a turbine as prime mover an increase of 5 per cent. can be assured, and some of my own consumers are prepared to admit this.

Mr. S. L. PEARCE: I do not altogether agree with the last speaker (Mr. Blackmore). I do not go quite so far as he does where textile plants are concerned in respect to condemning every type of engine in a central station except turbines. In connection with these textile power loads I am quite aware, of course, that in dealing with a cross-compound engine an even turning moment is not obtained, and that, therefore, a turbine is the best prime mover to put in. But I think very much better results will be obtained by utilising multiple crank engines. As regards turning moment, the Moscrop recording diagram that we have at our station shows that the speed, variation, and therefore the frequency of the system, is well within half of 1 per cent. Now that is, I think, a very good record. I am prepared to admit that it may be somewhat improved on by using turbines. Mr. Wilson refers Mr. Pearce

Mr. Pearce. on the opening page of his paper to the fact that no very great progress has been made in this part of the country with electrical driving in textile factories. Now, it seems to me that he has hardly stated the case quite fairly. I do not think we, as electrical engineers, can expect to take on mills which involve the replacement of fairly modern steam plants that have only been installed a matter of four or five years. I do not see how mill-owners can be expected to throw those plants out and go in for electrical driving. Again, there are cases where steam engines are giving good results, which have been at work a good number of years, in which the capital cost has been largely written down and stands at a very low figure in their books, and there, again, I do not quite see how they can be expected while they are getting fairly good results to put down capital for brand-new electrical plant. These reasons, I think, explain to a very great extent why progress has been somewhat slow in Lancashire. In the United States the large increase in textile loads is probably due to the fact that they are new mills. Mr. Wilson refers on page 161 to a case in which the management of a certain mill has taken into consideration that an increased output would be obtained by electrical driving. I know of another similar case, the mill being on the Corporation mains, where a figure of 5 per cent. was allowed in making up their comparisons. But that 5 per cent. did not represent anything like the figure that Mr. Taite has worked out; still it is a substantial item to go to the credit of the electrical side. With respect to the efficiency of the various systems, I think if we take it on the basis of the power delivered on the line shafting, there is not very much difference between an ordinary steam-driven mill and an electrically driven mill employing their own plant. But the case is different taking the supply from a public power company, because in that case the efficiency of the engine or the turbine is eliminated. Where power is measured on the low-tension side of the transformers we simply have losses in cables and motors, and in that case the power delivered to the line shafting shows a loss of about 10 per cent. This is a particularly substantial saving over either of the alternative schemes. It appears that the real question that has to be answered is whether it is to the advantage of the mill-owner to have their own steam-driven plant or whether they should take a supply from a public company. I think the cases where it will pay them to put in their own electrically driven plant must be very few and far between nowadays. Dealing with Schedule I., I agree to a very great extent with all that Dr. Bowman has said as to the costs on the mechanical side being over-rated. Mr. Wilson's figures work out to about £15 per horse-power. I am not competent, perhaps, to express an opinion on that point, but I do know, having had access to a good many costs, that the figure of £9 or £10 would be very much nearer the mark. Applying the figure of £10 to Schedule II., it reduces the interest and depreciation charges, and brings down on the mechanical side the cost to 0·4d. as against 0·415d. on the electrical side. Taking the electrical side, I would endorse the last speaker's remarks

that the capital expenditure on motors, cables, and switchgear seems to be altogether too low, and I think that figure should be increased. With regard to the question of cheap rates from a supply company, various speakers have dealt with that point. This question of power rates has been very well considered, I may say, in this district, and there is no doubt that the rates that are being quoted to-day about here are, I think, such as will very favourably compare with any costs obtained for mechanically driven mills, and are, moreover, prices that pay the supply company. I have no hesitation in saying that I think there is a certain duty resting on the large supply undertakings, whether they be municipal or otherwise. It is their duty to frame their prices, whilst safeguarding their own interests, in such a way that a reasonable opportunity is given to the various industrial works, whether textile or not, to take advantage of electrical driving, and in so doing we help to a very great extent the manufacturing industry of the country. Mr. Pearce.

Mr. J. S. Colquhoun: I myself have within the last fortnight tested one of the latest up-to-date direct-driven mill plants in the Oldham district. This is a triple-expansion Corliss engine, and it is working with 150° superheat at about 26 in. vacuum, and after very careful tests the coal consumption, including 5 tons weekly for heating the mill, which is a considerable item, came out at 1·6 lbs. per I.H.P. The coal used was Yorkshire slack, costing 8s. 6d. per ton delivered at the mill. This is a very excellent figure, and it is an example of what can be done with up-to-date plant properly installed. Another important point is that in a mill district like Oldham, water is very scarce, and there is no doubt that if a turbine plant was put down in this district, the cost of getting the amount of water required for the condenser would be a very serious item. The temperature of the water at this mill where this trial was made was 103°, and the vacuum obtained was 25 in., so, really, if a turbine plant was put down using the same cooling water the coal consumption would be very large on account of the low vacuum obtained. With regard to turning moment, Mr. Pearce mentions that with a multiple crank engine a very good turning moment is obtained. In this particular case a Moscrop recorder is driven from the engine-shaft, and the records taken quite agree with his figure of half of 1 per cent. I cannot see, therefore, that there would be any gain in driving with turbo-generators under the foregoing conditions. Mr. Colquhoun.

Mr. J. Purrett: Probably Mr. Wilson has never had to put motors into an old mill. He says motors have often been fixed without sufficient consideration as to placing. The difficulty is usually to find a place to put them in at all, as the mill-owner will often make it a condition that the existing arrangement must not be interfered with. Referring generally to the paper, it seems to me that one has to take every case upon its merits, because what applies to a new mill does not apply to converting old mills, and there is certainly a great deal of work to be done in converting the old mills, even more per- Mr. Purrett.

Mr. Purrett. haps than in equipping new mills at the present time. There is one other point, where Mr. Wilson says an induction motor, either squirrel-cage or slip-ring, made by one manufacturer, does not differ very materially from that made by another. I do not agree with him there. Manufacturers certainly will give the same guarantees, and apparently one is able to get as good a machine from one firm as from another, but the design of the motor very often makes all the difference to the result of the electrical equipment. I can mention a case in point. Quite recently I heard of a motor which was installed to develop 40 H.P., and this motor actually indicated 43 H.P. when set to work. It was therefore assumed to be overloaded and taken out. Another motor was put in of a different make to do exactly the same work. The new motor was, I think, 52 H.P., but actually on the same ammeter and under exactly the same conditions of working the new motor indicated something under 40 H.P., yet the guaranteed efficiency of the two motors was within 1 per cent. of each other. I think it is certainly a thing that should be considered by those who have to deal with these matters to put in the very best motors and equipment possible, otherwise it often means that people are disappointed with the results obtained, and they condemn electrical driving generally.

BIRMINGHAM LOCAL SECTION.

THE TORQUE CONDITIONS IN ALTERNATE-CURRENT MOTORS.

By VAL. A. FYNN, Member.

(*Paper received June* 3, 1907, *and read at* BIRMINGHAM, *December* 11, 1907.)

It is proposed in this contribution to deal with the torque conditions obtaining in some of the most important types of single and polyphase commutator and squirrel-cage or slip-ring motors, in a manner which will bring out their several characteristic features, make it possible to compare the various types on a fair basis, and enable us to form an opinion as to the relative degree in which the active material is, or can be, utilised in the several types, *i.e.*, as to their relative weight efficiency as distinct from their conversion or transformation efficiency, which is simply the ratio of output to input. This question of weight efficiency is of paramount importance in the case of traction motors. It will be necessary carefully to distinguish in many cases between the torque conditions obtaining at starting and those prevailing under speed.

(*a*) *The Self-excited, partly Compensated, Single-phase, Shunt Induction Motor* (Fig. 1).—This type will be considered first, as it is thought that once the principles underlying the operation of this machine are thoroughly understood, it becomes a very easy matter to arrive quickly at the main features governing the performance of other alternate-current motors. It will be sufficient for our present purposes simply to outline the manner in which this machine operates; a fuller discussion of its theory has already been given by the author in other publications.

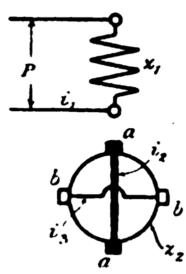


FIG. 1.

The motor in question is shown diagrammatically in Fig. 1. To begin with, there can be no doubt that the machine, along its axis $a\,a$, is nothing but a transformer, and when the rotor is not revolving it behaves exactly like a short-circuited transformer. Let P be the potential difference at the terminals of the stator or primary winding having z_1 turns, and let i_1 represent the current flowing through that winding. Now i_1 can be looked upon as the vector sum of a magnetising current i_0, lagging far behind P, and of a current i_2', which is equal and opposite

to the current i_2 flowing along the axis $a\,a$ in the short-circuited secondary, the number of effective turns of which is z_2. As long as the rotor is stationary the magnitude of i_2 will depend on e_1, which is the E.M.F. *induced* in the rotor along $a\,a$, by the flux N_1^o, set up by i_0, and on the impedance of the rotor along that axis; whilst the phase relation between i_2 and e_1 will depend on the ratio of the inductive to the ohmic resistance of the rotor in the axis $a\,a$, *i.e.*, on tangent ω_2.

Both i_2 and i_2' will produce fluxes along every available path, the magnitude of these fluxes standing in inverse ratio to the reluctance of the various paths. If the motor is devoid of leakage, then the only path having a finite reluctance is that followed by N_1^o, or the path for the flux of mutual induction; consequently the fluxes set up by i_2 and i_2' respectively will only have that same path open to them, and since i_2 and i_2' are equal and opposite, the fluxes set up by them will cancel out—therefore they are not really present in the machine, are not available for any purpose whatsoever, are termed imaginary, and need not be further considered. If, however, the motor is not devoid of leakage, and in practice such is always the case, then over and above N_1^o and the two imaginary fluxes just referred to, there will exist others both in stator and rotor; these others closing over leakage paths having as a rule a very high reluctance as compared with the path of mutual induction, and being known as leakage fluxes. It is vital for the proper operation of the motor under consideration that these leakage fluxes be as small as possible.

The actual leakage fluxes for both stator and rotor can be broadly classified under the headings "peripheral" and "flank" dispersion.* The "peripheral" leakage can further be subdivided into slot and zigzag leakage. This whole question of leakage teems with difficulties, enters into every alternate-current problem, and always requires very careful consideration; in the present case it is of particular complexity.

The reluctances of the various leakage paths differ, and the M.M.F.'s acting on each of these paths are also different, then the reluctance of each path is not constant for all values of the corresponding M.M.F., and, finally, it is a difficult matter to predetermine the various reluctances. It can, however, in most cases be assumed that where the leakage fluxes thread iron, that iron will be saturated, in which case the reluctance of all leakage paths may be taken to be constant for all values of the acting M.M.F.'s. A fair approximation to the actual facts can then be arrived at by assuming *equivalent* leakage paths for both primary and secondary such paths having constant reluctances ρ_s' and ρ_s'', as compared to the variable and very much smaller reluctance ρ of the path of mutual induction, it being understood that the whole of the ampere-turns $\overline{(i_2'\,z_1) + (i_0\,z_1)} = (i_1\,z_1)$ acting on ρ_s' produce a leakage flux N_1', equal, for all conditions, to the sum of the true primary leakage fields as they actually exist in the motor, and that the ampere-turns $(i_2\,z_2)$ acting on ρ_s'' produce a leakage flux N_2' equal, for all con-

* Behn-Eschenburg, *Journal of Institution of Electrical Engineers*, vol. 33, p. 239, 1904; J. Heubach, "Der Drehstrommotor."

ditions, to the sum of the true secondary leakage fields as they actually exist in the motor.

It has been stated that ρ is variable; strictly speaking this is true, for one part of the ampere-turns producing the mutual flux goes to overcome the reluctance of the iron path, which reluctance varies with the density, whilst the rest of these ampere-turns is expended on forcing N_1° twice through the air-gap. The iron ampere-turns are, however, as a rule, so small compared to the air ampere-turns that their variable nature may be neglected within the range within which N_1° is likely to vary, so that no serious mistake is made in further simplifying matters by assuming ρ constant as well. We then have at any instant a flux N_1 produced in the stator where $N_1 = \overline{N_1^\circ + N_1'}$ of which N_1° threads both stator and rotor windings, whereas N_1' only threads the stator winding; in addition we have N_2', which only threads the rotor winding.

The phase relation existing between a M.M.F. and the corresponding flux depends on the presence or absence of iron in the path of that flux. When there is no iron in that path, then M.M.F. and flux are cophasal, but if iron is present the flux lags behind its M.M.F. by an angle θ depending on the losses arising in that iron and due to the oscillations of the flux. This angle is appreciable but not very large as a rule, and depends on the volume of iron involved, on its thickness, quality, and on the flux density; within the practical limits θ may be taken as constant for not widely differing values of the M.M.F. As a rule the leakage fluxes, which involve little iron, are taken to be cophasal with their M.M.F.'s, thus N_2' may be said to be cophasal with $(i_2 z_2)$, and N_1' with $(i_1 z_1)$. On the other hand, N_1° lags behind $(i_0 z_1)$ by the angle θ_1, which can be taken as constant for the motor shown in Fig. 1, and within the range of its normal operation, but which ought not to be neglected.

Every flux *induces* in the winding which it threads an E.M.F. lagging 90° behind itself. Thus in the stator winding there will be e_1°, due to N_1°, and e_1' due to N_1', so that P must at any instant equal and oppose the vectorial sum of $\overline{e_1^\circ + e_1' + i_1 w_1}$ where $i_1 w_1$ is the primary ohmic drop. In the rotor we have e_1 due to N_1°, also $e_2 = i_2 x_2$ due to N_2' and $i_2 w_2$, so that at any instant $\overline{e_1 + e_2 + i_2 w_2} = 0$. A consideration of Fig. 2, where these conditions are depicted in an exaggerated manner and for the case of the rotor standing still, will readily show that the magnitude of the mutual flux N_1°, and therefore the magnitude of i_0, *must* vary with every variation of the magnitude of i_2 or of the phase of i_2 relatively to e_1, in other words, with the angle ϕ_2, and even if P is kept constant. It is important to get an idea as to the order of magnitude of this variation of N_1°, for it will appear later that the actual value of N_1° is a determining factor in the starting and running performance of some of the motors which will be considered.

Any method which leads to the predetermination of the ratio $\frac{\rho_1'}{\rho}$ also allows of the magnitude of N_1° to be determined for any value or

phase position of the primary current. When a motor is connected across the mains whilst standing still with its rotor short-circuited, as for instance at starting, then the no-load value of N_1° will fall to $v_1 N_1^\circ$ where v_1 is always smaller than 1, and may easily be as small as 0·3, or sometimes even less. If the secondary has ohmic resistance only, then v_1 increases with w_2, and for a given current also varies directly with $\frac{\rho_1'}{\rho}$. When the secondary has reactance as well, then for a given current v_1 is the greater, the greater x_2. In addition N_1° may be said to vary proportionally with the E.M.F. at the terminals of the primary.

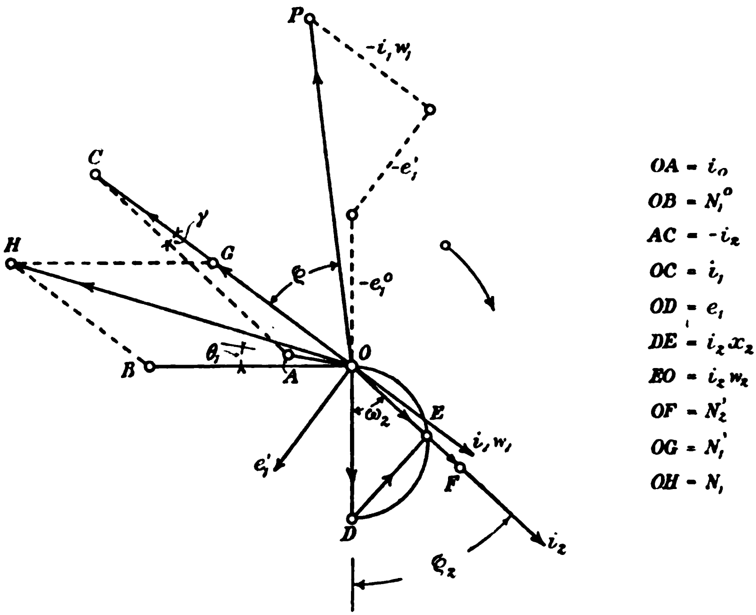


FIG. 2.

It is to be further noted that the better the power factor, *i.e.*, the smaller the phase difference between i_1 and P the smaller is the effect of $\frac{\rho_1'}{\rho}$ on $v_1 N_1^\circ$, and the greater the effect of w_1; on the other hand, the worse the power factor the greater the effect of $\frac{\rho_1'}{\rho}$ and the smaller the influence of w_1. In a tolerably good motor $v_1 N_1^\circ$ will, under running conditions, differ very little from N_1° for any value of the stator current within the working range of the machine.

It is now clear that as long as the motor in Fig. 1 is at rest no torque can result. The only fields which are present are the local leakage fields N_1', N_2' in stator and rotor and the transformer field

$v_1 N_1^o$ threading both stator and rotor. These three fields are *coaxial*, and their axis coincides with that of z_1. This axis will in future be referred to as the transformer or armature axis. In the rotor there is only a current i_2 flowing from a to a, which is the armature current, but as it is distributed on the rotor symmetrically on either side of the transformer axis it can produce no torque with any of the existing fields,

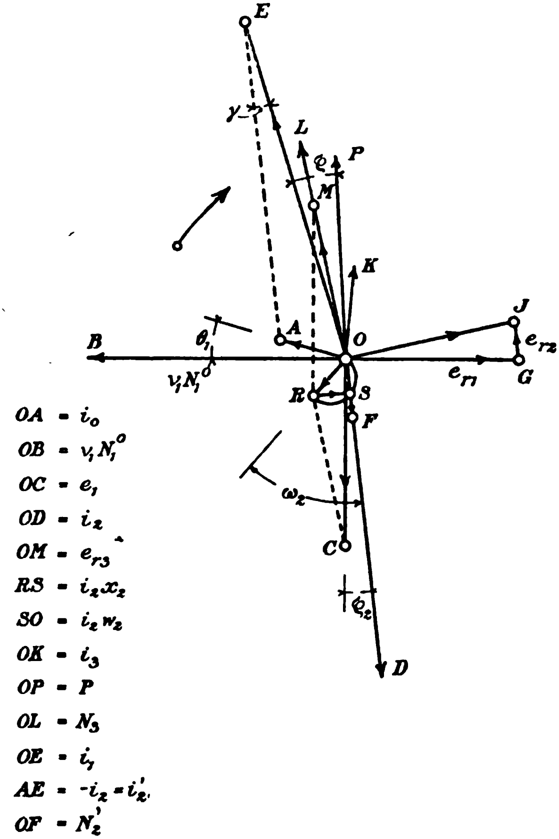


FIG. 3.

for they are all coaxial with the current distribution in the rotor. Even if N_2' or $v_1 N_1^o$ were of same phase as i_2 the torque produced by the ampere-turns on one side of the armature axis would be cancelled by an equal and opposed torque produced by the opposite ampere-turns on the other side of $a\,a$.

As soon as the motor is somehow brought up to a nearly synchronous speed the conditions undergo a material change. The diagram shown

in Fig. 3 represents these altered conditions, although in a slightly exaggerated manner, in order to gain in clearness. For the same reason many of the vectors such as $i_1 w_1$, e_1', e_1°, N_1' and N_1 of Fig. 2 have been omitted, and it has simply been assumed that $v_1 N_1^{\circ}$ lags by a little less than 90° behind P.

By rotation of the rotor conductors in $v_1 N_1^{\circ}$ an E.M.F. (e_{r1}) is *generated* (as distinguished from *induced*) at the brushes *b b*, and the phase of this E.M.F. will, according to the direction of rotation, be the same as the phase of $v_1 N_1^{\circ}$ or opposite to it. In Fig. 3 the direction of rotation of the motor is supposed to be such that the phase of e_{r1} differs from that of $v_1 N_1^{\circ}$ by 180°. Further, by rotation of these same rotor conductors in N_2' an E.M.F. (e_{r2}) is generated, which, for the same reason as before, differs from the phase of N_2' by 180° and is shown at G J. The leakage flux N_2' is cophasal with i_2 and is shown at O F. Since these two E.M.F.'s appear in the same circuit, that along the axis *b b*, which axis will hereinafter be referred to as the motor field axis, it is their resultant $\overline{e_{r1} + e_{r2}}$ which will determine the flow of current in that circuit. This resultant is O J, and it produces a current i_3, shown at O K, which current is nothing but the magnetising current of a transformer on open circuit, the primary of which is disposed on the rotor between the brushes *b b*, and has no secondary either on the rotor or on the stator. It follows that i_3 will lag behind O J much as i_0 lags behind P.

The ampere-turns $i_3 z_2$, the axis of which is perpendicular to that of the ampere-turns $i_2 z_2$ and $i_1 z_1$, produce a field N_3 along the axis *b b*, which will lag behind i_3 by an angle of the same order of magnitude as θ_1; this is the motor field N_3 and it is shown at O L. Now N_3 induces in the rotor conductors and along its own axis an E.M.F. e_3, purposely omitted from the diagram, which together with $i_3 w_2$, and e_3' (neither of which are shown) must be equalled and opposed by $\overline{e_{r1} + e_{r2}}$ or O J. Finally, by rotation of the rotor conductors in N_3 an E.M.F. (e_{r3}), shown at O M is generated at the brushes *a a*; its direction is always such as to oppose the armature current i_2. When e_{r1} is of opposite phase to $v_1 N_1^{\circ}$ then e_{r3} is of same phase as N_3 and *vice versâ*. It is seen that there are two E.M.F.'s in the rotor along the axis *a a*, the first is the working E.M.F. (e_1) which only depends on the magnitude and phase of $v_1 N_1^{\circ}$, the second is the back E.M.F. (e_{r3}) which depends on the magnitude of N_3 and on the speed, therefore on the square of the speed. The resultant O R of these two E.M.F.'s is the one which, together with the time constant of the rotor, determines the phase and magnitude of the armature or working current i_2, so that at any instant $OR = \overline{RS + SO}$ where $RS = i_2 x_2$ and $SO = i_2 w_2$. Fig. 3 approximately illustrates the conditions for such a motor operating near its full load and it is now possible to recognise the true torque conditions.

The production of a torque in such a machine has been ascribed to the interaction of all sorts of currents and fields, the theory which has

perhaps been most generally accepted distinguishes between the following three torques—

$$\begin{array}{lll} & D_1 \text{ between } v_1 N_1 & \text{and } i_3 z_2 \\ & D_2 \quad ,, \quad N_2 & ,, \ i_3 z_2 \\ \text{and} & D_3 \quad ,, \quad N_3 & ,, \ i_2 z_2 \end{array}$$

where $v_1 N_1$ is supposed to be that part of the field due to $i_1 z_1$ which threads the rotor, where N_2 is supposed to be a field linking with both rotor and stator and due to $i_2 z_2$, and where N_3 links with both rotor and stator and is due to $i_3 z_2$. According to this theory $D_2 = - D_3$, and the actual effective motor torque $D_a = D_1$.

From what has already been said, and by reference to Fig. 3, it must be clear that no field such as N_2 exists in the motor at all, so that $D_2 = 0$, and it will be shown that D_3, which is supposed to be cancelled by D_2, is the main torque, whereas D_1 is either zero or very small indeed. As a matter of fact three torques can exist, but they are due to the interaction of other factors than those which have been just enumerated.

The ampere-turns $i_3 z_2$, like any other, can produce a torque with any field, the axis of which does not coincide with their own axis, which in this case is $b\,b$; they can therefore produce a torque with any of the fields in the transformer axis as long as these actually thread the rotor and are either in phase with i_3 or differ from i_3 by a phase angle $\lesssim 90°$.

Thus $i_3 z_2$ can produce a torque D_1 with $v_1 N_1°$, *i.e.*, O K with O B in Fig. 3, and $i_3 z_2$ can produce a torque D_2 with N_2', *i.e.*, O K with O F in Fig. 3.

We can write—

$$D_1 \equiv (v_1 N_1°)\,(i_3 z_2) \cos \delta_{13} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where δ_{13} is the phase angle between $v_1 N_1°$ and i_3; for $\delta_{13} = 90°$, $D_1 = 0$, but O K is practically at right angles to O B, and D_1 is therefore very small if not zero. It is further to be noted that the phase relation between O B and O K hardly changes during the operation of the motor, so that D_1 is of little importance as a rule.

Any change in the magnitude of D_1 is almost entirely due to the fact that the phase of O K alters slightly with varying load, the angle B O K increasing with the load. If it were not for this phase variation D_1 would be practically constant under all loads, for the "motor field" $v_1 N_1°$ and the "armature current" i_3, which determine the torque D_1, change but little from no load to full load.

$$D_2 \equiv N_2'\,(i_3 z_2) \cos \delta_{23} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The phase angle δ_{23} between N_2' and i_3 in Fig. 3 is nearly 180°, both torque factors are therefore fully effective. Notwithstanding D_2 can never be very large, for i_3 is only of the same order of magnitude as i_0. Strictly speaking i_3 is, as a rule, somewhat greater than i_0, because although the reluctance of the path for N_3 is about equal to the

reluctance of the path for $v_1 N_1^\circ$, yet the winding in which i_3 flows is generally distributed over the whole pole-pitch, whereas z_1 is generally only distributed over $\frac{2}{3}$ of the pole-pitch. As a rule i_3 is only about $\frac{1}{3}$ to $\frac{1}{4}$ of the value of i_2 at full load and N_2' being only a *leakage field* is not only very small as compared to $v_1 N_1^\circ$ or N_3, but little of it links usefully with the whole of $i_3 z_2$ so that D_2 is never likely to be of any appreciable magnitude.

The character of D_2 differs from that of D_1; in this case i_3 must again be looked upon as the "armature current" and remains materially constant for all loads; strictly speaking it decreases proportionately with the speed, whereas the "motor field" N_2' increases with the load. These conditions are exactly opposite to those existing as a rule in a shunt motor where it is the field which is constant and where the armature current varies with the load. The phase relation between i_3 and N_2' varies somewhat with the load, but the direction of D_2 remains constant.

Finally the rotor ampere-turns in the axis *a a* can produce a torque D_3 with N_3, which appears along the axis *b b*, thus—

$$D_3 \equiv N_3 (i_2 z_2) \cos \delta_{32} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where δ_{32}, the phase angle between N_3 and i_2 or O L and O D, is all but 180°. Now N_3 is all but equal to $v_1 N_1^\circ$ in magnitude, whereas i_2 is all but equal to i_1 so that both are large; D_3 therefore is the *real torque* of the motor, and that is why I have always designated i_2 as *the* armature current, and N_3 as *the* motor field of this machine. D_3 is a true "shunt torque," for the motor field is practically constant for all loads, and the armature current varies with the load.

If it is desired to express the torque of the motor as a function of the primary current i_1 and not of the secondary current i_2 then it need only be remembered that here, as in every transformer, i_1 is the vectorial sum of the working current i_2 and the magnetising current i_0 thus—

$$i_2 z_2 = \overline{i_1 z_1 - i_0 z_1}$$

$$= v_0 i_1 z_1$$

where v_0 is a coefficient which varies with the variation of the magnitude of and the phase relation between i_2 and i_0; v_0 is very small indeed at no load and approaches unity without ever equalling it as long as no phase compensating device is made use of. If the motor is, however, compensated, then v_0 may easily become equal to or greater than 1; a glance at Fig. 3 will make this clear. We can also write—

$$D_3 \equiv N_3 \overline{(i_1 z_1 - i_0 z_1)} \cos \delta_{32} \equiv N_3 (v_0 i_1 z_1) \cos \delta_{32} \quad . \quad . \quad (4)$$

If it be desired to determine the actual value of D_3 in kgm., then we must write—

$$D_3 = B \frac{i_2}{\sqrt{2}} z_2 \frac{l \cdot d}{9{\cdot}81 \times 10^8} \cos \delta_{32} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where i_2 is the maximum and $\frac{i_2}{\sqrt{2}}$ the effective amplitude of the armature current on the assumption that this current varies according to a sine function, where l and d are the useful lengths and diameter of the rotor in centimetres, and B the mean space value of the effective density of that portion of the field N_3 in which are immersed the ampere-turns $i_2 z_2$. In this case $i_2 z_2$ are distributed over the whole pole-pitch so that B will be the same as the mean effective magnetic density of N_3 as a whole.

This same formula (5) is applicable in the same manner to all the torques discussed in this paper, and need not again be referred to in detail; it only introduces constants which enable the absolute value of the torque to be determined. These constants are, however, not the same in all cases.

The relative *directions* of the three torques just dealt with must next be considered. For that purpose the phase and direction, or the relative signs of the determining factors, must be taken from Fig. 3 for the same instant in all cases and plotted as in Figs. 4, 5, and 6 on the

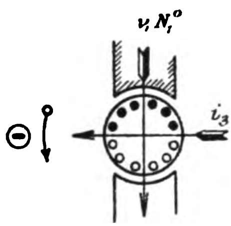


FIG. 4.

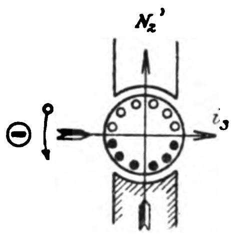


FIG. 5.

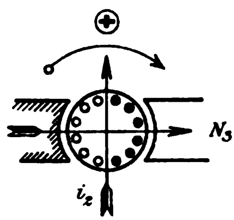


FIG. 6.

assumption, for instance, that from top to bottom and from left to right of the paper be positive, also that the current distribution on the rotor is such as to tend to produce a field in the same direction as the arrow showing the sign of the current vector in the rotor. Care must be taken to consider only those components of the two factors (current and field) involved which are in phase with each other.

One can take any arbitrary line passing, for instance, through O and R in Fig. 3, and count all the vectors to the left of it as positive, all those to the right of it as negative. Fig. 4 refers to the torque D_1; for the instant chosen, $v_1 N_1^0$ is positive, whereas the effective component of i_3 or O K is negative, for it is represented by the projection O K′ of O K on O B, and since $\sphericalangle$ B O K > 90° then O K′ coincides in direction with O G. Fig. 5 refers to the torque D_2 for the same instant, i_3 is clearly positive and N_2' negative, whereas in the case of Fig, 6, which refers to the torque D_3, it is easily seen that N_3 is positive and i_2 negative.

Since D_3 is by far the greater torque, it can be referred to as the positive torque; this is indicated in Fig. 6 by placing a plus sign near a long direction arrow. The two other torques, being much smaller and opposite in direction, have smaller direction arrows and minus signs.

It should be noted that whereas there is no practical possibility of the direction of either D_2 or D_3 being reversed in normal operation, it is quite possible and even probable that D_1 may be so reversed. The effective component of O K changes its sign as soon as $\sphericalangle$ B O K < $90°$; in Fig. 4 the direction i_3 is thereby reversed and with it the direction of the torque D_1. As will be seen from what follows, this reversal actually does take place when the author's method of phase compensation is made use of.

In an ideal motor without leakage or resistance, e_{r3} is, at synchronous speed, exactly equal to and exactly opposed in phase to e_1, for N_2' does not exist, and there is therefore only e_{r1} acting in the field circuit *b b*. It cannot be sufficiently emphasised that e_{r1} is the exciting E.M.F. of the motor, it is to e_{r1} that the motor field is due. Now e_{r1} is generated in the motor itself by rotation, and is practically in quadrature with P, therefore with e_1 which is the working E.M.F. There exist

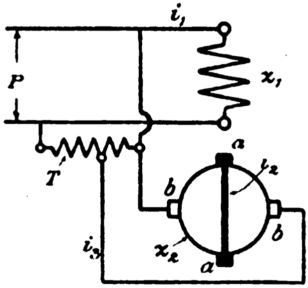


FIG. 7.

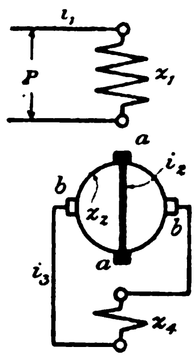


FIG. 8.

consequently in this machine the necessary conditions for a true alternate-current *shunt motor*, and this motor is *self-excited*.

The presence of leakage in the rotor along the armature axis is most detrimental to torque and power factor; happily the leakage field N_2' gives rise, as has been explained, to the E.M.F. (e_{r2}), the effect of which is to alter the phase of the motor field N_3 so as to bring the armature current i_2 more into phase with the working E.M.F. (e_1), as can be well seen from the diagram in Fig. 3. This small E.M.F. (e_{r2}) therefore tends to compensate the motor, but since e_{r2} depends on the speed, and since under load that speed is always below the synchronous, it is never possible for e_{r2} to compensate the motor fully. Even if e_{r2} sufficed to bring i_2 into phase with e_1, the reactance of the stator would still remain uncompensated, and this is why the machine has been referred to by the author as partly compensated. The method of fully compensating such motors, which was devised by the author, is based on the considerations just set forth and simply consists in introducing into the field circuit an E.M.F. which will assist e_{r2}. Since e_{r2} is nearly of the same phase as P,

this auxiliary compensating E.M.F. is conveniently derived from the mains, and by way of a transformer T, which may obviously be separate from the motor, as in Fig. 7, or embodied in it, as in Fig. 8. If e_{r_2} is sufficiently increased to get a very high power factor, then D_1 becomes positive, there is, however, a danger in improving the power factor too much. It is true that D_1 keeps increasing as the power factor rises, but then D_1 is very small, and it is quite sufficient if it be made zero. What is much more important is that O L or N_3, and OD or i_2, both tend to approach the phase of O B, and thereby to get more and more out of phase with each other, causing the motor to yield a smaller torque D_3 per ampere, thus lowering its efficiency. Discretion in the use of the compensating E.M.F. in the exciting circuit is clearly of importance, but when required the motor can be not only fully compensated but also over compensated, when i_1 will lead P. Apart from the auxiliary compensating E.M.F. the power factor of the motor for a given speed depends on the time constant of the rotor in the axis *b b*; this time constant should be such that N_3 may lag as far behind the exciting E.M.F. (e_{r_1}) as possible, which will reduce the no-load current and improve the power factor.

The transformer in Fig. 7, and the winding z_4 in Fig. 8, are not introduced for the purpose of exciting the motor; the motor is self-excited as was shown with reference to Fig. 1, and T, or z_4, only serve to fully compensate the machine. The machine in Figs. 7 or 8 is therefore without doubt, a "self-excited compensated single-phase shunt induction motor," and so far as torque is concerned, does not differ fundamentally from the motor shown in Fig. 1.

The resultant torque of the partly or fully compensated motor under consideration may therefore be written as—

$$D_a = D_3 - D_2 \pm D_1 \quad . \; . \; . \quad . \; . \; . \; . \; . \quad (6)$$

where both D_2 and D_1 are very small as compared to D_3, so that it is sufficiently accurate to say that—

$$D_a \equiv D_3 \equiv N_3\,(i_2\,z_2) \cos \delta_{32} \equiv N_3\,(\overline{i_1\,z_1 - i_0\,z_1}) \cos \delta_{32}$$
$$\equiv N_3\,(v_0\,i_1\,z_1) \cos \delta_{32} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (7)$$

Disregarding the very small torques D_1 and D_2, it can be said that the motors in Fig. 1, 7, or 8, although they have two current axes per pole pair on the rotor, namely *a a* and *b b*, and also two fields per pole pair threading all the rotor windings, *i.e.*, the transformer field $v_1\,N_1{}^{\circ}$ and the motor field N_3, yet only one current and one field axis is effective as far as torque production is concerned. These machines may therefore be said to have only one effective armature and only one effective field axis per pole pair, in which particular they closely resemble the ordinary neutralised continuous-current motor or generator diagrammatically illustrated in Fig. 9.

This Fig. 9 may serve to illustrate either the continuous-current series or the continuous-current shunt motor according to whether the armature circuit *a a* including the neutralising winding z_1 is connected

in series or in parallel with the field winding *b b*. It is no secret that there is no theoretical reason why the field winding *b b* of such a continuous-current motor should not be disposed on the rotor as in the case of the alternating-current machine under discussion; if this is done the analogy becomes more striking still, as will appear from an inspection of Fig. 10 which shows this modification. The arrangement in Fig. 10 is, of course, quite suitable for a continuous-current shunt motor provided armature and field windings be connected to independent sources of energy. In the case of a continuous-current series machine a further and obvious precaution is necessary when two sets of brushes per pole pair are used; this precaution consists in the provision of two independent windings on the rotor when the connections shown in Fig. 11 can be made. But it is not necessary in this last case to use two sets of brushes per pole pair; it is sufficient to displace the brush set *a a* from its position of coincidence with the axis

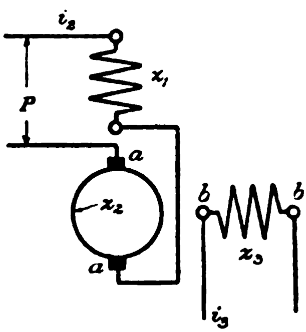


Fig. 9.

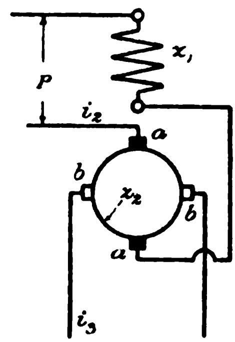


Fig. 10.

of the neutralising winding z_1 so as to neutralise only that part of the rotor winding which in Fig. 12 is shown in heavy lines, and the remainder of the rotor winding will then excite the motor field in a direction perpendicular to the axis of the neutralising winding z_1; the direction of rotation is shown by the curved arrow. If z_2 is the total number of effective turns on the rotor, and if a is the angular displacement of n_1, the perpendicular to the neutralising winding, and of n, the perpendicular to the brushline *a a*, then those rotor ampere-turns which do duty as armature ampere-turns $A\,T_a$, and are distributed on the portion of the rotor distinguished by heavy lines, can be expressed as—

$$A\,T_a = \left(1 - \frac{4\,a}{360}\right) i_2\, z_2,$$

whereas the expression for the remainder of the rotor ampere-turns which do duty as field ampere-turns, is—

$$A\,T_f = \frac{4\,a}{360}\, i_2\, z_2.$$

I am fully aware that I am disclosing no startling novelty in these Figs. 10, 11 and 12; continuous-current motors arranged as shown by these figures have been known for years, and the one shown in Fig. 12 has been carried out in practice repeatedly in a tentative way, but practical considerations have always militated against the use of these modifications of the time-honoured dispositions shown in Fig. 9. It is, however, thought to be of no small importance to recall these possibilities at the present stage, for their careful consideration leads one to gain a true and remarkably simple insight into the operation of every one of the many alternate-current motors which have been proposed in the last five or six years. It has been known for years that every continuous-current motor is susceptible of being operated by alternate current provided certain known precautions are observed, so that the diagrams shown in Figs. 9, 10, 11 and 12 apply equally to alternate-current motors. It remains to be remembered that in the case of alternate currents energy can not only be transferred from one

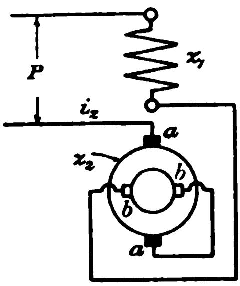


FIG. 11.

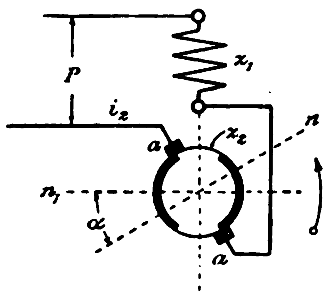


FIG. 12.

circuit to another by *conduction*, as in the case of continuous currents, but also by *induction* when every one of the remaining types of alternate current motors must at once lose much of the mystery so often associated with their mode of operation. The fact of transferring energy from one circuit to another by induction necessitates the presence of some link or other between the circuits involved, this link is invariably a flux of mutual induction, and is always referred to as the transformer field by the author. Whenever, therefore, any of the conduction motors shown in Figs. 9, 10, 11 and 12 are converted into machines in which the energy is conveyed to the armature by induction, an additional field is introduced into such machines. In studying the mode of operation of induction machines, the influence of this additional or transformer field on every element of the apparatus must be taken into account: this presents no serious difficulty, but requires care. The influence of this transformer field has been fully taken into account in the short discussion which has already been given of the theory of the motors shown in Figs. 1, 7, and 8. These induction motors are probably the most intricate yet known, and those

who have followed the reasoning so far will have no difficulty whatsoever in following it to the end, for it may fairly be said that the true theory of the motor shown in Fig. 1 contains the key to any puzzle in the way of induction motors, whether of the single or polyphase type, which may possibly be set.

(*b*) *The* (*Separately Excited*) *Series Induction Motor* (Fig. 13).—This motor is the "induction" counterpart of the conduction machine shown in Fig. 9, when in the latter armature and field circuit are connected in series relation. At starting the conditions will, as a rule, be something like those shown in the diagram, Fig. 14, the voltage at the terminals of z_1 and z_3 being P_1 and P_3 respectively, if the voltage at the motor terminals is P. Along the axis *a a* the motor is again a short-circuited transformer, the value of the transformer field $v_1 N_1^o$ being smaller than would be required to balance P_1 for the same reasons as have already been fully set forth in connection with Fig. 2; in fact, Fig. 2 is a correct representation of the conditions existing in Fig. 13 along the axis *a a* if

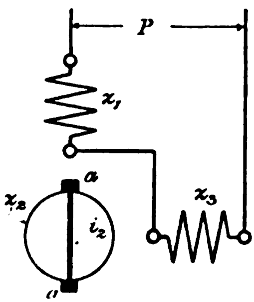


FIG. 13.

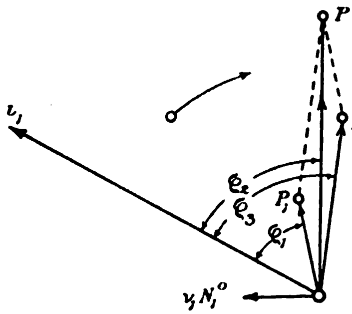


FIG. 14.

in Fig. 2 we replace P by P_1 of Fig. 14. Along the axis of z_3 in Fig. 13 the field N_3 is, on the other hand, directly proportional to P_3 if we neglect the ohmic drop.

In this case, and contrary to what obtains in the case of Figs. 1, 7, or 8, the transformer field $v_1 N_1^o$ does nothing but convey energy into the circuit *a a* by induction. When the motor is running an E.M.F. is certainly generated in the coils undergoing commutation, and by rotation in $v_1 N_1^o$, but as we are not considering the commutating conditions in the present contribution, this effect need not be gone into. The field winding z_3 is disposed on the stator and not on the rotor, and is consequently quite outside any influence of $v_1 N_1^o$, for the axis of z_3 is in addition perpendicular to that of the transformer field. The torque is due to the interaction of N_3 and i_2, and this is the only possible torque in the machine; since N_3 is excited by i_1 and since $i_1 = \overline{i_2 + i_0}$ the characteristic is a series one, for motor field N_3 and armature current i_2 vary proportionally or nearly so. As the speed increases and as i_1 decreases, so does P_3 in Fig. 14 decrease and P_1 increase, their vectorial sum remaining constant and

equal to P. As long as i_1 is large as compared to i_0 the proportionality between i_1 and i_2 is very fair, but since i_2 decreases with increasing speed whereas i_0 then increases, it follows that the proportionality between i_2 and i_1 will not be as thorough at high speeds. For a given reduction of i_2 the corresponding reduction of i_1 will be the greater, the better the power factor.

The formula for the torque must take into account the phase difference between the current and field involved. Now N_3 is due to i_1, and there exists between i_2 and i_1 a by no means constant phase difference γ (see Fig. 2), which not only depends on the phase difference ϕ_2 between i_2 and e_1, but also on the magnitude of i_0. As the speed increases so does i_2 approach e_1, and may even come to lead e_1 whilst i_0 increases. Further, N_3 will always lag behind $i_1 z_3$ by a practically constant angle θ_3. Finally, the whole of the field generated by $i_1 z_3$ will not link with the rotor; a small portion, some 4 to 8 per cent., will close round z_3 without linking with z_2; if the *useful* motor field is $v_3 N_3$, then we can write :—

$$D_b \equiv (v_3 N_3)(i_2 z_2) \cos(\theta_3 + \gamma) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

If it is desired to express the torque in function of i_1, then—

$$D_b \equiv (v_3 N_3)(\overline{i_1 z_1 - i_0 z_1}) \cos(\theta_3 + \gamma)$$
$$\equiv (v_3 N_3)(v_0 i_1 z_1) \cos(\theta_3 + \gamma) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where v_0 has the same meaning as in equation (4). It is obvious that the smaller v_3, v_0, θ_3, and γ, the greater the torque for a given current. Now γ will be very small at starting, for then i_0 is very small; i_2 large and lagging far behind e_1, but at high speeds, and whether the power factor be good or bad, γ may become very large, thus limiting the torque per ampere and the no load speed of the motor.

It is worth noting that the torque per ampere of any motor with a series characteristic is quite independent of the power factor, and is only governed by the factors contained in equations (8) or (9) and such constants as may have to be added from equation (5) in order to obtain the absolute value of the torque. Where such a series motor is of the induction type, then the magnetising current of the transformer along the armature axis has a very great influence on the value of the torque when the speed is high and the armature current small.

At this stage it may be of interest to show just how the phase relation of i_2 and e_1 changes with the speed notwithstanding that the time constant of the rotor, as expressed by tan. ω_2 in Fig. 2, remains strictly constant. For this purpose the diagram, Fig. 15, has been prepared. For the particular direction of rotation chosen, the back E.M.F. (e_{r3}) is cophasal with the motor field N_3; similarly to what took place in the case of the first motor considered, and, as has been shown in Fig. 3, there appear under speed two E.M.F.'s in the armature circuit; these E.M.F.'s (e_1) and e_{r3} are out of phase but tend to oppose each other, consequently it is their vectorial resultant O R which has to be considered and which determines the magnitude and the phase

of the armature current. The phase of i_2 will vary because under the existing conditions the phase of O R must obviously vary. It should be noted that the working E.M.F. (e_1) depends on $v_1 N_1^\circ$ only, and that this transformer field increases with decreasing current because P_1 increases not only with increasing current but also with improving power factor and independently of the current, because then the weakening influence of e_1' decreases, as can be seen by reference to Fig. 2. On the other hand, the back E.M.F. (e_{r3}) depends directly on the speed and on the phase and magnitude of N_3, therefore on i_1. When the torque required is small, i_1 and therefore also P_3 will be small; so that P will nearly coincide with P_1. If the torque is to be small, then O R must be small so as to reduce i_2 and i_1. Now since e_1

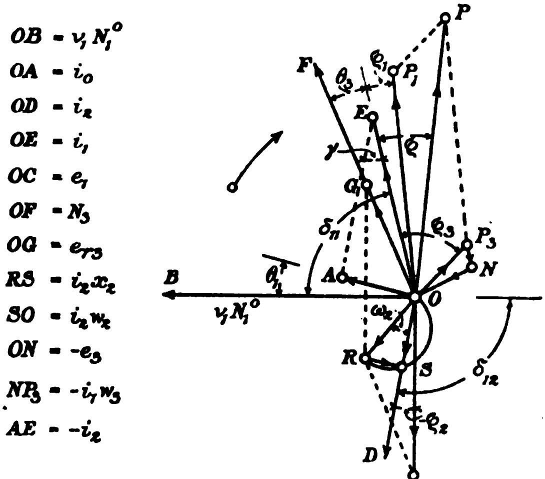


FIG. 15.

is large when i_2 and i_1 are small, then e_{r3} must also be large if O R is to be small; and since N_3 is small because of i_1, it follows that the speed must be high. But the greater e_{r3} in comparison to e_1, the further will O R lead e_1, and as i_2 lags behind O R by the *constant angle* ω_2, the less will i_2 lag behind e_1. As the lead of O R on e_1 increases, i_2 will even come to lead e_1—which all tends to improve the power factor, for i_1 follows the phase of i_2 as far as i_0 will allow.

In this series induction motor there are again two fields at right angles to each other in space, but whereas in the case of Figs. 1, 7, or 8 these two fields were practically constant and always differed by 90° in phase, in Fig. 13 they are neither constant in magnitude nor is their phase relation constant, but varies with the power factor; they get nearly into quadrature for values of cos ϕ near unity or for a leading current. At synchronous speed these two fields are nearly equal and

nearly in quadrature, and if a comparison between the motors shown in Figs. 1, 7, or 8 and 13 is required it is best taken on the assumption that the second motor's normal load is reached at about synchronous speed.

It is quite evident that the motor in Fig. 13 has only one effective armature ($a\ a$) and only one effective field axis (z_3) per pole pair, although it has two fields per pole pair threading both stator and rotor windings along the axes first referred to.

Comparing D_a and D_b under the conditions named we get—

$$\frac{D_a}{D_b} = \frac{N_3\,(v_0\,i_1\,z_1)\cos\delta_{32}}{(v_3\,N_3)\,(v_0\,i_1\,z_1)\cos(\theta_3+\gamma)}$$

$$\frac{D_a}{D_b} = \frac{\cos\delta_{32}}{v_3\cos(\theta_3+\gamma)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

It is likely that $\theta_3+\gamma$ will always be larger than δ_{32}, but even if it is only equal to δ_{32}, D_b will still be the smaller on account of v_3 which is always smaller than 1. There is, as a matter of fact, little to choose between the two machines as far as weight efficiency is concerned and on the showing of equation (10), the advantage, such as it is, lies with D_a.

Before proceeding, it appears advisable and useful to draw attention to the chief structural difference between the two machines, and to examine into the influence of this structural difference on the weight efficiency. Structural differences of the kind about to be analysed can hardly be fully taken into account in a general formula such as (5), although they are to some extent represented therein, but they must not be overlooked.

In Fig. 8 one and the same rotor winding is made use of as armature and field winding, whereas in Fig. 13 armature and field winding are separate, the field winding z_3 being disposed on the stator. Assume that for the motor in Fig. 8 $i_2 = 2\,i_3$, which is approximately the right proportion, and let there be independent windings disposed on the rotor, each with a resistance of 2 ohms. The loss in the armature winding, if $i_2 = 4$, will be 32 watts, and that in the field winding for $i_3 = 2$ will be 8 watts, or a total loss of 40 watts. The field winding in Fig. 8 is distributed over the whole pole-pitch t, whereas in Fig. 13 it may be distributed as desired; it is, as a rule, convenient to distribute it over $\frac{1}{3}$ of the pole-pitch only. Under such conditions the space distribution of the field flux for the two cases will be as shown in Figs. 16 and 17 respectively, and it will require many more ampere-turns to produce a given total flux distributed in space as shown in Fig. 16, which corresponds to the field distribution of the motor shown in Fig. 8. The same flux in the motor in Fig. 13 and distributed as indicated in Fig. 17 can be produced with a far smaller number of ampere-turns. The ratio is easily ascertained and works out at 200 : 120 in favour of Fig. 17. For the same C^2R loss the weight of copper in the field winding of Fig. 13 need therefore only be about $\frac{1}{3}$ of that

required in the case of Fig. 8, when two independent windings are used on the latter. As the weight of copper for these two windings in Fig. 8 has been assumed to be equal, it follows that the ratio of armature plus field copper in Figs. 8 and 13 is as 2 : 1·36. But in Fig. 8 armature and field winding are in reality *not* independent, and i_2 and i_3 pass through one and the same rotor winding; since these currents are practically in phase and since they traverse the same winding but along axes displaced by 90° in space, it follows that the C^2R loss they occasion only corresponds to a current $i' = \sqrt{i_2^2 + i_3^2}$ passing through the rotor in one direction only. The combined weight of the two distinct windings was twice that of the armature winding only; if the two windings are now combined into one, as is the case in the actual machine, then the

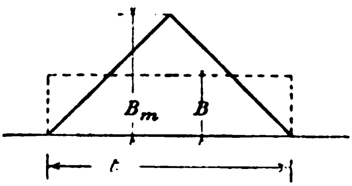


FIG. 16.

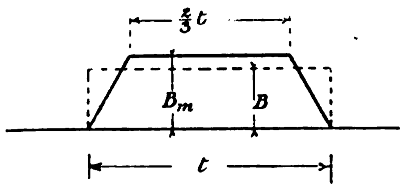


FIG. 17.

weight can be reduced in the proportion of 2 : 1 without increasing the C^2R loss, for the current now to be considered is only i'. In this respect the motor in Fig. 13 is consequently also at a disadvantage, having for the same total C^2R loss 36 per cent. more copper on armature and field than the machine shown in Fig. 8. The self-excited shunt motor is nevertheless at a disadvantage in another respect; the ratio of maximum to mean space density or $B_m : B$ for the same total flux is as 2 : 1 for Fig. 16 and as 1·2 : 1 for Fig. 17, from which follows that for a given total flux the maximum space density of the motor field in Fig. 8 must be 1·66 times greater than the maximum space density of that same field in Fig. 13. This is a disadvantage which can, however, be obviated by slightly increasing the rotor copper or the C^2R loss in the rotor of Fig. 8, for instance, by disposing the brushes as shown in Fig. 18. In Fig. 18 the armature current only flows through the heavily lined portions of the rotor circumference, and the field current only flows through the remaining portions. The armature ampere-turns must only increase in the proportion of 2 : 3 and not of 1 : 2 on account of the better field distribution which will now prevail, and notwithstanding the fact that the armature-turns are now halved. The increased armature current will only flow through half the winding, encountering only half the resistance, so that the loss

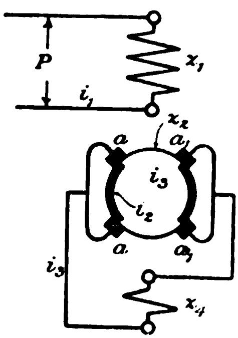


FIG. 18.

due to the armature current increases in the proportion of 32 : 36. The field winding now being distributed only over one-half and not over the whole pole-pitch, the field current i_3 will, for the same total flux, be reduced in the proportion 45 : 30 and then increased in the proportion 1 : 2 because the field-turns are halved, so that it now rises to 1·34 times of its value in Fig. 8, and the loss it occasions is reduced in the proportion 8 : 7 ; the total C^2R loss therefore increases to 43 watts, and can be reduced to its original value by increasing the copper on the rotor of Fig. 8 by 8 per cent., thus reducing the advantage held by Fig. 8 in respect to Fig. 13 from 36 to 25 per cent.

On the whole, the advantage, as far as weight efficiency is concerned, remains with Fig. 8, particularly as the latter is fully compensated. This advantage is quite material when that disposition of the brushes is made use of which is shown in Fig. 18.

(*c*) *The Neutralised Series Conduction Motor* (Figs. 19 and 20).—

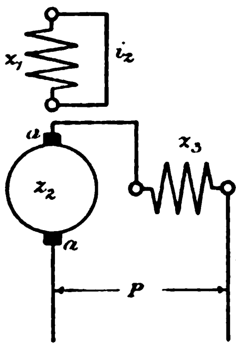


FIG. 19.

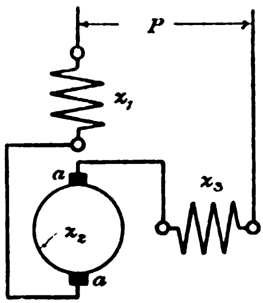


FIG 20.

This is the conduction counterpart of the induction motor shown in Fig. 13. This machine is not compensated in any way ; z_1, whether short-circuited or connected in series with the armature and in opposition to it, simply neutralises the armature self-induction ; this neutralisation is an absolute necessity in any alternate-current motor, and does not amount to "compensation," which expression should be reserved to designate any other special and controllable means adopted with a view to improving the power factor, beyond the insufficient values to be obtained by mere neutralisation of the armature ampere-turns, and to bring that power factor up to unity or thereabouts.

The torque in this case is easily determined. When neutralisation is complete there is no field along the armature axis *a a*, but even if such a field existed it could produce no torque whatever. We can write—

$$D_c \equiv (v_3 N_3)(i_1 z_2) \cos \theta_3 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

where θ_3 has the same meaning as in equation (10), *i.e.*, where it repre-

sents the angle by which the field flux N_3 lags behind its M.M.F. $(i_1 z_3)$. Comparing Figs 13 and 20 we get—

$$\frac{D_c}{D_b} = \frac{(v_3 N_3)(i_1 z_2) \cos \theta_3}{(v_3 N_3)(v_0 i_1 z_1) \cos (\theta_3 + \gamma)}$$

$$\frac{D_c}{D_b} = \frac{\cos \theta_3}{v_0 . \cos (\theta_3 + \gamma)} \qquad \frac{D_c}{D_b} = \frac{1}{v_0 \cos \gamma} \quad . \quad . \quad (12)$$

Weight for weight the motor in Fig. 20 has, at high speeds, a distinct advantage over that shown in Fig. 13, for in the latter the influence of the magnetising current in the transformer axis is greatly felt at high speeds, and is expressed by the coefficient v_0 which then approaches zero. At high speeds γ also assumes large values, thus further reducing the torque per ampere, but the angle θ_3 may be taken as equal in both cases, and may be left out of account from a comparative point of view. At normal speeds the above advantage is not at all great, but defined polar projections can be used in the case of the conduction machine, thus giving it a further advantage by reducing the weight of field copper by some 16 per cent. as compared with the corresponding weight of copper in Fig. 13, besides allowing of better ventilation.

On the other hand, the conduction motor is limited to some 250–300 volts for sizes up to, say, 150 B.H.P. and on periodicities of 25 to 15, whereas the corresponding limit for the induction machine is about 3,000 volts. The difference in weight for equal terminal voltage is in no case at all considerable, and would not exceed 10 per cent. in favour of the conduction machine. This motor has only one armature and one field axis per pole pair; both of them are effective, and there is no field in the motor (apart from leakage fields) which does not contribute to the torque; the efficiency of this machine will therefore be a little higher than that of any of those which have already been dealt with.

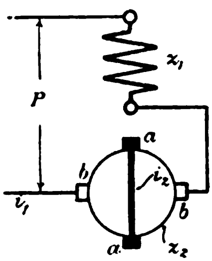


FIG. 21.

(*d*) *Partly Compensated* (*Separately Excited*) *Series Induction Motor* (Fig. 21).—The torque conditions in this case are just a little more complicated, and have given rise to widely differing suggestions. The interpretation which appears to have gained most ground ascribes the torque of the machine to the field due to z_1, and to the rotor ampere-turns along the axis *b b*, thus: $D \equiv (v_1 N_1)(i_1 z_2)$, the actual torque conditions differ widely and very materially.

It is fairly obvious, after what has already been said, that the only structural difference between Figs. 13 and 21 consists in that in the former the field winding is disposed *on the stator* (z_3) whereas in the latter it is disposed *on the rotor* (*b b*) just as was shown to be the case for Figs. 1, 7, 8, or 18. In Fig. 1 the field winding was fed by a constant E.M.F. (e_{r1}), and the motor, therefore, had a shunt charac-

teristic; in Fig. 13 and in Fig. 21 that field winding is traversed by a current which is in series relation to the armature current i_2, the series relation being obtained by interposing a transformer, embodied in the motor itself, between armature and field circuit in such a manner that the armature circuit is traversed by the secondary, and the field circuit is traversed by the primary current of that transformer. This arrangement is, of course, equivalent to the armature current itself being taken through the field winding as long as the magnetising current of the transformer is small; if this magnetising current is not small the proportionality and the phase angle between the primary and secondary currents (which angle ought to be nearly 180°) are disturbed. In the motors under consideration these disturbances reach an appreciable value only at high speeds or very light loads as has already been more fully explained in connection with Fig. 13, although the point was purposely put in a somewhat different manner.

A first deduction to be drawn from these facts is that the current i_2 in the rotor axis *a a* and the field N_3 produced along *b b* by i_1 flowing through the rotor conductors, will, when the rotor is at rest, yield a "series torque" under conditions exactly similar to those obtaining in Fig. 13. In Fig. 21 there will be a transformer field $v_1 N_1°$ along the axis *a a* just as in the case of Fig. 13. Now this field cannot have any inductive effect on the field winding whether the latter is disposed on the stator or on the rotor, as long as the axis of that winding is at right angles to the axis of $v_1 N_1°$. As soon, however, as the rotor begins to revolve, then an E.M.F. is generated in the rotor conductors by reason of their revolution within the flux $v_1 N_1°$; this E.M.F. appears at the brushes *b b*, and will be dealt with later; for the present the starting torque only need be considered. An expression for the torque due to the interaction between the motor field N_3 and the armature current i_2 is easily deduced from equations (8) and (9), for the only difference is the space position of the field winding. Since in Fig. 21 that winding is disposed on the same member as the armature winding, it follows that the coefficient $v_3 = 1$, thus—

$$\begin{aligned} D_1 &\equiv N_3(i_2 z_2) \cos(\theta_3 + \gamma) \\ &\equiv N_3(\overline{i_1 z_1 - i_0 z_1}) \cos(\theta_3 + \gamma) \\ &\equiv N_3(v_0 i_1 z_1) \cos(\theta_3 + \gamma) \quad . \quad . \quad . \quad . \quad . \quad . \quad (13) \end{aligned}$$

Here, as in Fig. 1, the space distribution of the field winding is the worst possible, requiring for an equal number of turns and for the same total flux just twice the current which would be needed in case a unicoil field winding could be used. This disadvantage is compensated by the fact that armature and field current flow through the same winding, which arrangement leads to a saving in copper notwithstanding the unfavourable space distribution of the field winding. In Fig. 21 the field density in the middle of each pole may easily reach undesirable values, but this difficulty can be met by disposing the brushes as has been shown in Fig. 18. In addition, the number of

field-turns is fixed by the number of turns required for the armature circuit and cannot be altered whilst the machine is in operation. If then it be desired to vary the field strength independently of the armature current, it becomes necessary either to vary the ratio between z_1 and z_2 or to interpose between z_1 and the rotor circuit in the direction *b b* a variable ratio series transformer, or to dispose part of the field winding on the stator making this part reversible and variable. Sparking difficulties always necessitate a low voltage armature. Unless, therefore, the terminal voltage is also low i_1 will not be large enough to produce an N_3 of sufficient magnitude with the help of the few turns available between the brushes *b b*, and a series transformer will again be required between z_1 and *b b*. Such a series transformer is practically inseparable from the machine in question if its qualities are to be fully taken advantage of.

Strictly speaking, and similarly to what has been said in connection with Fig. 1, a second torque should be considered arising out of the interaction of $i_1 z_2$ in the axis *b b* and of the leakage flux N_2' due to i_2 in the axis *a a*. Thus—

$$D_2 \equiv N_2' (i_1 z_2) \cos \delta_{21} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

This torque is only referred to for the sake of completeness; it must be very small indeed, but is of opposite sign to D_1.

A third and far more important torque is due to the interaction of $i_1 z_2$ and $v_1 N_1^\circ$, which corresponds to that shown in equation (1) for the motor in Fig. 1. It must be remembered that the motor field is not now excited by i_3, as is the case for Fig. 1, and as is shown at O K in Fig. 3, but by i_1 or O E of that figure; the material consideration is therefore the phase relation between O E and O B in Fig. 3. This phase relation is better shown in Fig. 15, which, of course, holds good for the motor under consideration just as well as for that shown in Fig. 13 as long as the former is at rest. Designating the corresponding phase angle by δ_{11}, we can write—

$$D_3 \equiv (v_1 N_1^\circ)(i_1 z_2) \cos \delta_{11} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

At starting δ_{11} may be something like 50°, and P_1 which, together with other factors already set forth in connection with Fig. 2, determines $v_1 N_1^\circ$ may be $\frac{1}{8}$ of P, so that the transformer field may at the moment of starting reach $\frac{1}{8}$ of its value at synchronous speed, whereas i_1 will have, say, four times its full load value. It will be understood that these figures are only given as an approximate guide, and must vary with the number of turns on the series field winding relatively to the number of turns in z_1, with the time constant of the rotor along both axes, and with other factors referred to in connection with Fig. 2. On the basis of the figures which have been given D_3 works out to about 0·43 of the normal torque of the motor, from which it appears that D_3 can by no means be neglected, although it is not the only nor the largest component of the total torque of the motor. A closer inspection of Fig. 15 will show that as the power factor increases, *i.e.*, as ϕ decreases, so does δ_{11} increase, causing D_3 to decrease. Under normal

working conditions $v_1 N_1°$ will lag practically 90° behind P_1, so that if the power factor at the motor terminals is subdivided into its component parts, *i.e.*, into the power factor at the terminals of the winding z_1, and into that at the terminals of the field winding $b\,b$, calling the former phase difference ϕ_1 and the latter ϕ_3, then it is seen that $\delta_{11} + \phi_1 \cong 90°$, when we can write, although only approximately—

$$D_3 \equiv (v_1 N_1°)(i_1 z_2) \sin \phi_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Interpreting this formula it will be seen that D_3 largely depends on the power factor, or at least on one component of same. Although $v_1 N_1°$ reaches a very high value on normal load, and although i_1 is then large, yet ϕ_1 is so small that D_3 becomes very small. D_3 is a "shunt torque," and has all the characteristics of the torque of a badly designed alternate-current shunt motor, *i.e.*, of a shunt motor in which the E.M.F. at the terminals of the armature circuit (here $b\,b$) is of same phase as the E.M.F. at the terminals of the field circuit (here z_1). The torque of such a motor is always great at starting when the power factor is poor, and decreases with improving power factor.

This conception of component power factors of two circuits connected in series affords an easy means of showing the manner in which the phase compensation of this motor is brought about. Reverting to Fig. 15, it will be recognised that the influence of the speed alone can hardly reduce ϕ_1 to zero, and it can on no account diminish ϕ_3. With increasing speed i_1 must diminish, causing P_3 to decrease, thus reducing the magnitude of ϕ, without, however, in the least affecting the magnitude of ϕ_3. Every tendency of i_1 to approach P_1—in other words, every attempt of e_{r_3} to get into direct phase opposition with e_1—is counteracted by the fact that as i_2 diminishes the predominance of i_0 in determining, the phase of i_1 must increase, so that the phase of i_1 must with decreasing i_2 approach the phase of i_0; in consequence ϕ_1 must increase when very small values of i_2 are reached. The maximum possible armature power-factor component corresponding to ϕ_1 is determined by the magnitude of i_0, of θ_3, and of the time constant of the rotor; the smaller i_0, θ_3, and tan. ω_2 the better will be the power factor obtainable by speed effect alone; in no case, however, can this effect alone reduce ϕ to zero unless θ_3 is very large, which is never the case.

The field power-factor component corresponding to ϕ_3 is influenced by that E.M.F. (e_{r_1}) which is generated in the rotor along $b\,b$ and by rotation in $v_1 N_1°$. This particular E.M.F. did duty as *exciting E.M.F.* in the self-excited shunt induction motor shown in Fig. 1; here, in this separately excited series induction motor, it does duty as *compensating E.M.F.* The diagram in Fig. 22 will make this clear.

In Fig. 13 it is the stator current i_1 which, passing through the stator winding z_3, excites the motor field N_3; this field induces in the field winding a back E.M.F. (e_3) lagging 90° behind N_3. Now e_3, which may be taken to include the local reactance e_3', together with the E.M.F. consumed in the resistance of the field winding and designated by $i_1 w_3$ are the only E.M.F.'s which must be

balanced by P_3 in the motor shown in Fig. 13. As soon, however, as the winding z_3 is transferred to the rotor, as is the case in Fig. 21, and combined with the armature winding on that member or not, then P_3 must not only balance e_3 and $i_1 w_3$, which, by the way, now becomes $i_1 w_2$, but it must also balance that E.M.F. (e_{r1}) which has just been referred to, and the phase relation of which with regard to $v_1 N_1^o$ was fully gone into when discussing the diagram in Fig. 3. For the particular direction of rotation chosen in Figs. 15 and 22, this e_{r1} will be of opposite phase to $v_1 N_1^o$, so that P_3 will have, in the case of the motor shown in Fig. 21, to oppose O Q, which is the vectorial sum of $\overline{e_3} + \overline{i_1 w_2 + e_{r1}}$. It is seen that e_{r1} tends to oppose e_3 thus compensating the motor, P_3 now moves to P_3', and leads i_1 by $-\phi_3$, notwithstanding that ϕ_1 has still a positive value.

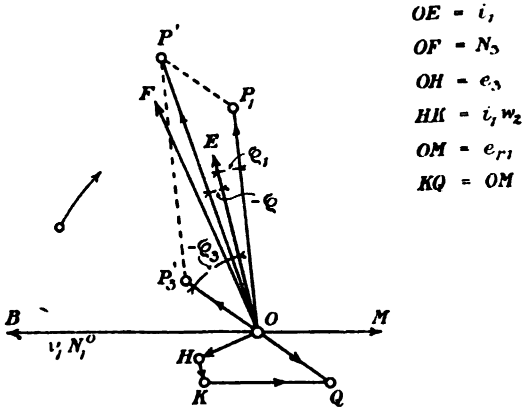


FIG. 22.

Under any normal working conditions $v_1 N_1^o$ is large, e_{r1} depends on $v_1 N_1^o$ and the speed; obviously, then, very high power factors may in this motor be reached at fairly low speeds and when N_3 and therefore e_3 are still large. The number of rotor turns in which the compensating E.M.F. is generated is constant, so that unity power factor can only be reached for one particular speed, which will be the higher the greater is the transformation ratio between z_1 and z_2. But this transformation ratio can be varied by varying the number of turns of z_1 or by interposing a variable ratio transformer between z_1 and $b\,b$, in which case unity power factor will be reached at a speed varying with the variation of the transformation ratio. As soon as such a series transformer is used the motor is designated as fully compensated, for then its power factor is under control.

That small E.M.F. (e_{r2}) which, in the case of the shunt motor shown in Fig. 1, rendered such valuable service by partly compensating the machine, sinks into comparative insignificance in the case of *series* induction motors. The secondary leakage field N_2', to which e_{r2} is

primarily due, is cophasal with i_2 or O D in Fig 15, the E.M.F. generated by its agency is of opposite phase to i_2, and therefore nearly in phase with P, e_{r_2} adds a trifle to the terminal voltage of the motor when $\phi_2 = 0$. When ϕ_2 is positive, then one component of e_{r_2} increases the reactance of the rotor along $b\,b$; when ϕ_2 is negative, then one component of e_{r_2} diminishes that reactance. This E.M.F. is not of vital importance comparatively, and has therefore been omitted in Fig. 22.

After this necessary digression we can return to our equations (15) or (16), and say that since ϕ_1 can never become zero, or δ_{11} ever equal 90°, then D_3 can never become zero or negative (although it can and does get very small), for δ_{11} corresponds to $\measuredangle$ B O K in Fig. 3, and it has been shown that the torque in question must be positive as long as $\measuredangle$ B O K < 90°. The total torque of the motor shown in Fig. 21 is at starting or in normal operation—

$$D_d = D_1 - D_2 + D_3 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

since D_2 may safely be disregarded we may write with sufficient approximation—

$$\begin{aligned} D_d &\equiv D_1 + D_3 \\ &\equiv N_3 (v_0 i_1 z_1) \cos(\theta_3 + \gamma) + (v_1 N_1^{\circ})(i_1 z_2) \cos \delta_{11} \quad . \quad . \quad (18) \end{aligned}$$

Comparing D_b with D_d we get—

$$\frac{D_b}{D_d} = \frac{(v_3 N_3)(v_0 i_1 z_1) \cos(\theta_3 + \gamma)}{N_3 (v_0 i_1 z_1) \cos(\theta_3 + \gamma) + (v_1 N_1^{\circ})(i_1 z_2) \cos \delta_{11}}$$

$$\frac{D_b}{D_d} = \frac{v_3}{(v_1 N_1^{\circ})(i_1 z_2) \cos \delta_{11}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

which means that not only is the "series torque" D_1 in the partly or fully compensated series induction motor (Fig. 21) greater than the corresponding torque of the series induction motor (Fig. 13), but that the former yields in addition a "shunt torque," which, although great at starting, decreases very much as the motor speeds up, but never becomes zero, and is under normal working conditions still of such a magnitude as to make it necessary to take it into consideration.

It will be clear without further explanation that the weight efficiency of Fig. 21 is also much superior to that of Fig. 20. At starting this may mean an advantage of as much as 30 per cent., whereas this percentage drops to perhaps 5–10 under normal load conditions. When Fig. 21 is operated with the help of a series transformer independent of the motor the torque D_3 will, for a given power factor, be as a rule greater than in cases where such a transformer is not used.

As soon as the line voltage exceeds that relatively small terminal pressure, which may not be exceeded in the case of Fig. 20, then the latter requires a reducing transformer capable of dealing with the whole energy required by the motor, thus placing Fig. 20 at a certain disadvantage, which, however, is not so serious as appears at first sight. From the point of view of traction it is principally the weight efficiency

of the motor itself which matters, for the space in which a traction motor must fit is so extremely limited that it is often difficult to squeeze into that space a sufficiently large continuous-current motor. If a reducing transformer independent of the motor is required, then the space difficulty does not enter into the problem, and there only remains the consideration of the greater dead weight to be carried. Since the maximum output of such a transformer is only required for extremely short periods, and is greatly in excess of the normal, it can be made very light.

The critical voltage for the conduction motor of Fig. 20 is somewhere about 350 volts. It has been said that the series induction motor can be wound for 3,000 volts, but it is a question whether it is advisable to go to that high figure for traction purposes. When the induction motor is wound for this pressure then its weight efficiency decreases very much indeed, and its reliability also decreases greatly; I feel inclined to fix the limit for this case at 1,000 volts or thereabouts, particularly as regulation along the armature axis is also necessary in the case of the series induction motor in order to take full advantage of all its qualities. This regulation can be carried out either on the primary or the secondary winding disposed in the armature axis, and may, amongst other things, be made use of to increase D_3 at starting.

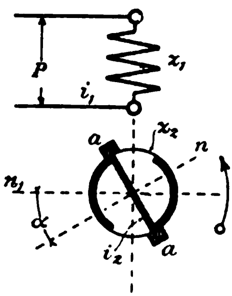


FIG. 23.

Fig. 21 is one example of a single-phase motor with two effective armature and two effective field axes per pole pair. At starting both sets of axes are very effective; in normal operation one set is very much less effective than the other. The material in this machine is therefore better utilised than in any of the preceding motors, and particularly is this the case at starting. Since D_3 is only a more or less transitory torque, it was not thought necessary to take it into account when fixing upon the proper name by which this machine ought to be known.

(*e*) *The Self-excited partly Compensated Series Induction Motor* (Fig. 23), —It will, no doubt, be of interest to note that this, the oldest representative of the single-phase commutator motors, bears such a striking resemblance to that type of machine which has become so fashionable of late, and which is shown in Fig. 21, as to justify one in saying that they are practically identical. It will be well in passing also to draw attention to that other striking resemblance which exists between the continuous-current neutralised series conduction motor shown in Fig. 12 and its alternate-current induction prototype shown in Fig. 23. In Fig. 23 as in Fig. 12 the series field ampere-turns are $A\,T_f = \frac{4\,\alpha}{360}\, i_2 z_2$ and the armature ampere-turns are $A\,T_a = \left(1 - \frac{4\,\alpha}{360}\right) i_2 z_2$, corresponding respectively to $i_1 z_2$ and $i_2 z_2$ of Fig. 21. In Fig. 23, as in Fig. 21.

there is a transformer field $v_1 N_1^\circ$ along the armature axis, so that the torque conditions in Fig. 23 are practically those obtaining in Fig. 21, and we can write—

$$
\begin{aligned}
D_1 &\equiv N_3(i_2 z_2)\left(1 - \frac{4a}{360}\right)\cos\theta_3 \\
&\equiv N_3\,\overline{(i_1 z_1 - i_0 z_1)}\left(1 - \frac{4a}{360}\right)\cos\theta_3 \\
&\equiv N_3(v_0 i_1 z_1)\left(1 - \frac{4a}{360}\right)\cos\theta_3 \quad \ldots\ldots \quad (20)
\end{aligned}
$$

also—

$$
\begin{aligned}
D_3 &\equiv (v_1 N_1^\circ)\frac{4a}{360}(i_2 z_2)\cos\delta_{12} \\
&\equiv (v_1 N_1^\circ)\frac{4a}{360}\overline{(i_1 z_1 - i_0 z_1)}\cos\delta_{12} \\
&\equiv (v_1 N_1^\circ)\frac{4a}{360}(v_0 i_1 z_1)\cos\delta_{12} \quad \ldots\ldots \quad (21)
\end{aligned}
$$

Omitting D_2 as safely negligible, we have—

$$D_t = D_1 \pm D_3 \quad \ldots\ldots\ldots\ldots \quad (22)$$

Comparing equations (13) and (20) and the corresponding Figs. 21 and 23, it is seen that the phase difference between i_2 and N_3 is only θ_3 in Fig. 23, because N_3 is excited by i_2 and not by i_1 as in the case of Fig. 21, and where this particular phase difference is $(\theta_3 + \gamma)$ because N_3 is excited by i_1. This advantage of Fig. 23 may at high speeds become very appreciable, for then γ is large if i_0 is at all large.

Comparing equations (15) and (21) it is seen by reverting to Fig. 15 that, except at starting, when δ_{11} may be about equal to δ_{12}, the latter angle must always be greater than the former, so that the "shunt torque" of Fig. 23 will, except perhaps at starting, be less than the corresponding torque of Fig. 21. Whereas it was seen that D_3 in Fig. 21 could never become negative, it is clear that D_3 in Fig. 23 does become negative as soon as i_2 leads e_1, and D_3 is zero only when i_2 is cophasal with e_1. This greatly limits the no-load speed of Fig. 23 and renders it unsuitable for speeds greatly in excess of the synchronous.

Apart from these not very important differences, the operation of the two motors is identical, and the partial phase compensation takes place in exactly the same manner in both cases. The weight efficiency of Fig. 23 is a little less than that of Fig. 21, and rather better than that of any of the other machines which have been considered. The field current cannot be varied independently of the armature current, but by moving the brushes the ratio of field to armature ampere-turns can be varied. A winding fed by i_1 placed in series relation with z_1 and disposed on the stator in the axis n_1 may serve to weaken or strengthen the effective field ampere-turns by connecting it up in opposition to or in the same direction as those field ampere-turns which are disposed on the rotor, and on that part of its circumference which in Fig. 23 is

distinguished by thin lines. Such a stator winding could be included in the circuit of the brushes *a a* instead. This machine cannot be fully compensated. Like its predecessor, this motor has two effective armature and two effective field axes per pole pair, one set of axes losing their effectiveness with rising power factor, and even being capable, at high speeds, of developing a negative torque.

(*f*) *The* (*Separately Excited*) *2-phase partly Compensated Shunt Induction Motor* (Fig. 24).—In order to complete this survey it is proposed to deal briefly with a 2-phase machine. This one example will no doubt suffice to bring out the essential difference which exists between single and polyphase motors. The machine in Fig. 24 is shown as being provided with a commutator rotor; that member may be of the slip-ring or the squirrel-cage type, both in this case and in that of Fig. 1.

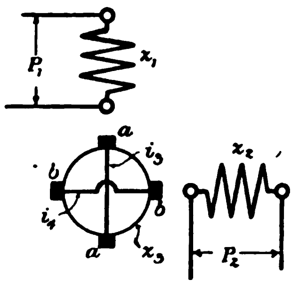


FIG. 24.

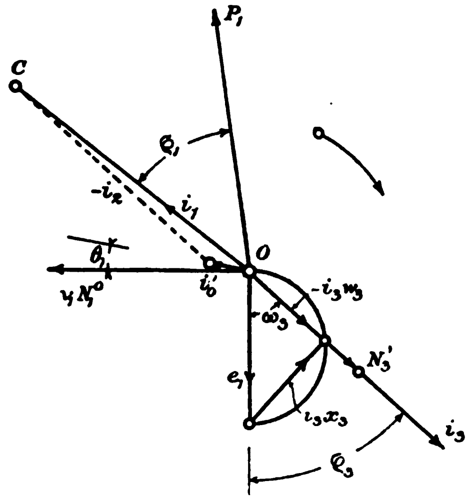


FIG. 25 (Phase I.).

Assume the rotor to be held fast, the two stator windings z_1 and z_2 to be displaced in space by 90°, let the rotor have the same time constant in all directions, then Figs. 25 and 26 will be the respective diagrams for the two transformers embodied in the motor but placed in non-inductive relation to each other along the axes *a a* and *b b*. These two diagrams show the correct relative phase relation of the various vectors in the two axes. The true diagrams for each axis would of course be on the pattern of that shown in Fig. 2; the simplified diagrams in Figs. 25 and 26 will, however, be perfectly intelligible at this stage. The same letters do not here refer to the same quantities: this was the case for all the figures dealt with up to now, and although such uniformity is very desirable throughout, it is unfortunately not possible to extend it to this case; still, it is hoped that the common features, for instance in Figs. 1, 21, and 24, may be readily recognised.

In Fig. 24 the rotor current i_3 can yield a torque D_1 with the transformer field $v_2 N_2^{\circ}$, provided that i_3 and $v_2 N_2^{\circ}$ are not in time quadrature. It will be noted that $v_2 N_2^{\circ}$ here takes the place of N_3, which, in Fig. 1, for instance, was excited by means of a rotor winding and not by means of the stator winding z_2 as in Fig. 24. The E.M.F. e_{r1} responsible for N_3 was in Fig. 1 generated in the motor itself and not taken from an outside source as in Fig. 24; at the same time the phase relation between P_1 and P_2 in Fig. 24 is the same as the phase relation between P and e_{r1} in Fig. 1. Then, again, i_3 of Fig. 24 takes the place of i_2 in Fig. 1.

Similarly i_4 can yield a torque D_2 with the transformer field $v_1 N_1^{\circ}$, provided that i_4 and $v_1 N_1^{\circ}$ are not in time quadrature. The rotor current i_4 in Fig. 24 corresponds to the rotor current i_3 in Fig. 1, but is

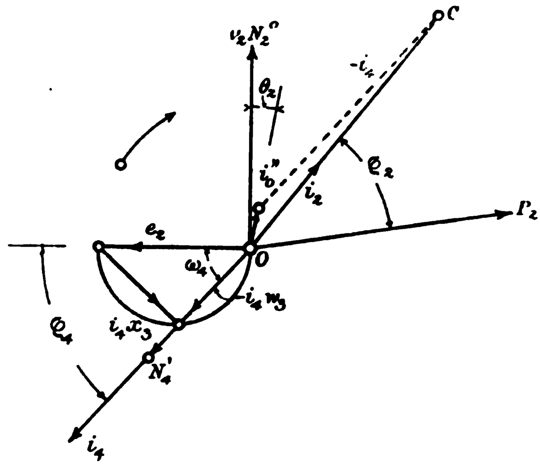


FIG. 26 (Phase II.).

in Fig. 24 induced in the rotor from z_2, and not produced therein by an E.M.F. generated in the rotor as in Fig. 1. Owing to this circumstance the phase of i_4 differs greatly from the phase of i_3 in Fig. 1, and tends, with increasing speed, to get into time quadrature with i_3 of Fig. 1; this point is of great importance, and will be more clearly recognised by reference to Fig. 28. As to $v_1 N_1^{\circ}$, it is in both cases due to a stator winding z_1.

From Figs. 25 and 26 it will be seen that D_1 is dependent on the phase angle ϕ_3, which itself only depends on the time constant of the rotor as long as the rotor is stationary. D_2 depends on ϕ_4, which is similarly governed. We can write—

$$D_1 \equiv (v_2 N_2^{\circ})(i_3 z_3) \cos \phi_3 \quad \ldots\ldots \quad (23)$$

and—

$$D_2 \equiv (v_1 N_1^{\circ})(i_4 z_3) \cos \phi_4 \quad \ldots\ldots \quad (24)$$

It can be ascertained in the manner explained in connection with Figs. 4, 5, and 6, that D_1 and D_2 are of same direction, thus—

$$D_f = D_1 + D_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (25)$$

and since the motor is symmetrical in all directions and both its stator windings are fed by E.M.F.'s of equal magnitude, then—

$$D_1 = D_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (26)$$

As a rough example, if $\phi_3 = \phi_4 = 30°$ and the motor takes in each phase six times the normal current when switched on to the full-line pressure with the rotor at rest, then it must first be ascertained in the

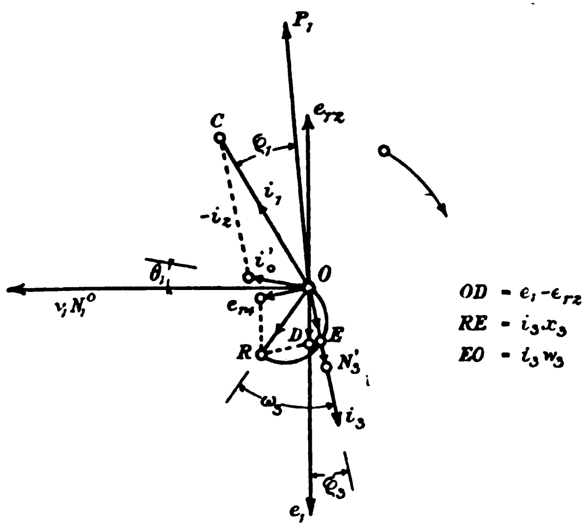


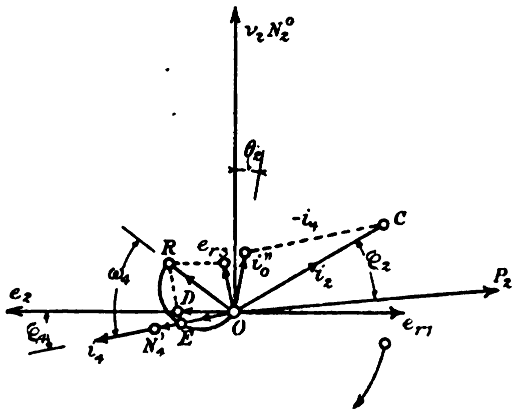
FIG. 27 (Phase I.).

manner previously set forth to what value $N_1°$ and $N_2°$ must fall. Assume that we find $v_1 N_1° = 0{\cdot}4\, N_1°$, then the torque at starting is $= 0{\cdot}4 \times 6 \times 0{\cdot}866 = 2{\cdot}08$, or $2{\cdot}08$ times the normal.

As the machine speeds up the conditions in each rotor axis undergo a material change. Taking the axis *a a* first, and reverting to Fig. 27, it will be found that to begin with an E.M.F. (e_{r_2}) will appear which will be generated by rotation of the rotor conductors in $v_2 N_2°$, let the direction of that rotation be such that e_{r_2} is cophasal with $v_2 N_2°$ as indicated in Fig. 27—this e_{r_2} is the *back E.M.F.* in the armature axis *a a*. Secondly, by rotation of the rotor conductors in the rotor leakage field N_4' (see Fig. 28) cophasal with i_4 and coaxial with $v_2 N_2°$, an E.M.F. (e_{r_4}) is obtained as shown in Fig. 27—this is the *compensating E.M.F.* for the armature axis *a a*. The resultant E.M.F. in that axis is now the vectorial sum of $\overline{e_1 + e_{r_2}}$, which is O D and of O D $+ e_{r_4}$, which is O R. Since

the time constant of the rotor remains constant throughout, then i_3 will always lag by a constant angle ω_3 behind whichever happens to be the resultant E.M.F. in the rotor along the axis *a a*. But it is seen that with increasing speed this resultant leads e_1 more and more, hence ϕ_3 will decrease with increasing speed. For nearly synchronous speed ϕ_3 is practically zero, and the torque can be very great, for at that time both transformer fields are also at their full strength. Notwithstanding, the power factor cannot be unity, because the effect of the stator magnetising current cannot be compensated for unless i_3 leads e_1; this is why these motors are referred to as partly compensated. At synchronous speed e_{r_2} is nearly equal to e_1, and e_{r_4} nearly equal to e_3' or $i_3\,x_3$.

The process for the axis *b b* is exactly similar, and the diagram in Fig. 28 will doubtless suffice.



$OD = e_2 - e_{r_1}$
$RE = i_4\,x_3$
$EO = i_4\,w_3$

FIG. 28 (Phase II.).

Now this machine represents truly ideal conditions, for it is fully utilised along two axes per pole pair. It operates at normal load (and at starting if ϕ_3 and ϕ_4 are kept small) with two fully effective armature and two fully effective field axes. This is the true reason why polyphase machines are lighter than corresponding single-phase or continuous-current machines; as a matter of fact, the whole of the material is utilised twice over. If Fig. 1 is changed into a 2-phase motor, then it will yield, roughly speaking, 2·1 times as great an output for the same heating, provided only that the stator copper be doubled by the addition of a winding $z_2 = z_1$. The same two fields per pole pair will be present in both cases, but the rotor will now carry a larger current in the axis *b b*—as a matter of fact, rather more than twice as large a current in the 2-phaser as in the single-phaser. The C^2R loss will not, however, be much, if at all, greater, for the two rotor

currents are in the case of the 2-phaser not only displaced by 90° in space as in the single-phaser, but also by 90° in phase. In order to utilise the 2-phaser to the full it is advisable to allow some 15 per cent. more rotor copper.

The weight efficiency of a 3-phase motor is practically equal to that of a 2-phaser.

(*g*) *Methods of Starting the Self-excited (partly or fully) Compensated, Single-phase, Shunt Induction Motor.*—It has been stated that the motor in Fig. 1 will not start from rest; this is correct, but at the same time it is known that there exist many ways of artificially starting such machines, and it is important to decide which of these is likely to be the most useful for heavy work, such as traction, lifts, and cranes.

The best is obviously the one which will yield the greatest torque per ampere. The method of starting such a motor by temporarily converting it into a motor of the type shown in Fig. 21 has been previously

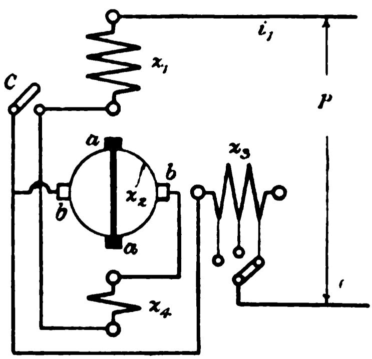


FIG. 29.

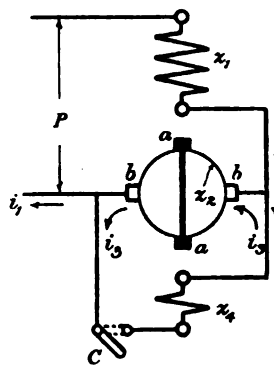


FIG. 30.

described by me,* and it would appear from what has been said that this method is by far the best as long as single-phase currents only are obtainable, since the motor then starts with two effective armature and two effective field axes. The connections necessary for starting and for subsequently converting the machine into a self-excited and compensated shunt induction motor are shown in Fig. 29, where the closing of the switch C at about synchronous speed is all that is necessary in order to effect the conversion in the simplest possible manner. It is seen that after this conversion has been accomplished the stator current does not pass either through the rotor or through z_4.

Fig. 30 shows a recently proposed modification of Fig. 29, the object stated being to provide a compound wound motor. It was suggested under the impression that the shunt excitation was provided by z_4 and in the hope that i_1 would take its way through the rotor *viâ b b*. It has, however, already been demonstrated that the excitation is provided

* *Journal, Institution of Electrical Engineers*, vol. 36, p. 324, 1906.

by e_{r1} *generated in the motor itself*, and that z_4 only provides the *compensating E.M.F.* As soon as switch C is closed, the exciting current i_3 (see Fig. 3) remains practically as constant as it does in the case of Figs. 1, 7, 8, or 29. It may be seen from Fig. 3 that i_1 and i_3 are all but cophasal. If, then, the transformation ratio between z_1 and z_2 is unity, or is made unity by the use of a series transformer between z_1 and $b\,b$, then the exciting current at full load would be about three times as large as at no load, and the speed would either drop to $\frac{1}{3}$ of the normal, or the motor field density would rise to saturation if i_1 did really take its way through the rotor *viâ* $b\,b$. Neither happens, for practically all the stator current i_1 passes through z_4. Strictly speaking, it divides between the circuits $b\,b$ and z_4 in inverse ratio to their respective impedances, and since the impedance of z_4 with its very few turns is very low as compared with the impedance of the rotor along $b\,b$, practically the whole of i_1 passes through z_4; it should be remembered that in addition i_1 opposes i_3 through z_4. The current through z_4 is always practically equal to the vectorial difference $\overline{i_3 - i_1}$, and calling this difference i_{31} it is easy to see that i_{31} may be smaller, equal, or larger than i_3, since i_3 is practically constant and i_1 is variable, and may, according to the transformer ratio z_1 to z_2, reach values exceeding those of i_3. The direction of i_{31} may be the same as that of i_3, for small values of i_1, or opposed to the direction of i_3 when i_1 is large. The change from the connections shown in Fig. 29 to those shown in Fig. 30 does not alter the character of the motor, the machine is in both cases one which is started as a series induction or what used to be called a "repulsion" motor, and is then converted into a self-excited and compensated shunt induction machine. The motor shown in Fig. 29 has the further advantage of permitting very gradual starting with the help of z_3 and of allowing the terminal voltage to be boosted at starting with the help of z_4.

(*h*) *Comparison between Continuous- and Alternate-current Motors.*—It only remains, in conclusion, to establish a link between any one of the alternating-current motors which have been dealt with and the continuous-current motor, when the important question of the weight efficiency of the various alternating-current motors as compared with corresponding continuous-current machines can be readily deduced for any given case by the aid of the formulæ which have been given. In applying these formulæ it must be borne in mind that, as far as torque is concerned, it is not the maximum but the effective values of current and magnetic flux which are of importance. Now, in the case of the continuous-current machine, these effective values are equal to the maximum values, whereas this is not the case where alternate currents are concerned. The effective value of the alternate current, or that measured by our instruments, is directly comparable to a continuous current of same magnitude, and there is no occasion to consider the maximum value of the alternate current, but the maximum value of the alternating flux *must* be considered. Assuming that the variations of the alternating current and flux follow a sine law, then the

maximum value of the flux at any point will be $\sqrt{2}$ times greater than its effective value. It is this maximum and not the effective value of the alternating flux which determines the iron cross-section of the motor. It follows that if the same maximum flux density be allowed for the continuous-current and the alternating-current machine that the latter must unavoidably be $\sqrt{2}$ times heavier for the same output as the "corresponding" continuous-current machine. By "corresponding" continuous-current machine is meant one in which the proportion of armature to field ampere-turns is the same as that of the alternate-current motor to which it is being compared; both machines operating with the same terminal voltage, yielding the same B.H.P. at the same speed, and working under the same general conditions. This increased iron cross-section necessitates more copper because the mean length of all the windings is thereby increased so that one is not far wrong in saying that the alternating-current machine must be about 50 per cent. heavier than the "corresponding" continuous-current machine of the same output, the same maximum flux density, and the same copper section. Remembering that the continuous-current machine has only one effective armature and only one effective field axis per pole pair it is at once seen that an alternating-current motor, such, for instance, as most of the single-phase motors, must be at least 50 per cent. heavier than the "corresponding" continuous-current motor, and that it is a physical impossibility to improve on this state of things unless the constants of the alternating-current machine are made very different from those of the continuous-current motor with which it is being compared, or unless the alternate-current motor is made with more than one effective armature and with more than one effective field axis per pole pair. If, for instance, the alternating-current motor is made with two effective armature and two effective field axes, as is the case with the 2-phase motor, then the alternating-current motor will be at least 30 per cent. lighter than a continuous-current machine of same output. In assessing the advantage of the 2-phase motor over the continuous-current motor in point of weight at some 30 per cent. it must be borne in mind that this type of machine allows of a rather better utilisation of the active materials than is possible in the case of a corresponding single-phaser. The difference in favour of the 2-phaser being some 10–15 per cent.

The foregoing deductions are broadly correct; they do not, however, take all the determining factors into account, but only give a limit. For a strict comparison each case must be treated separately, and recourse must be had to the formulæ which have been given. It should be remembered that the frequency for which the alternate-current motor is designed directly determines the permissible value of the total flux and of the maximum densities; down to about 15 $\sim$ a reduction of the frequency generally means an increase of the weight efficiency of a commutator motor. It will also be clear from the foregoing that the flatter the wave of the alternate current in use the better the weight efficiency of the motor. The effect of phase differences between the torque-producing factors, the effect of the power factor

and the like, are all against the alternating-current machine. These disadvantages do not, however, always exist to the same degree; moreover, they are often counterbalanced, or even entirely outweighed, by weight advantages, such as the absence of commutators or even of sliprings.

The successful design of continuous-current and alternating-current motors naturally follows widely differing lines, and this is why the weight efficiency of some of the alternate-current motors on the market can sometimes be increased beyond the limit given above, even in spite of the use of heavy commutators. Many considerations lead one, in the case of alternating-current motors, to the choice of lower magnetic densities and of a lower total flux than that which was adopted as a limit basis for the comparison which has just been made. The alternating-current machine is generally built with a greater number of ampere conductors per unit length of armature periphery than is deemed advisable in the case of continuous-current machines. Not the least amongst the determining factors for this course is the fact that iron losses present one of the greatest difficulties against which the designer of alternate-current motors has to contend, whereas C^2R losses are no more difficult to deal with in alternating than in continuous-current machines. Other important reasons for this tendency are the necessity of keeping the magnetising current low in the case of shunt motors, and correspondingly the necessity of reducing the reactance voltage of the field winding as far as possible in the case of series motors. The choice of a low total flux is also imperative in the case of commutator motors with a series characteristic, for it leads to a material improvement in the commutation. Every alternate-current motor must be neutralised, whether it has a powerful armature or not, whereas a neutralising winding is generally avoided in continuous-current machines. Its use certainly allows of the weight being reduced but increases the cost.

Taking every factor into account, it may be stated as a basis for comparisons that the weight of a commercial continuous-current motor as compared with that of a commercial 50-$\sim$ single-phase self-excited partly compensated shunt induction motor with sliprings is as 1 : 1·5, and that the weight of the continuous-current machine as compared with a commercial 50-$\sim$ 2-phase, partly compensated shunt induction motor with sliprings is as 1 : 0·7. In cases where a commutator is used the weight is, of course, increased, but where such a commutator can be fully utilised in a rational manner, as is, for instance, the case with constant-speed machines, this increase in weight is fully compensated for by a correspondingly increased output. In other cases the use of a commutator leads to an increase in weight without a corresponding increase in output as in the case of motors with a series characteristic. This increase in weight is from 20 to 25 per cent., varying with the type of machine.

The difference in the design constants between commercial continuous and commercial alternate-current motors is most marked in the

case of the continuous-current series traction motor as compared with the neutralised single-phase series conduction machine. The armature ampere-turns of the latter very greatly exceed those of the former in a motor of equal output and same speed, with the result that the weight discrepancy between the two can often be reduced below the limit given, although this particular type of alternate-current motor requires a heavy commutator. Thus, with a current wave approximately following the sine law *and* a periodicity of 25 $\sim$, this alternate-current machine for the same heating need only be some 35 to 40 per cent. heavier than a continuous-current series motor of same output, speed, and voltage.

It may be added that Professor S. P. Thompson's "output coefficient" formula—

$$d^2 l = \frac{60{\cdot}8 \times 10^{10}}{B_g \times q_1 \times \psi} \cdot \frac{\mathrm{KW}}{\mathrm{r.p.m.}} = k\,\frac{\mathrm{KW}}{\mathrm{r.p.m.}}$$

yields very good results for all alternate-current motors. When the motor under consideration has more than one effective armature and more than one effective field axis per pole pair, then the value of k must be worked out separately for each pair of effective axes with the help of the formulæ here set forth, afterwards adding these values. Thus in the case of a 2-phase motor, for instance, we should write—

$$d^2 l = (k' + k'')\,\frac{\mathrm{KW}}{\mathrm{r.p.m.}}.$$

In Professor Thompson's formula—

B_g = mean effective flux density at the pole face.

l = gross length of armature core.

ψ = ratio of equivalent pole span to pole pitch.

q_1 = number of ampere conductors per unit length of periphery.

By introducing one more constant, taking the mechanical features of a particular design into consideration, one arrives at very satisfactory weight curves for whole series of similar machines.

Discussion.

Mr. Orsettich.

Mr. R. Orsettich: I have had some experience in constructing a single-phase motor of Mr. Fynn's design, and am able to show some illustrations of the details and construction of this motor. Mr. Fynn's motor has several advantages from a manufacturing point of view. It is possible to use the same patterns and tools as are used for standard polyphase motors. As there is not a great demand for single-phase motors, if such a motor requires special patterns or dies (like in the case of series motors) it is very much against it. This is a point not fully realised by many inventors.

The single-phase motor has, no doubt, a good field in heavy traction work, but for ordinary power purposes there is no satisfactory motor

on the market yet, although a large number of designs and patents have been brought out during the last fifteen years. Even should a satisfactory motor be built now, I do not believe it will replace polyphase motors to any extent, and it will have to find a new field of its own. Mr. Orsettich.

The reasons of the non-success of the single-phase motor I attribute first to the large number of intricate patents covering practically the entire range, so that any design must necessarily come within the claims of another firm. Secondly, to the excessive cost of production due to the small market and complicated design. Finally, to the uneconomical performance of most motors—like sparking at the brushes, wearing of commutator, increased cost of upkeep due to complicated gears adopted in connection with them.

Almost all these difficulties have been overcome in the latest type of motor proposed by Mr. Fynn. The starting torque is large, and at full-load starting torque almost only full-load current is taken by the motor. The power factor at full load is round 0·95, and the efficiency is about 70–72 per cent. for a 5-H.P. motor. The starting gear is very simple indeed, as a motor up to 5 H.P. requires only a change-over switch to cut out the starting winding, and a large motor is started in a similar way by cutting off the starting winding in sections by means of a controller. No resistance or choking coils are used.

The sparking is very small indeed, and the brushes used are ordinary low-resistance carbons. It is in these practical points that the Fynn motor, in my opinion, is superior to all other types.

Dr. D. K. MORRIS: The author promised at the outset of his paper to deal with the theory, considering only the real fluxes, and not the imaginary flux components. Unfortunately, it is not possible apparently to deal with the true flux, but only separately with various components of this. For there is in all electric motors, after all, only one belt of flux per pole which actually cuts rotor conductors, and one belt of induced currents for each pole. In single-phase motors this belt fluctuates largely while it revolves with the speed of synchronism. It is curious how in dealing with the theory of electric motors so little is said of the actual cause of their torque. The torque is due without exception to the pull and side-push of magnetic flux falling obliquely on the surface of the rotor, the obliquity arising solely from the "surface field" due to the rotor currents. Induced or inserted rotor currents produce a flux tangential to the rotor surface, and it is this which deflects the incident flux. If only one could get at the actual flux band then the torque due to the currents it induces is at once obtained by methods which Mr. Lister and myself have already pointed out. The rotor reaction shifts back the flux band, and the inductance of the bars still further shifts it in reality; but where the self-induction of the rotor and connections or external circuit is negligible, it is even true to say that the belt of rotor currents is always exactly below and proportional both in space and time to the true flux band of magnetic induction which really cuts the rotor bars. Dr. Morris.

Dr. Kapp. Dr. GISBERT KAPP: The author has shown, and critically compares, various types of monophase motor, but that motor which he declares to be the best is a 2-phase motor. Since in this case, as well as in the case of a direct-current motor, the supply of power is continuous, whereas in the case of a monophase motor it is pulsating, one can understand that the latter is at a disadvantage as regards weight and efficiency. There may be another disadvantage connected with single-phase working, namely, greater tendency to skid the wheels; on this point I should like to get the author's opinion. We know that in a steam locomotive the skidding point is reached sooner than in a direct-current or 3-phase electric locomotive built for equal mean tractive effort. The reason is that skidding depends on the maximum value of the torque, whilst tractive effort depends on the mean value. The two are alike in the case of an electric locomotive, but in a steam locomotive there is considerable difference, and the net result of this is that the tractive effort which an electric locomotive can exert is some 30 or 40 per cent. greater than that of a steam locomotive of equal adhesive weight. Now, in a single-phase system we have a still greater difference between maximum and mean torque, but it is possible that owing to the very quick alternation between the two (some 30 to 50 times a second) and the inertia of the rotating masses combined with a certain amount of elasticity in the system between the armature wires and the tyres of the driving wheels, the tractive force has almost become steady by the time it reaches the rails. Perhaps the author in his reply could give some information on the subject.

Mr. Taylor. Mr. A. M. TAYLOR: Can the author give us any information as to the relation between torque and speed for single-phase traction motors?

Dr. Hay. Dr. A. HAY (*communicated*): Mr. Fynn's able and suggestive paper will form a valuable supplement to his previous well-known work on single-phase commutator motors, embodied in a number of earlier publications. The clear way in which he connects the various types of single-phase commutator motors and exhibits their relationships to one another and to continuous-current motors is deserving of all admiration, and should prove extremely helpful to those who have to make a thorough study of this type of motor.

Considering now some of the details of the paper, it would appear that in the first two pages Mr. Fynn is dealing with currents instead of ampere-turns, although it is clear from the context that the latter are meant and not the former. As this may prove confusing to some readers, I would suggest the substitution of $z_1 i_1$, $z_1 i_0$, $z_1 i_2'$ for i_1, i_0, and i_2' respectively at the foot of page 181, and of $z_2 i_2$ for i_2 at the top of page 182, as well as the substitution of "ampere-turns" for "current" throughout.

On page 182 it is stated that N_1° stands for the flux of mutual induction, but the symbol used for this quantity throughout the bulk of the paper is not N_1°, but $v_1 N_1^\circ$. The reference to v_1 given at the top of page 184 does not seem very clear, and perhaps Mr. Fynn will explain

a little more in detail how the two quantities N_1° and $v_1 N_1^\circ$ are related to each other. Dr. Hay.

It is to be regretted that Mr. Fynn has become so absorbed in the development of his own method of dealing with single-phase commutator motors that he deals out but scant justice to other workers in the same field. On page 187 he outlines a theory which, we are informed, "has perhaps been most generally accepted." He then proceeds to demolish this theory—a task requiring very little effort—and to show the soundness of his own.

The truth of the matter, however, is, that the theory which is described by Mr. Fynn as being the "most generally accepted" is so extremely puerile that, to my knowledge, it has never had a single advocate. In fact, I have not the slightest hesitation in saying that this "generally accepted theory" exists in Mr. Fynn's imagination only, and is not to be found anywhere in the already extensive literature of the subject.

Mr. Fynn's imaginary theory is obviously the result of a very hasty and superficial study of the contributions of others to the theory of the single-phase commutator motor, and as his off-hand denunciation of rival theories is likely to create the impression that the work of other investigators is worthless, and that his own method is the only sound one, perhaps I may be allowed to explain in detail the nature of the fallacy of which Mr. Fynn is the victim.

In dealing with problems relating to transformers, induction motors, single-phase commutator motors, etc., two distinct modes of treatment are available; one of these is the older method involving the use of "total" (as distinguished from "leakage") self-inductances, and mutual inductance. As in this method we are concerned with the hypothetical fluxes which would be produced by the primary and secondary currents if acting singly or independently, the method may be conveniently referred to as the "hypothetical flux method." It is the method so sadly misrepresented by Mr. Fynn.

In the second method, which is the method adopted by Mr. Fynn, the introduction of which into alternate-current theory we owe to the late Dr. John Hopkinson, we consider only the actually existing fluxes. For this reason it may be termed the "actual flux method." The relation connecting the two methods is in reality very simple, as the actually existing fluxes are obtained by the superposition of the hypothetical fluxes. The principle of superposition is perfectly legitimate so long as the resultant actual flux does not saturate the cores.

Applying the two methods to the case of the motor studied by Mr. Fynn, and depicted in Fig. 1 of his paper, we have the following hypothetical fluxes: (1) The total stator flux F_1, which would be produced by the stator ampere-turns $i_1 z_1$ if there were no winding, and hence no ampere-turns, on the rotor; (2) the total rotor flux F_2 along the *a a* axis which would be produced by the rotor ampere-turns $i_2 z_2$ if there were no ampere-turns on the stator. In addition to these

Dr. Hay. hypothetical fluxes, we have the actually existing flux F_3 along the *b b* axis due to the rotor ampere-turns $i_3 z_2$.

In order to find the relation between F_1, F_2, F_3 and Mr. Fynn's fluxes, we have to superpose the hypothetical fluxes and find their resultant. The flux F_3 is clearly identical with Mr. Fynn's flux N_3. If we neglect iron losses, the vector F_1 will fall along O E in Fig. 3, since F_1 will then be exactly in phase with the ampere-turns $i_1 z_1$, and hence with i_1. Similarly F_2 will fall along O D. Now, of the total flux F_1, a large fraction ν_1 (where ν_1 is the reciprocal of the Hopkinson leakage coefficient for the stator) will enter the rotor, similarly a large fraction ν_2 of the flux F_2 will enter the stator; and the vectorial resultant of $\nu_1 F_1$ and $\nu_2 F_2$ will give us the actually existing flux common to stator and rotor, or Mr. Fynn's "flux of mutual induction." Let, then, a length $\nu_1 F_1$ be laid off along O E in Fig. 3, and a length $\nu_2 F_2$ along O D, and let the diagonal of the parallelogram constructed on those two lines as sides be found. The diagonal will fall along O B*, which is the direction of the "flux of mutual induction $v_1 N_1^{\circ}$." From this construction it will be at once seen that the hypothetical flux $\nu_1 F_1$ is a large multiple of Mr. Fynn's $v_1 N_1^{\circ}$. The ratio of the two is found very simply, for, since $v_1 F_1$ is produced by $i_1 z_1$, and $v_1 N_1^{\circ}$ by $i_0 z_1$, it follows at once that :—

$$\nu_1 F_1 = \frac{i_1}{i_0} \cdot v_1 N_1^{\circ}.$$

Not only is the hypothetical flux $\nu_1 F_1$ a large multiple of Mr. Fynn's $v_1 N_1^{\circ}$, but there is in addition a very large phase-difference between them, $\nu_1 F_1$ falling along O E in Fig. 3, and $v_1 N_1^{\circ}$ along O B (or O A), as already explained.

The flux $\nu_2 F_2$ (which falls along O D) is the vectorial difference of $\nu_1 F_1$ and $v_1 N_1^{\circ}$.

Lastly, the flux F_3 is, as already pointed out, identical with Mr. Fynn's N_3.

We have now established a complete correspondence between the fluxes of the hypothetical flux theory and the fluxes employed by Mr. Fynn.

Next, as regards the question of torque. Just as the actually existing fluxes were obtained from the hypothetical ones by superposition, so the actually existing torque may be obtained by a superposition of the hypothetical torque due to the hypothetical fluxes, the principle of superposition being quite as legitimate in the case of torques as it is in the case of fluxes.

There are altogether three hypothetical torques, viz :—

T_1 between $\nu_1 F_1$ and $i_3 z_2$
T_2 „ F_2 „ $i_3 z_2$
T_3 „ F_3 „ $i_2 z_2$.

* As already stated, in order not to burden the demonstration with unessential details, I have put $\theta_1 = 0$, so that, in Mr. Fynn's Fig. 3, O A and O B would become coincident.

In each case, the torque is proportional to the product of the flux, ampere-turns, and cosine of angle of time phase-difference between flux and ampere-turns. We thus have, referring to Fig. 3— Dr. Hay.

$$T_1 \equiv (\nu_1 F_1)(i_3 z_2) \,.\, \cos KOE$$
$$T_2 \equiv F_2\ (i_3 z_2) \,.\, \cos DOK$$
$$T_3 \equiv F_3\ (i_2 z_2) \,.\, \cos DOK.$$

If we assume the magnetic reluctance along the *a a* axis to be the same as that along the *b b* axis (Fig. 1), then F_2 will be the same multiple of $i_2 z_2$ as F_3 is of $i_3 z_2$. From this it is evident that T_2 is, on this assumption, numerically equal to T_3. But the consideration of a diagram, such as Mr. Fynn's Figs. 4–6, show at once that T_2 and T_3 are of opposite signs. Hence they cancel each other, so that the actually existing torque is numerically equal to the hypothetical torque T_1.

If this conclusion be correct, it follows that the hypothetical torque T_1 must be identical with Mr. Fynn's torque D_3. It is a very simple matter to show that such is actually the case. To begin with, in deducing the expressions for the torque, we have neglected iron losses. If these be taken into account, cos KOE becomes cos LOE (Fig. 3), and, to all intents and purposes, cos LOE is identical with Mr. Fynn's cos δ_{32}. We may therefore write—

$$T_1 \equiv (\nu_1 F_1)(i_3 z_2) \cos \delta_{32}\,;$$

or, since $\nu_1 F_1$ is very nearly equal to F_2—

$$T_1 \equiv F_2 (i_3 z_2) \cos \delta_{32}.$$

If as before we assume the same magnetic reluctance along each of the two directions *a a* and *b b* (which is the necessary condition that T_2 be numerically equal to T_3), we may put $(i_2 z_2) F_3$ for $F_2 (i_3 z_2)$, in which case the expression for T_1 takes the form—

$$\begin{aligned} T_1 &\equiv F_3 (i_2 z_2) \cos \delta_{32} \\ &\equiv N_3 (i_2 z_2) \cos \delta_{32} \\ &\equiv D_3, \end{aligned}$$

and the torque T_1 is thus seen to be equal to Mr. Fynn's D_3. Incidentally it may be noticed that the three torques T_1, T_2, T_3, are all numerically equal if, as assumed, the magnetic reluctance along *a a* be the same as that along *b b*.

From the above it will also be evident that the torque T_1 is totally different from Mr. Fynn's D_1, being a very large multiple of this latter, and equal to D_3.

It thus appears that the theory referred to in such disparaging terms by Mr. Fynn leads to conclusions identical with his own. It is to be regretted that Mr. Fynn did not take the trouble to make a thorough study of the hypothetical flux theory before attempting to pass on it criticisms which can only be characterised as extremely hasty. From the explanations given above, it will be seen that Mr. Fynn hopelessly confuses such widely different quantities as the

Dr. Hay. ν_1 F_1 of the hypothetical flux theory with his own v_1 N_1°, and the torque T_1 with his own D_1.

Mr. Fynn. Mr. FYNN (*in reply*): I quite agree with Mr. Orsettich that polyphase motors are preferable to single-phase machines wherever the former can be used. The single-phase machine is only superior to the polyphase one if speed regulation or a series characteristic is called for. The speed of a single phaser can be regulated as efficiently as that of a continuous-current shunt motor, and no polyphase machine of the induction type is known which possesses a series characteristic.

In reply to Dr. Morris, I would say that what I intended to convey when introducing my paper was, that whilst I proposed avoiding all imaginary physical or mathematical conceptions, yet I was not going to avoid dealing with the real components of the resultant flux. My experience in this matter is, that—except perhaps when trying to predetermine iron losses—it is hopeless from almost all points of view, to deal directly with the resultant flux in such motors; we do not even do it in the very simple case of the continuous-current machine. If we consider the true components of the resultant flux together with the true components of the resultant rotor and stator currents, we get, I think, a very much clearer idea of which conditions should be avoided and which others should be aimed at. I hope to emphasise this point even more strongly in another paper I am about to present to the Institution. In some particularly simple cases, such as that of the 2- or 3-phase machine, there is no very marked advantage in dealing with the true components of the resultant flux, although an advantage does exist even in this case; the matter, however, assumes a very different aspect in the case of single-phase machines. My theory is based throughout on the consideration of the true component fluxes, the true component currents, and on the simple fact that a given current carrying conductor immersed in a magnetic field will produce a certain torque under certain known conditions. The true components being known, it is a simple matter to arrive at the resultant flux whenever desired. It would be most interesting to see what could be done in the way of applying to the treatment of induction motors the method employed by Messrs. Morris & Lister when dealing with their well-known brake. Perhaps Dr. Morris will some day make the attempt; off-hand, I rather doubt whether this method will allow of the torque conditions being sufficiently clearly recognised from the designers' point of view.

Dr. Kapp is quite right in calling attention to the fact that the best motor from the point of view of weight efficiency is the 2-phase machine with which I dealt last, and that all single-phase motors are far behind it in this respect. Although, and perhaps for the very reason that, I am spending a good deal of my time in studying the single-phase proposition, I do not wish to convey the impression that I believe single-phase motors to be supreme. I am well aware of their defects, but at the same time I also know of their inherent qualities,

and am convinced that they have a very large field of utility of their own. In the case of traction, for instance, the single-phase motor presents, for the moment, no direct advantages, but the indirect advantages connected with its use for that purpose are very great. They are shortly, the possibility of using as high a voltage as required, static transformer sub-stations, one overhead conductor, low voltage at the motor terminals, and a series characteristic. Mr. Fynn.

Owing to the pulsating nature of the torque in most single-phase motors, these machines, if quite rigidly geared to the axis, would cause the wheels to skid sooner than a corresponding continuous-current motor. Below 15 ∾ this skidding would be very serious. However, a very slight elasticity in the gearing reduces this evil enormously, and it may be said that certain single-phase 25 ∾ motors are equal to continuous-current machines in this particular respect. Polyphase motors do not suffer from this disability, and amongst the single-phase motors dealt with in the paper the least likely to cause skidding are those shown in Fig. 21 (Winter—Eichberg) and in Fig. 23. Both exert two torques at starting (D_1 and D_3), and the maxima of these do not quite coincide in time. That particular point has received much attention from me, and I hope to show, in a publication which will soon appear, that these conditions can be greatly improved upon. The best treatise on the subject of skidding is to be found in an article by Bergman.*

In reply to Mr. Taylor, the single-phase traction motors as now used have a characteristic approaching, and in some cases nearly equalling, that of a continuous-current series motor. The characteristic of the motor shown in Fig. 20, for instance, approaches that of a continuous-current machine very closely indeed, whereas that of the motors shown in Figs. 13, 21, and 23 differs rather from the latter for the reasons given on page 195 second paragraph, page 201, first paragraph, and elsewhere in the paper.

As to which of the motors dealt with is the best, it all depends on the purpose for which it is required. Thus amongst the single-phase machines the neutralised series conduction motor (Fig. 20) is no doubt the most efficient, whereas the fully compensated series induction motor (Fig. 21 with a series transformer) decidedly exerts the greatest torque per ampere at starting.

Dealing with Dr. Hay's communicated remarks, I am sorry to find that, in his opinion, I am lacking in generosity towards workers in the same field, and denounce their theories after a hasty and superficial study of their contributions. I must assert, for the second time to Dr. Hay's knowledge, that I have made an honest attempt to understand the theory in question when the latter was published. I was unable to subscribe to it at the time, and I am unable to do so now. I hope I have sufficient sporting instinct in me to give everybody their due at all times, and to take particular care that my adversaries shall receive their full share of recognition at my hands. On the whole,

* *Electrical World*, vol. 48, p. 713, 1906.

Mr. Fynn. I have every reason to be particularly grateful to those whose views I cannot share; for I believe that I have learnt more through them than from others. At the same time, if I happen to differ from anybody, then I claim the right to say so—particularly as it is for the good of the community that all erroneous views be speedily eliminated.

Turning to the facts of the case, I wish to state that the theory to which I refer on page 187 of my paper appeared on April 11, 1907, in the *Elektrotechnische Zeitschrift*, and subsequently on July 12, 1907, in *The Electrician* (London). I ventured to criticise that theory at the time,* for I was not at all sure that I had understood it correctly, its teachings being somewhat surprising. I felt that others might be in the same position, and hoped that a discussion would clear the matter up.

Dr. Hay is right in saying that the task of demolishing the theory in question as *I* had understood it, required very little effort. I may say now that the task of demolishing this same theory as understood, and as set forth by Dr. Hay, is equally simple.

Dr. Hay admits that the theory of the single-phase self-excited partly compensated shunt induction motor (Fig. 1 of my paper) as put forward by me is correct, and seeks to prove that the theory which he champions so warmly, and which may be his own for all I know, is also correct because it leads to the same results as mine. The proof advanced by Dr. Hay does not bear investigation.

He states that his T_1 equals my D_3 for the following reasons:—

Since—

$$T_1 \equiv (\nu_1 F_1)(i_3 z_2) \cos \delta_{32}$$

and—

$$\nu_1 F_1 = F_2 \text{ (very nearly)},$$

it may be said that—

$$T_1 \equiv F_2 (i_3 z_2) \cos \delta_{32},$$

and since—

$$(i_2 z_2) F_3 = (i_3 z_2) F_2,$$

we may write—

$$T_1 \equiv F_3 (i_2 z_2) \cos \delta_{32} \equiv N_3 (i_2 z_2) \cos \delta_{32} \equiv D_3.$$

Now, in his remarks Dr. Hay put it down that—

$$\nu_2 F_2 = \overline{\nu_1 F_1 - v_1 N_1^{\circ}}.$$

If, then, $\nu_2 F_2$ or F_2 (for ν_2 is admittedly very small) is to equal $\nu_1 F_1$, as is necessary for the above proof, it follows that $v_1 N_1^{\circ}$ must be practically zero. Seeing that in reality $v_1 N_1^{\circ} = N_3 = F_3$, it is difficult to understand how Dr. Hay can suggest that $v_1 N_1^{\circ}$ is to be considered as being practically *nil*. Such a suggestion is certainly most convenient for the purpose Dr. Hay has in view, but it is quite certainly erroneous. It must be perfectly obvious, and can be easily demonstrated in practice, should anybody require a confirmation, that as soon as $v_1 N_1^{\circ} = 0$, or nearly zero, the motor must stop; for no energy can under such conditions be transferred from the stator to the rotor,

* *Electrician*, vol. 59, pp. 604 and 644.

The second condition necessary for the success of Dr. Hay's proof is that— Mr. Fynn.

$$(i_2 z_2) F_3 = (i_3 z_2) F_2 ;$$

this can only be the case if F_2 varies proportionately with $i_2 z_2$, and F_3 proportionately with $i_3 z_2$. A glance at Fig. 3 will show that i_2 (or O D) is, at full load, perhaps four times as great as i_3 (or O K). Now in a commercial motor the maximum density of N_3 (or F_3, as Dr. Hay prefers to designate the motor field) may easily reach 18,000 or 20,000 lines per cm.^{2} in some parts of the magnetic circuit, which means that even F_3 will not vary proportionately with $i_3 z_2$—what chance, then, has F_2 of varying proportionately with $i_2 z_2$, which, as we have seen, is some four times greater than $i_3 z_2$? If Dr. Hay can overlook such glaring discrepancies with equanimity, I feel sure that the fact of his cos L O E not really being equal to my cos δ_{32}, will trouble him but little; still I venture to mention the point.

Dr. Hay's formula for the total torque of the motor shown in Fig. 1 is—

$$T_1 \equiv \nu_1 F_1 (i_3 z_2) \cos KOE.$$

Supposing for one moment that it is correct, what impression does it convey, and of what use is it to the designer? It surely indicates that $\nu_1 F_1$ is the motor field and i_3 the armature current; it suggests that the motor field varies with the load and has a certain value even when the rotor is on open circuit, further that the armature current is practically constant within the working range of the machine. All these suggestions are contrary to the facts. The designer can make no use whatsoever of this formula, for he must fail to ascertain the value of $\nu_1 F_1$ with any degree of certainty, but even if he succeeds in this particular his result will still be far removed from the truth.

Having carefully considered Dr. Hay's able exposition of the theory which I apparently so sadly misrepresented, and which has perhaps been most generally accepted, I am forced to the conclusion that this theory of his is just as hopelessly wrong whether considered from Dr. Hay's point of view or from that which I took to be the correct one.

Dr. Hay's remarks have, however, made me realise now for the first time that his theory was based on what he calls the "hypothetical flux method." Although he suggests that I did not make a thorough study of this method, yet I can assure him that I am sufficiently familiar with the latter to assert that if the results obtained with its help are not correct it is in no way due to the "hypothetical flux theory" itself, but to the manner in which it has been applied. After this I ought perhaps to show how this general theory may be correctly applied to the problem presented by the motor shown in my Fig. 1.

Taking hypothetical fluxes into account, we may say that the total stator flux due to $(i_1 z_1)$ is made up as follows:—

$$F_1 = \overline{F_1' + f_1' + F_1'' + f_1''},$$

Mr. Fynn. where—

$\overline{F_1' + f_1'}$ is due to $i_0 z_1$, and F_1' alone threads both stator and rotor;

where—

$\overline{F_1'' + f_1''}$ is due to $-i_2 z_1$, and F_1'' alone threads both stator and rotor.

It is then seen that—

$$F_1' = \text{my } v_1 N_1^\circ,$$

that—

$$\overline{f_1' + f_1''} = \text{my } N_1',$$

and that F_1'' is a hypothetical flux which I have not taken into account, and which threads both stator and rotor. Further that—

$$\overline{F_1'' + v_1 N_1^\circ} = \text{Dr. Hay's } \nu_1 F_1.$$

The total rotor flux along the axis $a\,a$ is due to $(i_2 z_2)$, and is made up as follows:—

$$F_2 = \overline{F_2' + f_2'}$$

where—

$$f_2' = \text{my } N_2',$$

and—

$$F_2' = \text{Dr. Hay's } \nu_2 F_2,$$

which is a hypothetical flux, and has not been taken into account by me.

In addition there exists along the axis $b\,b$ of the machine a flux F_3 equal to my N_3.

For the sake of simplicity we may assume that the primary and secondary windings are identical in every respect, so that number of turns and leakage conditions are alike. Let also the reluctance be the same along every axis of the machine and the resistance of the windings be small. Then—

$$v_1 N^\circ_1 = F_3 = N_3 \text{ (very nearly)},$$
$$f_2' = N_2' = f_1',$$
$$F_2' + f_2' = \overline{F_1'' + f_1''},$$
$$\nu_2 F_2 = F_2' = \overline{\nu_1 F_1 - v_1 N_1^\circ} = F_1''.$$

Turning to the torque conditions, we see that as soon as the hypothetical fluxes are taken into account then *five* torques must be considered. These are—

(a) Along the axis $a\,a$—

T_1'	between	F_1' or $v_1 N_1^\circ$ and $(i_3 z_2)$		= to my D_1,
T_2'	„	F_1'' or $\overline{\nu_1 F_1 - v_1 N_1^\circ}$ and $(i_3 z_2)$,		
T_3'	„	F_2' or $\nu_2 F_2$ and $(i_3 z_2)$,		
T_4'	„	f_2' or N_2' and $(i_3 z_2)$		= to my D_2,

(*b*) Along the axis *b b*— Mr. Fynn.

T_5' between F_3 or N_3 and $(i_2 z_2)$ = to my D_3.

Now it is clear that $F_1'' = F_2'$, no matter whether these fluxes saturate the iron or not; and as they are due respectively to the primary load ampere-turns and the secondary ampere-turns of a transformer they must be of opposite sign. It follows that $T_2' = -T_3'$, and there remain T_1', T_4', and T_5'. Taking the directions of these torques into consideration, we find the resultant torque to be—

$$T_a' = T_5' - T_4' \pm T_1',$$

substituting the nomenclature which I have adopted in my paper we arrive at the expression—

$$D_a = D_3 - D_2 \pm D_1,$$

which is identical with the one given in the paper. It appears from the foregoing that Dr. Hay would have reached a correct result with the help of the "hypothetical flux theory" had he but applied it correctly.

Towards the end of his remarks Dr. Hay states that his torques, T_1, T_2, T_3, are all numerically equal; after what I have just said it will be obvious that this statement of his is also incorrect.

Reverting to the questions asked by Dr. Hay, I must point out that the symbol "$v_1 N_1$" is the one used by Mr. Osnos.* According to Dr. Hay's view it corresponds to his $\nu_1 F_1$, from which it would appear that my interpretation of its meaning was after all not so very far removed from the truth, for I said on page 187, "where $v_1 N_1$ is supposed to be that part of the field due to $i_1 z_1$ which threads the rotor."

The factor v_1 in the expression $v_1 N_1^\circ$, used by me throughout the paper, simply takes into account the fact that the transformer flux diminishes with increasing load. Its no-load value is N_1°, and its value for any other load is $v_1 . N_1^\circ$. O H or N_1 of Fig. 2 remains nearly constant for all loads if w_1 is small, but O G or N_1' increases with the load, from which it follows that the other component of N_1, *i.e.*, N_1° must decrease as the load increases.

In conclusion, I should like to express my thanks for the manner in which the paper has been received, and particularly to thank those who have kindly taken part in the discussion.

* *Elektrotechnische Zeitschrift*, vol. 28, p. 336.

ORIGINAL COMMUNICATION.

THE AIR-GAP CORRECTION COEFFICIENT.

By P. H. POWELL, M.Sc., M.Eng., Associate Member.

(*Communication received October* 21, 1907.)

The following is a short account of some experiments that have been recently carried out in continuation of an investigation of the air-gap correction coefficient for slotted armatures.

In the previous paper * the slots treated were open, having parallel sides, and a series of figures were given connecting the variables (width of slot and tooth, length of air-gap, etc.) with the air-gap correction coefficient; these results were compared with those given by the various formulæ that have been proposed from time to time, and indicated their limitations. The taper of the tooth was also considered, and was found to have only a very slight effect on the coefficient, so that in the remainder of the experiments the teeth and slots were made parallel.

The results obtained can thus be regarded as being correct: whether the teeth are tapering inwardly (as for a rotor) or outwardly (as for a stator).

In the present investigation the slots are semi-enclosed, the majority of them being of a rectangular shape with the corners rounded. One of the experimental difficulties encountered in connection with the hydrodynamical method employed was the casting of the layer of wax so that it should be free from air bubbles. It was found that if the dish containing the wax and glass plate was kept vibrating, the air bubbles were not able to form, the result being a clear plate of wax. The vibration was produced by rigging up an electric bell so that the oscillating hammer tapped the sides of the dish in which the wax and the glass plate were cooling together.

As a preliminary to these later experiments the proportions of the teeth of a large number of machines of different makes were determined, and the following table gives a summary of them. The meanings of the symbols are given by the diagram Fig. 1.

A number of sets of photographs were taken, one dimension being varied at a time, while the remainder were kept substantially constant. Slight discrepancies creeping in were eliminated by plotting a series of curves. Table I. gives the results obtained in twenty-eight

* *Journal of Institution of Electrical Engineers*, vol. 34, p. 21, etc., 1905.

Ratio.	Limits.
s/d	0·2 — 0·7
s/t	0·4 — 2·7
s/g	3·0 — 28·0
s/a	5·0 — 32·0
s/b	2·5 — 8·0

different cases, while Tables II.–VI. and Curves I.–V. contain an analysis of the results.

Naturally there are an infinite number of combinations of the variables, but these curves should be capable of giving fairly accurate values for almost any combination.

As will be seen, the chief factors determining the coefficient are the length of air-gap and the width of slot opening. The rounding of the corners is unimportant, as also is the width of iron in the lip (a) of the slot, provided this width is above a certain limit. The ratio of the width of slot to width of tooth is of some importance.

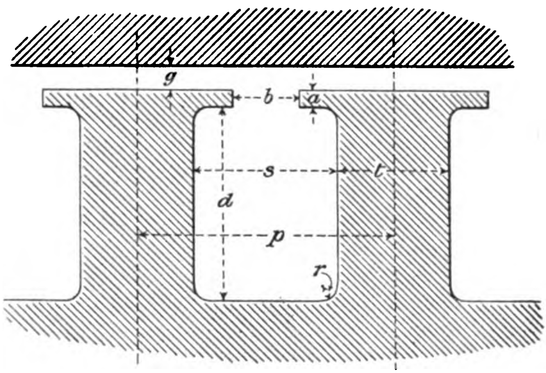


FIG. 1.—Diagram of Teeth and Slots.

p = pitch of teeth.
s = width of slot.
t = width of tooth.
b = opening of slot.
a = thickness of lip.
g = air-gap length.
d = depth of slot.
r = radius of corners of slot.

In all these diagrams one member alone is slotted, that is, they represent the case of an alternator with slotted armature and simple poles; the case of the induction motor, that is, having both members slotted, can be similarly treated, but there arises a difficulty from the fact that the pitches of the teeth on rotor and stator are not the same.

Figs 2, 3, 4, and 5 are samples of the photographs obtained. It will be noticed that in each case the maximum induction occurs at the edge of the tooth.

TABLE I.

No.	$\frac{\text{Slot}}{\text{Pitch}} = s/p$.	$\frac{\text{Tooth}}{\text{Pitch}} = t/p$.	$\frac{\text{Gap}}{\text{Pitch}} = g/p$.	$\frac{\text{Lip}}{\text{Pitch}} = a/p$.	$\frac{\text{Opening}}{\text{Pitch}} = b/p$.	$\frac{\text{Depth}}{\text{Pitch}} = d/p$.	$\frac{\text{Radius}}{\text{Pitch}} = r/p$.	Coefficient.
1	0·552	0·448	0·1520	0·0460	0·2590	0·695	0·050	1·075
2	0·552	0·448	0·1260	0·0460	0·2590	0·695	0·050	1·096
3	0·555	0·445	0·1010	0·0462	0·2600	0·700	0·050	1·121
4	0·555	0·445	0·0520	0·0462	0·2600	0·700	0·050	1·170
5	0·550	0·450	0·0290	0·0462	0·2600	0·700	0·050	1·215
6	0·800	0·200	0·0765	0·0500	0·2530	0·706	0·050	1·230
7	0·706	0·294	0·0756	0·0494	0·2560	0·695	0·050	1·210
8	0·599	0·401	0·0756	0·0494	0·2560	0·695	0·050	1·190
9	0·497	0·503	0·0760	0·0496	0·2570	0·701	0·050	1·170
10	0·399	0·601	0·0752	0·0492	0·2540	0·701	0·050	1·155
11	0·298	0·702	0·0760	0·0497	0·2570	0·706	0·050	1·140
12	0·247	0·753	0·0764	—	0·2590	—	0·050	1·135
13	0·552	0·448	0·0771	—	0·5500	—	0·050	1·360
14	0·544	0·456	0·0780	0·0492	0·4050	0·695	0·050	1·290
15	0·552	0·448	0·0794	0·0500	0·2530	0·705	0·050	1·175
16	0·551	0·449	0·0798	0·0502	0·1055	0·710	0·050	1·099
17	0·553	0·447	0·0765	0·0501	0	0·706	0·050	1·062
18	0·552	0·448	0·0800	0·0465	0·2450	0·698	0	1·145
19	0·550	0·451	0·0753	0·0492	0·2540	0·707	0·075	1·138
20	0·552	0·448	0·0813	0·0465	0·2450	0·698	0·100	1·141
21	0·555	0·445	0·0780	0·0492	0·2430	0·695	0·150	1·120
22	0·552	0·448	0·0813	0·0087	0·2450	0·741	0·050	1·200
23	0·552	0·448	0·0813	0·0233	0·2500	0·721	0·050	1·160
24	0·550	0·450	0·0810	0·0752	0·2500	0·672	0·050	1·134
25	0·550	0·450	0·0810	0·0954	0·2500	0·648	0·050	1·135
26	0·552	0·448	0·0291	0·0494	0·2560	0·436	0·050	1·320
27	0·552	0·448	0·0291	0·0930	0·2500	0·653	0·050	1·242
28	0·553	0·447	0·0265	0·0472	0	0·706	0·050	1·090

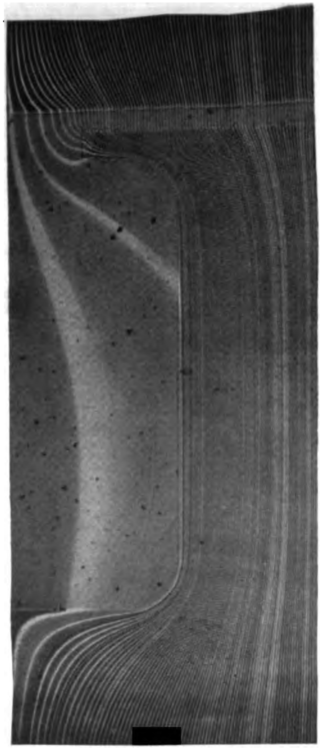


FIG. 2.

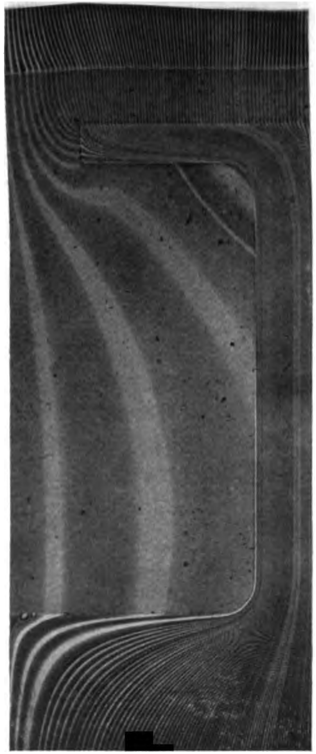


FIG. 3.

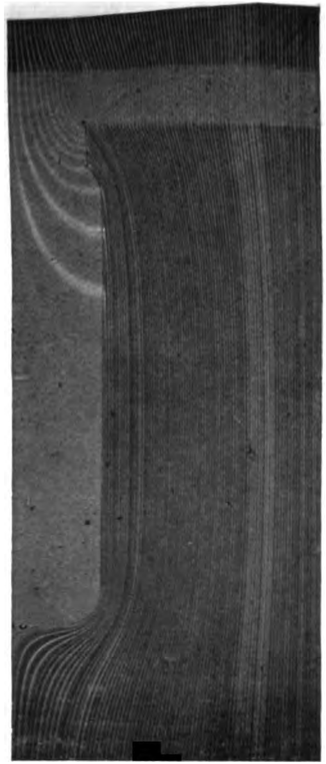


FIG. 4.

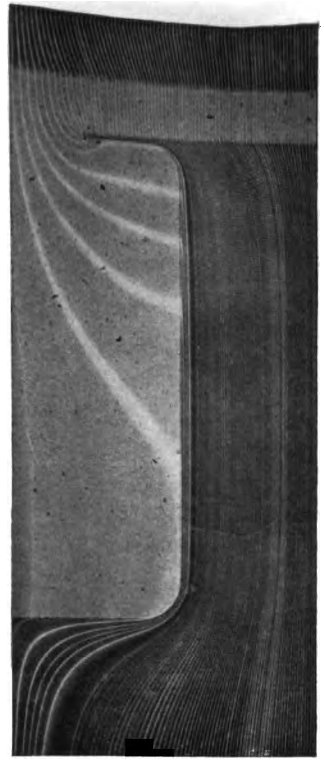
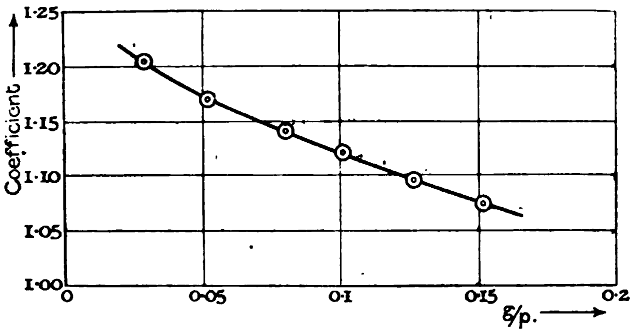


FIG. 5

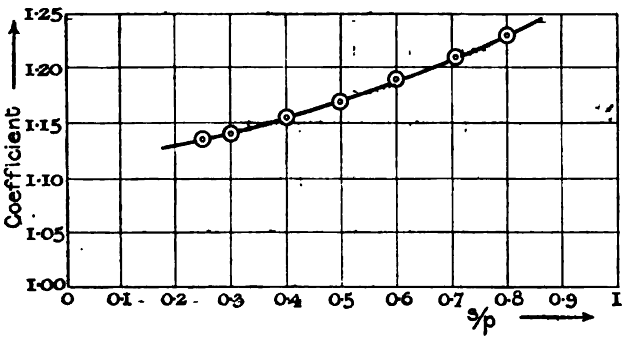
FIGS. 2–5.—Representative Samples of Stream-line Photographs.

In the photographs shown, and in many others not here reproduced, the depth of slots is relatively small, as it was thought that any increase in the depth of the slot below a certain point would not influence the distribution of the flux appreciably; this allowed the remaining portions to be dealt with on a larger scale.

It will be noticed throughout the photographs that only a very small proportion of the lines manage to keep in the slot; this is very marked in comparison with some of the diagrams in the previous paper for open slots.



CURVE I.



CURVE II.

As might be expected, the disturbing effect of the presence of the teeth on the distribution along the pole-face and in the pole shoe becomes more and more marked as the gap decreases. This will be seen by reference to Curve I. which connects values of the coefficient with values of the ratio g/p.

Curve II. connects the coefficient with the ratio s/p: it is nearly a straight line within the limits shown.

Curve III. connects the coefficient with the length of lip of tooth, and the two last points seem to lie somewhat off the line. If these

two points are correct the curve would show a point of inflexion at a value $b/p = 0{\cdot}35$.

Curve IV. shows the effect of the rounding of the corners on the coefficient.

Curve V. shows the result of varying the thickness of the lip, and from this it will be seen that as long as the ratio a/p is above 0·03 the coefficient remains sensibly constant; below this value, however, the curve ascends steeply.

The above experiments have occupied the best part of two years chiefly in vacation time, and were conducted at the School of Engineering, Canterbury College, Christchurch, N.Z.

In conclusion, the author desires to express his gratitude to Dr. Hay for his kindness in revising this paper.

TABLE II.

$s/p = 0{\cdot}555$; $t/p = 0{\cdot}445$; $a/p = 0{\cdot}046$; $b/p = 0{\cdot}26$; $d/p = 0{\cdot}7$; $r/p = 0{\cdot}05$.

No.	1.	2.	3.	4.	5.
g/p	0·152	0·1265	0·101	0·052	0·029
Coefficient ...	1·075	1·0960	1·121	1·170	1·215

Equation of nearest straight line—

$$\text{Coefficient} = 1{\cdot}22 - 0{\cdot}96\, g/p.$$

The above results are plotted in Curve I.

TABLE III.

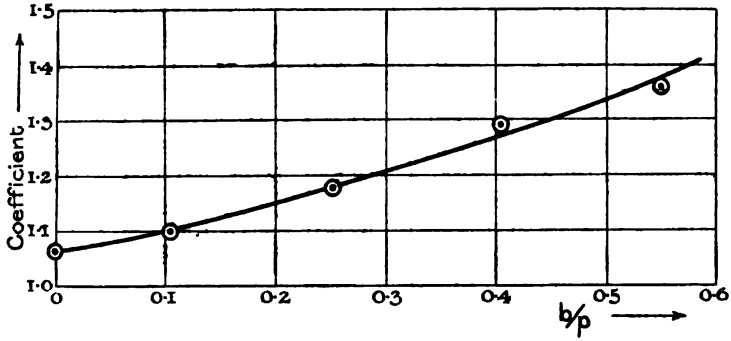
$g/p = 0{\cdot}076$; $a/p = 0{\cdot}0494$; $b/p = 0{\cdot}256$; $d/p = 0{\cdot}7$; $r/p = 0{\cdot}05$.

No.	6.	7.	8.	9.	10.	11.	12.
s/p	0·80	0·706	0·599	0·497	0·399	0·298	0·247
t/p	0·20	0·294	0·401	0·503	0·601	0·702	0·753
Coefficient	1·23	1·210	1·190	1·170	1·155	1·14	1·135

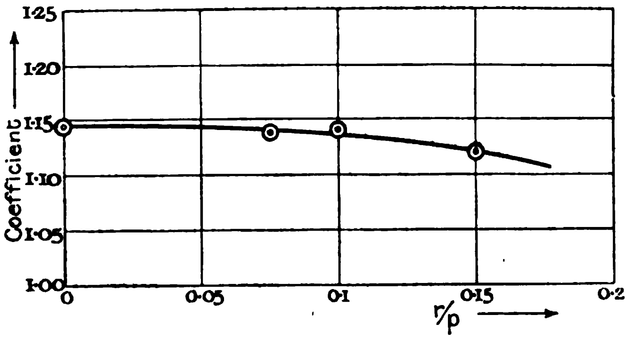
Equation of nearest straight line—

$$\text{Coefficient} = 0{\cdot}18\, s/p + 1{\cdot}085.$$

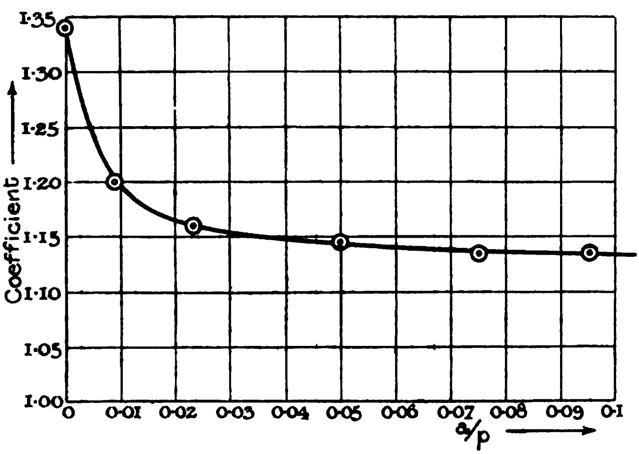
These values are plotted in Curve II.



CURVE III.



CURVE IV.



CURVE V

TABLE IV.

$s/p = 0{\cdot}552$; $t/p = 0{\cdot}448$; $g/p = 0{\cdot}078$; $a/p = 0{\cdot}05$; $d/p = 0{\cdot}7$.

No.	13.	14.	15.	16.	17.
b/p	0·55	0·405	0·253	0·1055	0
Coefficient ...	36	1·290	1·175	1·0990	1·062

Equation of nearest straight line—

$$\text{Coefficient} = 0{\cdot}58\, b/p + 1{\cdot}04.$$

The results are graphically represented in Curve III.

TABLE V. (Curve IV.)

$s/p = 0{\cdot}552$; $t/p = 0{\cdot}448$; $g/p = 0{\cdot}08$; $a/p = 0{\cdot}048$; $b/p = 0{\cdot}245$; $d/p = 0{\cdot}7$.

No.	18.	19.	20.	21.
r/p	0	0·075	0·100	0·15
Coefficient	1·145	1·138	1·141	1·12

TABLE VI. (Curve V.)

$s/p = 0{\cdot}552$; $t/p = 0{\cdot}448$; $g/p = 0{\cdot}081$; $b/p = 0{\cdot}25$; $d/p = 0{\cdot}7$; $r/p = 0{\cdot}05$.

No.	22.	23.	24.	25.
a/p	0·00873	0·0233	0·0752	0·0954
Coefficient	1·20000	1·1600	1·1340	1·1350

Kelvin

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. 40. 1908. No. 188.

WILLIAM THOMSON, BARON KELVIN OF LARGS, IN THE COUNTY OF AYR.

PRESIDENT OF THE INSTITUTION OF ELECTRICAL ENGINEERS IN 1874, 1889, AND 1907. ELECTED MEMBER IN 1871 AND HONORARY MEMBER IN 1899.

Died December 17, 1907.

William Thomson, Baron Kelvin of Largs, was born at Belfast, Ireland, on June 26, 1824. He was the second son of James Thomson, afterwards Professor of Mathematics in the University of Glasgow. In 1832 he accompanied his father to Glasgow, and matriculated in the University there in 1834. He entered St. Peter's College, Cambridge, in 1841, and graduated as Second Wrangler and First Smith's Prizeman in 1845. In the same year he was elected a Fellow of his College, and afterwards proceeded to Paris, where he spent a short time in Regnault's Laboratory. In 1846 he was elected Professor of Natural Philosophy in the University of Glasgow. The Royal Society made him a Fellow of their body in 1851. His invention of the mirror galvanometer and other instruments was made in 1858. He became Director of the Atlantic Telegraph Company about the same time, and took an active part in putting into operation the cable of 1858. The honour of knighthood was conferred upon him in 1866, in recognition of his services in connection with the laying of the new Atlantic cable in that year. In 1867 his siphon recorder was invented. He was

President of the British Association at Edinburgh in 1871, and was elected President of the Society of Telegraph Engineers, now the Institution of Electrical Engineers, in 1874. In 1876 he was President of the Physical Section of the British Association at Glasgow. In the years 1880–1 and 1881–2, he was President of the Physical Society of London. He was awarded the Copley Medal of the Royal Society in 1883, and his Baltimore Lectures on Molecular Dynamics and the Wave Theory of Light were delivered at the Johns Hopkins University in 1884. In 1889 he became President of the Institution of Electrical Engineers for the second time. In 1890 the Royal Society elected him as their President, which office he held until 1895.

Lord Kelvin was created a member of the House of Peers in 1892, taking his title from the stream flowing past the University where so large a portion of his life was spent. On the celebration of his jubilee as Professor at Glasgow in 1896, a unique gathering assembled there to do him honour, and he received congratulations from scientific men in all quarters of the globe. Three years later, in 1899, he retired from his Professorship in that University. In 1902 he was created a member of the Order of Merit, on its institution, and in the same year became a Privy Councillor. He was unanimously elected Chancellor of the University of Glasgow in 1904, in succession to the Earl of Stair. In 1907 he was elected for the third time President of the Institution of Electrical Engineers.

His death took place on December 17, 1907, and he was interred with national honours in Westminster Abbey on December 23.

Lord Kelvin was twice married: first, in 1852, to Margaret, daughter of Walter Crum, of Thornliebank (died 1870); secondly, in 1874, to Frances Anna, daughter of Charles R. Blandy, of Madeira, who survives him.

He was an original Member of the Institution of Electrical Engineers, and in 1899 was elected an Honorary Member. Between the years 1872 and 1889 he contributed ten papers and memoirs to the *Proceedings* of the Institution, covering a wide range of subjects connected with electrical science.

The Institution is indebted to Dr. Silvanus P. Thompson, F.R.S., Past-President, for these brief notes of the career of the late Lord Kelvin. The first Kelvin Lecture, which, at the invitation of the Council, Dr. Thompson has kindly consented to deliver on April 30, 1908, will deal fully with the life and work of Lord Kelvin, and will be published in the *Journal* in due course.

Proceedings of the Four Hundred and Sixty-Third Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, November 28, 1907—Mr. CHARLES P. SPARKS, Vice-President, in the chair.

The minutes of the Ordinary General Meeting held on November 14, 1907, were taken as read and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council:—

TRANSFERS.

From the class of Associate Members to that of Members:—

Reginald K. Morcom. | Albert P. Pyne.
Norman J. Wilson.

From the class of Associates to that of Associate Members:—

Louis T. Healy. | David P. Reid.
Hubert C. Sparks.

From the class of Students to that of Associate Members:—

Arthur George Whitfeild.

Messrs. A. Russell and B. B. Heaviside were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected:—

As Associate Members.

Hubert Dobell.
Arthur Hamilton Ellis.
John Lloyd Forsyth.
Willie Waite Skirrow Ibbetson.
James Alfred William Kerr.
Robert Taylor McArthur.
William Henry Mahon.
George Alexander Mennie.
Albert Richardson.
Reginald Arthur Stoker.

As Associates.

Walter Lyulph Johnson.	Charles Bluthner Lessner.

Frederick Richard Simms.

As Students.

William Reginald Abram.	Alfred Llewelyn Paramor.
Thomas George Symonds Babb.	Christopher Redman.
Dorabji Rustomji Cooper.	Manmathadhan Roy.
Paul Robert Fortin.	Cyril Marsh Simpson.
Gerard Hartley.	Arnold Southall.
George Frederick Hilton.	Frank Charles Topham.
John William Gaitskell Meaby.	Guilherme Dumont Villares.

Edgar Longshaw Walton.

Donations to the *Library* were announced as having been received since the last meeting from F. J. Sprague and L. H. Walter, to whom the thanks of the meeting were duly accorded.

The following paper was read and discussed :—

THE DEVELOPMENT OF TURBO-GENERATORS.

By Dr. ROBERT POHL, Associate Member.

(*Paper received from the* LEEDS LOCAL SECTION, *September* 17, 1907, *read in London, November* 28, 1907, *and at Leeds, January* 23, 1908.)

Through the great improvements made in late years in the construction of steam turbines the electrical industry is confronted with the problem of designing dynamos suitable to be directly coupled to these high speed prime-movers. The inter-dependence of steam and electrical engineering, and the consequent great influence which the solution of this problem would exercise on the future of both industries, explains the untiring energy with which most of the leading firms in all countries have been working at it for the last four or five years, following the lead of Messrs. Parsons and Co. of Newcastle, and Messrs. Brown Boveri and Co. of Baden. The difficulties of the problem are partly of a mechanical, partly of an electrical nature, and whilst the same may now be said to be satisfactorily overcome in the case of alternators, there is admittedly much room for improvement in the construction of direct-current machinery.

Several most valuable and exhaustive papers have been recently read before this and other societies dealing with turbo-machines both for alternating current and direct current. In the present paper it is not intended to consider the whole of this vast subject, but it is confined to D.C. generators. The paper will deal with these only, or at least mainly, from the point of view of their electromagnetic design, in which, as is well known, the greatest obstacles to further improvement are encountered. The object is to show why certain outputs and speeds cannot at the present time be safely exceeded, to define these output limits, and, in conclusion, to discuss briefly the directions in which further improvements may be expected. In this connection one or two suggestions will be made which may possibly prove to be of interest.

In view of the desirability—from the turbine maker's point of view —of running the set with as high a speed as possible, we have first of all to answer the questions: Given a certain diameter of the armature, what will be the maximum permissible speed at which it may be run, without undue stress being set up? and secondly, what is the maximum possible output thereby obtainable, assuming the armature to be made axially as long as possible? In finding the reply to these questions, we

shall arrive at a curve showing the highest possible speed for which a generator can be built as a function of its output.

In slow speed machines the output, as is well known, is limited either by the sparking or by the heating. For the purpose of our present investigation, as may be particularly noted, we shall leave the question of temperature rise altogether out of consideration, because, though it requires the most careful attention in the design and construction, it cannot be considered as an output limit. The extremely high periodicity and consequent large iron losses with which most turbo-generators have to work, coupled with their small dimensions, necessitate in most cases the employment of artificial cooling devices. A number of systems for artificial ventilation, which allow of the regulation of the quantity of air forced through, are in use, so that the heating limit can, for our purposes, be considered as lying above the output limit as fixed by other considerations.

The mechanical or speed limit of an armature is defined by the circumferential velocity, for which the tensile stresses reach their permissible limit. In working through modern turbo-generator designs it will be found that it is invariably the stress in the end shells protecting the connections which first approaches the limiting values. If we calculate the stress K occurring in an ordinary unloaded ring, of radius r cm., thickness ∂ cm., and specific weight γ, rotating with velocity v metres per second, we obtain—

$$K = \frac{1}{\partial} \int_{\phi=0}^{\phi=90} \frac{r \cdot d\phi \cdot \partial \gamma}{98 \cdot 1} \times \frac{v^2}{r} \times \sin\phi = \frac{v^2 \cdot \gamma}{98 \cdot 1} \text{ kg./sq. cm.,}$$

therefore the highest permissible velocity—

$$v_{\text{perm.}} = \sqrt{\frac{K_{\text{perm.}} \times 98 \cdot 1}{\gamma}} = \sim 10 \sqrt{\frac{K_{\text{perm.}}}{\gamma}} \text{ m./sec.}$$

Allowing a permissible stress for ordinary bronze casting of 260 kilogrammes per square centimetre (tenacity about 1,500), we obtain as maximum velocity for an unloaded ring 55 metres per second. Considering the additional load due to the end connections, the maximum speed will only be about 50 m./sec. In order to allow of higher circumferential velocities, special phosphor-bronze and manganese-bronze castings are generally employed for the end shells. As the permissible stress for these materials is about 600 kilogrammes per square centimetre or even higher (tenacity about 3,500 to 4,500), the maximum velocity will be found to be 75 to 85 m./sec. under the conditions of load prevailing in such machines. If, for the purpose of our consideration, we assume 75 m./sec. to be the maximum circumferential velocity, we arrive immediately at the highest possible speed in revolutions per minute for any armature of given diameter.

Let us now proceed to the purely electrical output limits.

Flash-over Limit.—While in ordinary slow speed D.C. machinery the only electrical limit is that of sparking, in high speed machines we have

not only to deal with this factor but also with a new one, which we may term the "flash-over limit," and which requires quite as much attention as the commutation. In fact, not a few turbo-generators, in spite of their good commutating qualities, have proved to be failures on account of their tendency to arc round the whole commutator. If one were to inquire from the designers as to the cause of this trouble, they would, in most cases, attribute it to insufficient insulation of the shrink-rings and of the brush-gear. This may often have been the cause; so much is certain, however, that in not a few cases the real source of the trouble must be attributed to the excessive potential difference between two adjacent commutator bars.

It is, of course, a matter of common experience that bad commutation, particularly in connection with a dusty commutator, can by itself be the cause of flashing over, and it is further well known that the kind of winding employed and the velocity of load fluctuations, which result in momentary increases of pressure, materially influence the phenomenon. Whilst fully recognising the importance of these subsidiary causes it must, in my opinion, be admitted that the maximum voltage per segment is the most important factor, which for satisfactory performance should not exceed a certain value. This value we shall have to decide upon. In doing so, however, it must be remembered that the circumferential velocity is already fixed as not more than 75 m./sec., and that now in limiting the voltage per segment, the highest number of lines of force which can be allowed to pass through the gap per centimetre armature circumference is at the same time settled. In other words, by limiting $e_{seg.\ max.}$ the product $B_{g\ max.} \times l_a$ is simultaneously limited, which practically means also the length of the armature.

Experience has shown that slow speed machines, with clean commutator and the usual thickness of mica insulation between the segments, approach their flash-over limit if the voltage per segment reaches a maximum of about 60 volts. As a safe limit in the case of turbo-generators 40 volts may be taken, the smaller figure taking into account the effect of load fluctuations, dust, etc. In testing and examining a large number of different machines the author found this figure only once or twice slightly exceeded. From $e_{seg.\ max.} = 40$ volts and $v = 75$ m./sec. the product $B_{g\ max.} \times l_a$ is found to be—

$$B_{g\ max.} \times l_a = \frac{40 \times 10^8}{2 \times 75 \times 100} = 267{,}000.^*$$

Assuming 25 per cent. field distortion—

$$\left(\frac{B_{g\ max.}}{B_{g\ mean}} = 1{\cdot}25\right)$$

then—

$$B_{g\ mean} \times l_a = 215{,}000,$$

* The armature winding is assumed throughout the paper to be an ordinary lap winding, for which the P.D. between two adjacent commutator bars is equal to the E.M.F. induced in two conductors in series.

which for $B_{g\ mean} = 5{,}000$ corresponds to $l_a = 43$ cm. as greatest permissible length of the armature, independent of its diameter. We now arrive at the total number of lines $p \times N$ (p = number of poles, N = flux per pole), entering or leaving the armature. Taking the ratio $\frac{\text{pole-arc}}{\text{pole-pitch}} = \alpha = 0{\cdot}65$,* we find $p\,N = d_a\ \pi\ \alpha\ B_{g\ mean}\ l_a = 0{\cdot}44\ d_a\ 10^6$ (second column of Table I.). It will thus be seen that in the design of the magnetic circuit of a turbo-generator the flash-over limit is of the utmost importance, as it restricts the number of lines allowed to enter or leave an armature of given diameter.

Commutation Limit.—The only point which now remains to be settled, before arriving at the highest possible output, is the question as

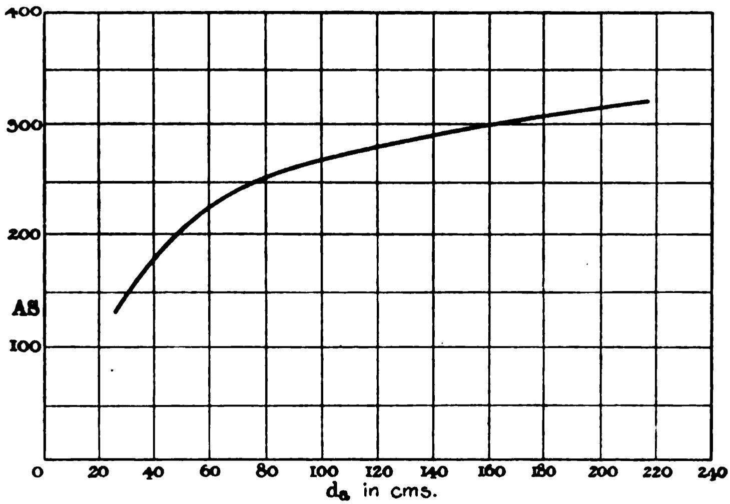


FIG. 1.

to how many ampere-turns can be accommodated on the circumference of the armature, or in other words, what is the highest permissible specific electric loading A S (ampere conductors per centimetre armature circumference).† The limiting factor in this case is the sparking. The quality of the commutation, of course, depends not only on the A S value of the armature, but is intimately connected with quite a number of other electric and magnetic conditions, particularly the effectiveness of the commutating poles. Last and not least, it depends greatly on the mechanical conditions of the armature, the commutator, and the brushes. Further, in machines of large output the pressure, or better, the absolute value of the current strength, is of the greatest importance,

* For reasons of leakage and core density α is usually much smaller than in ordinary machines.

† The term A S being now so largely adopted no apology is needed for its employment throughout the paper.

so that for 550-volt machines A S may be considerably higher than for 240 or even 120 volt machines. Although a general fixing of A S values is therefore impossible, the accompanying curve, Fig. 1, applicable to well-designed 550-volt machines working with the above-stated values of v and l_a, may be adopted as containing the limiting figures. Having thus settled upon the values of A S, $p \times N$, and the speed, the maximum possible output in kilowatts for any assumed diameter can now easily be deduced.

As the E.M.F. produced by a generator is—

$$E = p N \times \frac{s}{a} \times \frac{n}{60} \times 10^{-8},$$

where—

s = number of conductors,
a = number of circuits,

and the current strength—

$$C = AS \times d_a \pi \times \frac{a}{s},$$

the output will be—

$$K.W. = \frac{E \times C}{1,000} = p N \times AS \times d_a \pi \frac{n}{60} \times 10^{-11}.$$

The following table, based on the above calculation, shows the maximum outputs and speeds which at the present time are obtainable in the very best designs :—

TABLE I.

d_a in Centimetres.	n for v = 75 m./sec.	$p N \times 10^{-6}$.	A S.	Output in Kilowatts.
30	4,780	13·1	150	148
40	3,590	17·6	185	244
50	2,870	22·0	210	347
60	2,390	26·3	230	454
80	1,800	35·1	255	670
100	1,435	44·0	270	890
120	1,195	52·9	280	1,110
140	1,020	61·5	290	1,340
160	895	70·2	300	1,585
200	720	88·0	315	2,080

In Fig. 2 the output figures are plotted as function of the speed, and at the same time the respective curve of the Parsons turbine, as usually constructed, is indicated by a dotted line. The comparison is of interest as it shows that up to about 500 k.w. the dynamo can be built to the requirements, whilst for outputs above 500 k.w., the proper domain of the steam turbine, it is no more possible to construct D.C. generators running at so high a speed as the equivalent turbine demands.

There are two ways of overcoming the discrepancy. The one consists in artificially reducing the speed of the steam turbine, which, however, is connected with increased cost and steam consumption, the second is the tandem arrangement of two dynamos coupled to one turbine, the capacity of each being one-half of the turbine output.

The latter method obviously brings the dynamo well within the above calculated limits, and it also has certain practical advantages. It adds, however, considerably to the cost of the set, involves further expenses in foundations, bearings, etc., and increases the floor space required. Both methods can, therefore, only be considered as temporary measures. Considering further the growing demand for

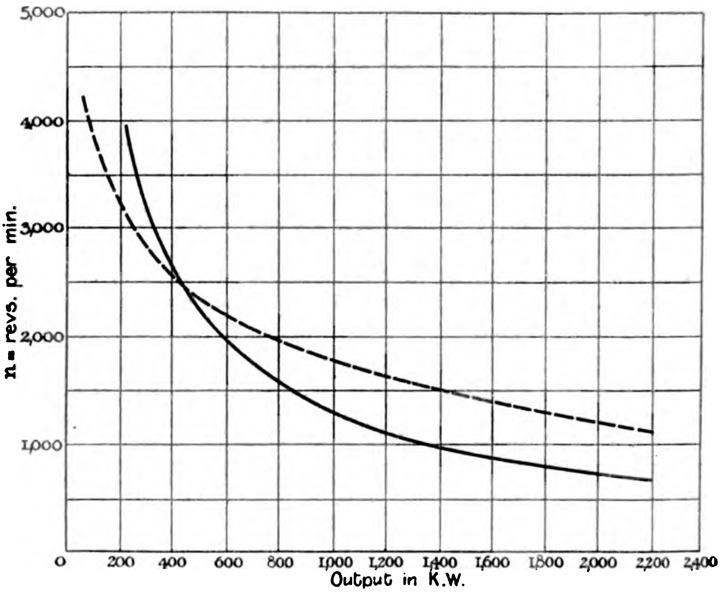


FIG. 2.

large units, the endeavour of all manufacturers of turbo-generators to extend the output limits appears to be fully justified. In fact, the problem is perhaps even more important than it might appear from the above considerations, seeing that the successful production of D.C. turbo-generators suitable for much higher speeds than were hitherto possible would further reduce the steam consumption, and considerably lower the output, for which the turbine becomes superior in performance and in first-cost to the reciprocating engine. It would also exercise great influence on the choice of a system for central stations and power transmission schemes.

Let us consider in which directions such further improvements may possibly be accomplished.

Homopolar Machines.—Before dealing with methods for the reduction of the electrical difficulties in commutating machines, it will be of interest to consider the prospects of the now well-known homopolar type of direct-current dynamo. Though historically the oldest electromagnetic generator, this type had been almost forgotten, until in 1904 the publication of the test results and of the construction of a 300-k.w. homopolar turbo-dynamo, built by the General Electric Company, U.S.A., directed the universal interest again to the same. Since that time, however, so far as I am aware, no further experimental results of homopolar machines have been published, although the patent records indicate considerable activity in the direction.

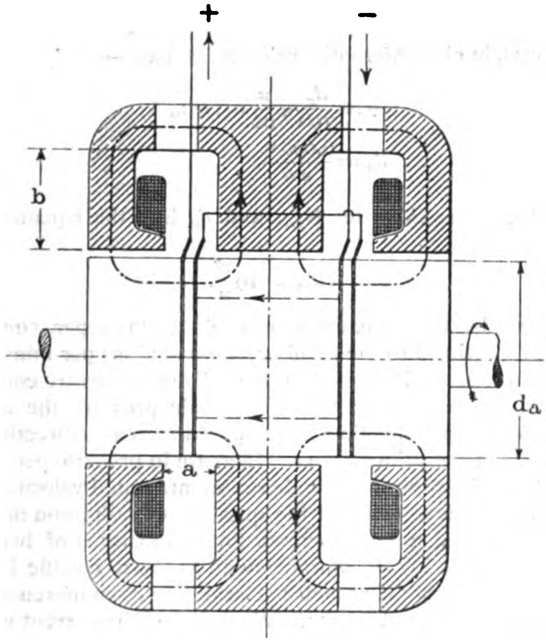


FIG. 3.

The main and somewhat threadbare objection cited against the homopolar principle is the difficulty of obtaining sufficiently high voltages. I venture to suggest, however, that this is not the most important one, but that the excessive weight of active material, which these machines require, is, from a commercial point of view, practically prohibitive. Returning, however, first of all to the production of high voltages, the belief has often been expressed that the development of high speed prime-movers would greatly facilitate the introduction of homopolar machines on the ground that turbo-speeds would remove the longstanding objection against this type. It may be pointed out that this argument must be taken with great reserve. In fact, it is

easy to show that exactly the opposite is the case, provided that in all cases the same circumferential velocity of the armature is allowed. Taking the arrangement shown in Fig. 3, which is the most suitable one for producing high voltages, and assuming the whole cross-section of the armature to be magnetically effective, the maximum possible number of useful lines will be—

$$N_{max.} = 2\, d_a^2 \frac{\pi}{4} B_{a\ max.}$$

where $B_{a\ max.}$ is about 20,000. The highest possible voltage obtainable per conductor—

$$e_{max.} = N_{max.} \times \frac{n}{60} \times 10^{-8}.$$

Now, for certain circumferential velocity we have—

$$v = \frac{d_a}{100} \pi \frac{n}{60} \text{ m./sec.},$$

$$d_a = \frac{6{,}000 \text{ v.}}{\pi n}.$$

Inserting the expressions for $N_{max.}$ and d_a into the equation for $e_{max.}$ we find—

$$e_{max.} = 19 \frac{v^2}{n}.$$

In other words, the maximum possible voltage per conductor is inversely proportional to the number of revolutions per minute, taking v as being constant. Thus, if a number of conductors are connected in series for the purpose of obtaining a certain pressure, the number of insulated conductors and slip-rings required increases directly with the speed (r.p.m.) of the prime mover. Increase in pressure per conductor is only obtainable by allowing higher circumferential velocities.

Let us now further consider the question of utilisation of material. It will be found that quite an extraordinary apparatus of brushes and slip-rings is required when working with as low a specific load as 30-ampere conductors per centimetre of armature circumference. Let us for our purpose assume $A\,S = 32$. We then find the current which may be taken out of a simple cylinder $c = d_a \times \pi \times 32 = \sim 100\, d_a$, and as for $B_a = 20{,}000$ e can be written $e = \frac{d_a v}{100}$, we obtain the output in kilowatt

output $= \frac{e \times c}{1{,}000} = \frac{d_a^2 v}{1{,}000}$ k.w. (see columns 3, 4, 5, 7, 8, and 9 of Table II.).

In order to obtain a further approximate idea as to the weight of steel required a simple formula may be propounded for the same. Taking, again, $B_a = 20{,}000$, further B gap $= 10{,}000$, B pole $=$ B yoke $= 15{,}000$ (without leakage), $a = 20$ cm. and $b = \frac{1}{3} d_a$,* the weight of steel will be found to be—

$$W = 7{\cdot}8\,(5{\cdot}8\, d_a^3 + 74\, d_a^2)\; 10^{-6} \text{ tons}.$$

* This assumption is somewhat unfavourable in the case of larger sizes.

From this a table can be immediately drawn up showing the weights of steel of homopolar machines, and the possible outputs as functions of the diameter. This is done in Table II. both for 100 and for 200 m./sec. circumferential velocity.

It appears that for $v = 100$ m./sec., for instance, a 1,000-k.w. generator could be built for 600 volts, obtained by means of twice six slip-rings connected in series. The speed of 1,900 r.p.m. would be quite convenient. The weight of steel, however, is not less than 51 tons as compared with approximately 5 tons magnetic material for a commutating machine of similar output. This comparison will suffice to show that, at present, it is out of the question to obtain commercially satisfactory results with homopolar machines. At the same time the fact must not be lost sight of, that the twelve slip-rings for 1,670 amperes each, which are not easily accessible and rotating at very high speed, will most likely require as much attention as a turbo-commutator. The extraordinary increase in steel weight as compared

TABLE II.

d_a in cm.	$v = 100$ m./sec.				$v = 200$ m./sec.				Weight of Steel in Tons.
	Revs. per Min.	Volts.	Amps.	K.w.	Revs. per Min.	Volts.	Amps.	K.w.	
20	9,550	20	2,000	40	19,100	40	2,000	80	0·6
40	4,775	40	4,000	160	9,550	80	4,000	320	3·8
60	3,180	60	6,000	360	6,360	120	6,000	720	11·6
80	2,390	80	8,000	640	4,780	160	8,000	1,280	26·8
100	1,910	100	10,000	1,000	3,820	200	10,000	2,000	51
150	1,270	150	15,000	2,250	2,540	300	15,000	4,500	166
200	955	200	20,000	4,000	1,910	400	20,000	8,000	384
300	635	300	30,000	9,000	1,270	600	30,000	18,000	1,280

with the steel required for commutating machines cannot by any means be compensated for by a slight saving in copper which may possibly be effected.

Considering the last table for 200 m./sec., the generators contained therein would perhaps be suitable for direct coupling to De Laval turbines. It would, however, hardly be possible to have insulated conductors and slip-rings rotating at so high a circumferential velocity. But for a simple homogeneous cylinder, as Table II. shows, the pressure obtained would be too small for ordinary purposes unless one goes, at the same time, to extremely large outputs. Even then the weight will be prohibitive. If it were possible to employ as high a velocity as 400 m./sec., which, as De Laval turbines prove, is quite feasible from a mechanical point of view, one would arrive at generators the speed of which is far higher than that of any known prime-mover of corresponding output.

The poor utilisation of the material in homopolar machines is

mainly due to the low A S values, which are approximately only one-tenth of those employed in large commutating machines. We necessarily arrive, therefore, at approximately ten times the weight of magnetic material. Seeing that the A S value is limited by the brush apparatus, the problem of building a commercially successful homopolar machine appears to be identical with the problem of constructing a reliable apparatus for taking current out of a cylinder rotating at very high velocity with a current density corresponding to about 200 to 300 amperes per centimetre circumference. So long as this problem remains unsolved the homopolar machine seems to be only suitable for such outputs and such low pressures for which a commutating machine is practically impossible. The solution of the problem, however, is perhaps connected with even greater difficulties than those involved in the further improvement of commutating machines.

Means for the Improvement and Increase of Output of Commutating Machines.—Such means must have for their object the further extension of the above discussed two limits, namely, the sparking and the flash-over limit. This can be accomplished, first, by working with a higher circumferential velocity of the armature. Considering two machines of equal output and speed, of which the one has the armature diameter D, the other the diameter 2 D, the length of the armature of the second will be only a quarter of that of the first, whilst for the same pressure the number of conductors and segments will be doubled. The voltage per segment, and approximately also the reactance voltage, will therefore only be one-half of the respective values of the first machine. These two voltages may be taken as being approximately inversely proportional to the circumferential velocity employed. Now 75 m./sec. cannot very well be exceeded with ordinary manganese-bronze or phosphor-bronze castings for the end shields. By employing, however, certain steel alloys of very low magnetic conductivity and high tensile strength, further progress in this direction appears to be possible. The difficulties connected with balancing, which rapidly increase with increase of circumferential velocity, must, however, not be disregarded. Even more important than the first balancing is the steady running and maintenance of balance under continuous work. It is therefore infinitely preferable to aim at such further improvements as do not entail any further increase in the velocity.

Directing our attention to the permissible A S values, a further possibility seems to lie in the careful adjustment of the commutating field. This would have for its object the extension of the A S line without exceeding the sparking limit. As is well known, in machines with artificial commutation additional short-circuit currents and sparking can only occur if the E.M.F. induced by the commutating field is different from the reactance voltage. Now as the latter increases exactly in proportion to the armature current, the characteristic of the commutating field should be exactly a straight line. Unfortunately this is not the case, and, in fact, it is mainly the deviation from this straight line which ultimately causes sparking and limits the permis-

sible A S. Even for the very low saturations obtaining in the commutating poles of turbo-generators the characteristic of the commutating field is slightly convex instead of being exactly straight.

The author has found that the following method allows an adjustment of the form of the characteristic. A shunt parallel to the commutating poles is employed, the resistance of which increases rapidly with increasing load. Iron resistances of the kind used for Nernst lamps are particularly suitable, although thin copper wires may also be employed, for which without enclosure in an air-tight bulb high temperatures may be permitted. As will easily be seen, the ampere-turns of the commutating poles as a function of the load will now rise according to a concave curve, thus compensating for the curvature of the characteristic. Experiments have shown that almost exact straight line relationship between the intensity of the commutating field and the load is thus obtainable within very wide limits.

Coming finally to the flash-over limit, the possibility might be suggested of improving the commutator construction with the view of preventing flashing over. This would permit of a higher voltage per segment, and would enable us to work with a larger magnetic flux and thus to increase the output. It must not be forgotten, however, that even if this were possible, an increase of the output by means of larger flux is, in principle, connected with a lengthening of the armature, and means therefore at the same time an increase of the reactance voltage and of the commutation difficulties. Thus what might be gained in flux would at least partly be lost again by the reduction of A S thereby necessitated.

There is no likelihood of decided improvements in the direction of increased flux unless means are found which would enable the armature to be lengthened without exceeding the flash-over limit and simultaneously without increasing the reactance voltage. This can only be accomplished, it seems, by a suitable armature winding, and I believe that it is in this direction that important developments must be looked for.

A brief description of a new winding which has the above aim for its object may be permitted, although no experimental results can as yet be communicated.

In machines working in the near proximity of the above calculated limit the number of commutator segments is in most cases extremely small, and the width great. Leaving out of consideration multiple windings, which in practice have proved unreliable, it is impossible to increase the number of segments, as this would cause a proportionate rise of A S and of the reactance voltage. It has been repeatedly proposed to adopt additional segments connected to suitable points of the armature coils, and to increase thereby the number of bars without increasing at the same time the A S value. The intention is to commutate each coil step by step and to obtain a reduction of both the reactance voltage and of the voltage per segment.

Fig. 4 shows diagrammatically such a winding, in which in this

manner the number of commutator bars is doubled. There is no doubt that the voltage per segment is actually halved. As regards the commutation, however, it will be found on closer examination that the reactance voltage is by no means necessarily reduced, because the self-induction of the connecting wires always adds itself to that of the conductors undergoing commutation. In practice the connectors will be necessarily arranged in holes in the armature iron. They are surrounded entirely by magnetic material, and the self-induction of a connecting wire will most likely be far higher than that of a conductor. Assume, for sake of argument, that practically no current at all would pass through the connecting wires to the commutator, owing to their very high self-induction. Then the reactance voltage would be twice as great as in an ordinary winding, as the time of commutation would be reduced to a half. If, on the other hand, the resistance and

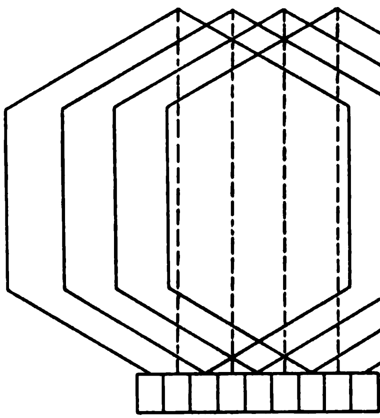


FIG. 4.

the self-induction of the connecting wires were similar to that of the ordinary commutator lugs, the reactance voltage would be halved. From this it is seen that whether the auxiliary segments improve the commutation or otherwise depends entirely on the self-induction of the connectors. Now there is a simple way for the neutralisation of their self-induction. If they are arranged in groups in such a way as to have connectors of opposite polarity always in close proximity to each other, so that the number of ampere conductors in every one of these groups is always zero, practically no lines of force will then be interlinked with any such group. Their self-induction is thus eliminated. This will be more clearly understood by considering a bipolar machine in which all the connectors are arranged in one group—for instance, in a hollow shaft. At any instant the connecting wires will carry exactly the same amount of current in both directions, so that no change of magnetic flux can take place. Fig. 5 will make the arrangement clear.

In the practical construction of this winding it is by no means necessary to make the number of groups equal to the number of pairs of poles, although in 4- and 6-polar machines this does not create any great difficulty. The number of groups may be made as large as convenient if only care be taken to have in each group wires which are displaced by 180 electrical degrees. The objection could be made that there will always be irregularities in the distribution of the current and induction effects due to the same. To prevent irregularities as completely as possible, the connectors are joined to the equalising rings at the back of the armature, and furthermore they are arranged in copper tubes through the armature core, which exercises a very strong damping effect on any remaining magnetic oscillations. The connectors are made of considerable size, so as to keep the ohmic resistance as low as that of the ordinary commutator lugs. Satisfactorily running

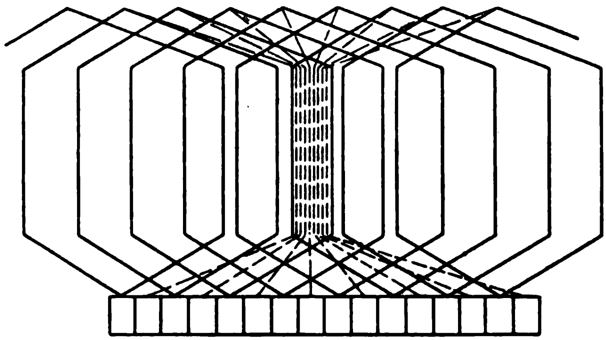


FIG. 5.

machines of twice the present armature length, and of twice the output at present obtainable, appear to be within the possibilities of this winding. The necessity of arranging two machines in tandem would thereby be removed, and even for such outputs and speeds which at present can be obtained by means of one machine only the application of the new winding should mean a very great margin of safety as to sparking and flashing over. Such results, however, would, probably, by no means form the limit of what may be expected from the future as regards D.C. turbo-generators. We are only at the very beginning of their history, and, in spite of some reverses, the great progress made during the last few years justifies the confident expectation of much more important developments.

DISCUSSION.

Mr. G. STONEY: The paper gives a great deal of information which I do not think has hitherto been published, but there are several points to which I should like to draw attention. Mr. Stoney.

In slow-speed machines with clean commutators the author says

Mr. Stoney.

that the flash-over limit is reached if the voltage per segment reaches a maximum of about 60 volts. This I cannot trace in modern slow-speed dynamos, which have nothing like a maximum voltage per segment such as this. In Silvanus Thompson's "Dynamo Electric Machinery" there are a large number of examples given, but the highest there is about 34 and the next highest 21 volts. Again, he says that the safe limit in the case of turbo-generators is about 40 volts, and I may say that this is true if the mica is thin; but if, as in modern turbo-generators, the commutator is built with rather thick mica, a higher limit is quite safe. We have made several 2-pole machines for 550 volts, with a maximum of about 49 volts per segment, which have been perfectly satisfactory. The curve of ampere conductors per centimetre of armature circumference, or what is the same thing, which is often taken in this country, of ampere-turns per 1 in. diameter, seems to be approximately correct, but rather on the low side, as we find that, even allowing a turbo-generator to carry 50 per cent. overload without serious sparking, and capable of being varied from no-load to 25 per cent. overload without sparking or shifting the brushes, considerably higher figures can be taken with surface-wound armatures. These are, to my mind, much preferable to sunk wound, where, as in turbo-generators, the very best conditions of commutation are requisite to get the best results. I may further mention that Messrs. Parsons have obtained much better results with 2-pole machines than with 4-pole, there never having been a case of flashing over with a 2-pole machine with our compensated winding or of injurious sparking, and the cost of 2-pole machines has been found not to exceed that of 4-pole machines, so that now 2-pole machines are being built up to outputs of 1,000 k.w., and we are prepared to build them up to 1,500 k.w. in single dynamos. This leads on to what Dr. Pohl says on page 243, that for outputs of over 500 k.w. it is not possible to construct direct-current generators running at so high a speed as the equivalent turbines demand. This was true perhaps six or seven years ago, but, I am afraid, is now very much out of date. We have made several 750 k.w. running at 1,800 revs. per minute, and 1,000 k.w. running at 1,500 revs. per minute, which is exactly the same speed as that of 1,000-k.w. 50-$\sim$ 4-pole alternator, and, as is well known, turbines of this size and speed give very economical results. Similarly, as the ampere-turns per 1 in. diameter go up rather with size, a 1,500 k.w. would run at about 1,100 or 1,200 revs. per minute, and this is quite in accordance with modern practice in Parsons' turbines. It will thus be seen that at present the limit is somewhere about 1,500 k.w., but it is quite certain that with the steady improvement in design of turbines and generators this limit will very soon be as much out of date as the man who said in the early sixties that it would be neither safe nor advisable to cross the Atlantic faster than 12 knots an hour. The whole thing depends on the struggle that has always been going on between the turbine people and the dynamo people for the one to lower their speeds and the other to increase theirs for any given size.

With reference to Dr. Pohl's remarks on homopolar machines, we have gone carefully into these, and have come to the conclusion that they are not practicable; and as to his method of doubling the number of commutator segments, if this proved satisfactory it would be very valuable, but I much doubt whether it will be found possible in practice to equalise the self-induction of the conductors which are connected to the commutator by the outside and the inside of the armature, and unless the self-induction of these two is absolutely the same; or in other words, unless the self-induction can be made negligible, it is obvious that there will be more tendency to spark at certain segments than at others, and thus the commutator will rapidly become uneven. Mr. Stoney.

We may say that we have found in our practice that unless the winding of the armature is absolutely symmetrical, sparking at certain segments takes place, and this was found to be especially the case in some machines in which the conductors were grouped slightly closer together at one part than another to provide extra ventilation.

Mr. E. J. Fox: Speaking from the steam turbine builder's point of view, I am glad Dr. Pohl has persuaded his company to take up the manufacture of dynamos for this special purpose, as without doubt the absence of satisfactory continuous-current dynamos on the market has hindered the more extended adoption of direct-current turbine sets. I am afraid in this particular matter we must plead guilty to being considerably behind the Continent. On the Continent slow-speed direct-coupled sets have been the universal practice, and there has therefore been a greater incentive to introduce direct-current turbine sets at a considerable saving in prime cost than has been the case in this country, where satisfactory high-speed sets are available. I think there is no doubt that until recently we have had to look to the Continent for this particular type of dynamo. Until the last year or two my firm have kept clear of direct-current dynamos for coupling to their turbines for the simple reason that we did not think they were sufficiently reliable, and it was only after inspecting a good many plants on the Continent that we appreciated that big sets were being built which to all intents and purposes could be considered satisfactory. Only last week I was speaking to one of the leading engineers on the Continent who is continually putting down large plants, and his impression was that the turbine dynamo as built at present by one or two firms is certainly quite good enough, and he added that he would never think of spending extra capital in putting down reciprocating plant. As far as I have been able to judge from observation, one has to look to the slotted core armature with carbon brushes for a satisfactory machine, and personally I am very doubtful whether anything running without carbon brushes can be considered sufficiently satisfactory. I was looking only the other day at a 1,000-k.w. set running at the Salford Station, and certainly the performance of that machine is in every way satisfactory. The brushes require more attention than an ordinary slow-speed set would do, but short of that one point, which after all is not very much to sacrifice in return for the advantages Mr. Fox.

Mr. Fox.

which are obtained with that type of plant, there is nothing that can be said against it. I understand that dynamos are now being built up to 1,800 k.w. in one armature, running at a speed of about 1,000 or 1,100 revolutions. Until recently I think the output has been limited to about 1,100 or 1,200 k.w. at the outside in a single machine. There is only one other point I would like to remark upon, and that is the question of speed which Mr. Stoney has just referred to. I am afraid there is a tendency to overdo the question of speed. We have now arrived at a stage when these dynamos can be built to run satisfactorily at certain speeds, and the tendency at once is to put up those speeds in order slightly to reduce both the first cost and the steam consumption. I am afraid that, with the competition which is prevailing both in this country and on the Continent, the tendency may be to overdo the question of speed and to sacrifice reliability and satisfactory running to a slight saving in first cost.

Mr. Miles Walker.

Mr. Miles Walker: Dr. Pohl's paper is particularly interesting at a time when the direct-current turbo is struggling to oust the big engine-type generator. It will be interesting to watch the progress of design during the next few years and see the outcome. The author has looked at the matter from the designer's point of view. I will try to put myself in the user's place, and see what characteristics a direct-current turbo-generator should have in order to give universal satisfaction. The user would like a direct-current turbo to have all the good qualities of the massive engine-type machine.

Let us consider what these good qualities are.

1. *Carbon Brushes.*—The turbo-generator, if it is to oust the engine type of machine, must have carbon brushes, or, at least, brushes which hardly wear at all and do not wear the commutator. It is true that metal-brush machines are made to operate fairly satisfactorily in the hands of careful attendants. But if the brushes are left without attention they soon wear so as to cover the wrong arc and begin sparking and burning. Even in the best hands the wear of the brushes is considerable, the whole set having to be renewed every two or three months. The wear of the brushes fills the air with fine copper dust, which settles on insulating surfaces, and is often the cause of breakdowns. Carbon brushes can be made which will run for years without renewal, always occupying the same arc and giving to the commutator a bright gloss, so that after years of running only the slightest wear can be detected. Electrical engineers owe a great deal to the makers of carbon brushes for the way in which they have solved the problem of brush manufacture and given us such excellent brushes. It is a pity to throw away all the advantages of carbon brushes and go back to metal brushes. Another advantage of the carbon brush is that it does not require such a fine adjustment of the commutating pole. We have just heard something of the convex saturation curve of the commutating pole. With a carbon brush the designer is not much troubled with this convexity. All that is necessary is to put more than the strict theoretical number of ampere-turns on the commutating pole. At half-

Mr. Miles Walker.

load the ampere-turns are theoretically too high, but a carbon brush at half-load does not mind a little thing like that. At full-load the ampere-turns may still be a little too high, but it does not matter. On a sudden heavy overload the ampere-turns, even if rather too few, are sufficient to stop the sparking which would occur with metal brushes.

2. *Maintenance of Balance.*—It is very important that a turbo-machine should compare favourably with the engine type in the matter of balance. The going out of balance has been a troublesome defect with turbo-machines in the past, but is being cured with better design. If carbon brushes are used the arrangements must be such that even if the balance is bad the brushes will not leave the commutator. It is possible to accomplish this.

3. *Shrink Rings.*—The row of exposed shrink rings on a direct-current turbo-commutator is an objectionable feature which does not appear on the engine-type machine. This must be done away with on all 500-volt machines. It is true that many commutators with shrink rings are running satisfactorily. The commutation is good, so that the flash from brush to ring does not very often occur. But I am looking at the matter from the user's point of view. If I had to bend over a commutator to attend to brushes I should feel very much safer without those rings. Think what we have—a steel ring running within a fraction of an inch of both positive and negative brushes. Suppose that the ½ in. of mica surface which insulates the ring from the commutator gets coated with copper dust, and the opening of a circuit-breaker makes a rise in pressure, a flash might occur right in the face of the attendant.

4. *Voltage between Commutator Segments.*—Dr. Pohl takes 40 volts per segment as a safe figure to work to. From the user's point of view I should prefer 20. If we look into the design of good engine-type generators we shall find that the best practice is to have as many as 48 commutator bars per pole for a 550-volt machine, giving a mean pressure of 11½ volts, or a maximum of 17 volts between bars. This has partly come about in obtaining good commutation, but it is this high number of bars that makes the engine-type machine comparatively free from flash-overs, even when abnormal rises in pressure occur. If I were a user of a direct-current turbo, say for traction work at 550 volts, I should insist on as many as 48 bars per pole with a fair uniformity of distribution of potential, allowing about 20 volts per bar as a maximum. From the designer's point of view this means a more expensive machine. We have tried to increase the acceptable voltage per bar, and to persuade ourselves by experiment that we can go up to 60 volts per bar, and that 40 volts is safe; but we cannot deny that if we were the users we would prefer 20 volts. One might just as well reduce the factor of safety in a mechanical structure as increase the voltage per bar on a commutator.

5. *Ease of Repair.*—This is an important consideration which is apt to be lost sight of in the many difficulties with turbo-generators.

There is hardly time to say how all these points may be met. The

Mr. Walker. method of reducing the volts per bar shown in Fig. 4 has been employed by the American Westinghouse Company for a number of years with good results. It is rather difficult to support the connectors running through the spider in a satisfactory way, and either a gun-metal spider should be employed or another circuit for mutual induction provided to kill the self-induction. The method shown in Fig. 5 is good, but it still involves an awkward mechanical construction and would be difficult to repair.

I show in Fig. A an alternative method of increasing the number of bars in the commutator without increasing the diameter of the armature. The iron of the armature is built up in two separate batches, separated by an inch or two. One coil of the main winding is shown by the full line. Its ends are connected to bars Nos. 1 and 3. A small

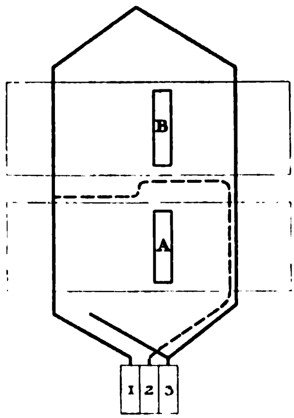


FIG. A.

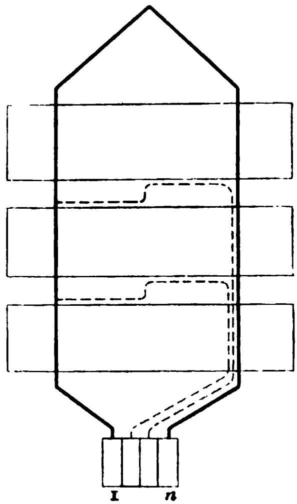


FIG. B.

auxiliary coil, shown dotted, has one end electrically connected to bar No. 1, and its other end connected to bar No. 2. It will be seen that the circuit from bar 1 to bar 2 embraces half the magnetic circuit of one pole, while the circuit from bar 2 to bar 3 embraces the other half, so that 2 is always midway in potential between 1 and 3. It is easy to show that under the action of the commutating poles A and B bar No. 2 delivers its share of the current to the brush and the current is reduced to zero before bar 2 leaves the brush. Any dissymmetry in the self-induction of the circuits 1–2 and 2–3 can be allowed for in the adjustment of the commutating poles A and B. With this type of winding there is no limit to the number of bars we can have. The iron can be broken up into a number of batches, as shown in Fig. B. The auxiliary windings are placed in the bottom of the slots and connected so as to

Mr. Walker

keep bars intermediate between bars 1 and n at points of intermediate potential.

The advantage of this type of winding over those in which connections are brought through the spider is that it does not present as great mechanical difficulties.

Dr. Pohl, in Table I., takes a machine with a diameter of 80 cms., running at 1,800 revs. per minute, and says that the biggest possible output is 670 k.w. Running at 1,500 revs. it would be about 550 k.w. Adopting a winding of the kind shown in Fig. A, it is possible to get a machine of that diameter running 1,500 revs. with 1,000 k.w., or almost double the output given in the table.

I cannot now go into the various ways in which carbon brushes can be made to work at high speeds, but I can give some results. A machine of 250 k.w. running at 3,000 revs. with carbon brushes was run for ten days on continuous load. No attention was paid to the brushes during the run. At the end of the time the brushes had just bedded themselves; the commutator was absolutely unworn. The commutation was excellent, and it was possible to adjust the commutating ampere-turns through a range of 20 per cent. without making any appreciable effect, which shows that the carbon brush can be made to work and that it has very great advantages indeed.

Professor Silvanus Thompson.

Professor SILVANUS P. THOMPSON: Dr. Pohl has placed before us in so very clear a way the main points for our consideration that there is no need to re-state them; but we have to thank him in particular for having pointed out a limitation which, although known certainly before, has not been put forward, so far as I know, as a definite limitation for the rating of a machine. I am old enough to remember the time when the limitation of the number of kilowatts at which a machine might be rated was the likelihood of its armature flying to pieces. Long ago that limit of rating gave way to another, namely, that the voltage must not drop too much, and the nominal output of the rating was limited when the voltage drop was thought to have arrived at a sufficient point. Closely connected with that is the heating which accompanies the voltage drop. For a long time there has been a double limitation to the rating of the output of a machine. It must not be rated so high as to be liable to spark, even with an overload of some prescribed percentage, and it must not be liable to heat above prescribed limits at its ordinary maximum load. I have no doubt whatever that there are some machines in the market to-day which will come to the sparking limit before they come to the heating limit; while most machines to-day are so well arranged in commutation that the output is governed by heating rather than by sparking. And the point is not unimportant, though it is not the question under our consideration to-night, because at the present moment the International Commission is discussing the question of the rating of the output of a machine, and in one of the documents put forward the only consideration for limiting the output of a machine was the temperature limit. Yet I protest that it is not physically the only consideration. But

Professor Silvanus Thompson.

to-night we have another limitation, namely, the liability to flash over, which, as Dr. Pohl tells us, may determine the maximum normal output of a machine which otherwise for either overheating or sparking troubles would be able to go quite satisfactorily to a higher output. The flash-over limit is a very real thing when we come to high speeds, with their unfortunately necessarily small number, comparatively, of commutator segments per pole. It seems to me that in the two parts of the matter which Dr. Pohl has put before us—commutating machines and non-commutating machines—we have really two distinct classes of machinery. The commutating machine—I mean the commutating high-speed turbine-driven machine—is entirely unsuitable for a low-voltage machine; it is only suitable for ordinary high voltages, for one would not dream of building a machine to generate at such low electromotive forces as 60 or 80 volts, on the principle of the commutating turbo-generator. But, on the other hand, the homopolar or acyclic type appears, in spite of the other defects it has, to be particularly well adapted for very low voltages. We have to thank the author for having pointed out so emphatically a defect which appears, I think, in his judgment to be inherent in the acyclic machine, namely, that for a given output and a given speed they are so extraordinarily heavy. We knew they were heavy in proportion to their output, but I had no idea that the heaviness ran to something like ten times that of an ordinary machine. It is rather a revelation to hear of this 1,000-k.w. machine, of which the ironwork alone would weigh 51 tons; it would be an unendurable monster.

It is of some interest to look at what has been done, and I have brought down a drawing, which was given to me by Mr. C. E. L. Brown fifteen years ago, of an actual homopolar or acyclic machine, of which he had supplied, I think, more than one example for certain electrolytic work. It is a machine to give out 5,000 amperes at 10 volts, running at a speed of 1,200 revs. per minute, with the main body of the machine made not of cast steel but of cast iron. It is curiously reminiscent in form of the drawing which Dr. Pohl has given in Fig. 3, and it is fifteen years old.

The difficulty occasioned by the risk of flash-over when the brushes are in close proximity to the shrunk-on rings on the commutator has been alluded to, and on that I want to say two things. I believe that in certain machines—I think they were of Continental build—in order apparently to provide against flash-over, an unusual arrangement of the brushes was made. Two-pole machines will naturally have brushes at the opposite ends of the diameter, say one set of brushes near the top and the other set near the bottom. In the machine to which I am alluding the commutator was very long; it was confined in the middle by a shrunk-on ring; the top brushes were placed not along the whole length, but only on half of the commutator, and the bottom brushes were on the other half of the commutator, so that a flash-over could not literally go from brush to brush. A flash-over could, of course, go round the commutator, but it could not go from brush to brush because

the brushes were not opposite one another at the same part of the commutator. I would like to ask those who have had experience of this machine whether that device is really of any useful effect, whether that unusual disposition, which certainly has obvious disadvantages (it involves a longer commutator unless something else is done), is really an effective thing in obviating flash-over. Secondly, the risk of flash-over occasioned by the presence of shrunk-on rings can certainly be diminished by a further device—I do not know whether it is in use—namely, of abandoning reliance upon the projecting quarter inch or so of mica underneath the shrunk-on ring, and placing an annular disc of mica against the face of the shrunk-on ring, so that there shall be no risk of any small amount of carbon or copper dust which might settle there making a conductive path to the shrunk-on ring. I think an insulation on the flanks of the shrunk-on rings might be a distinct advantage, although I have not seen such. Professor Silvanus Thompson.

Lastly, I have to make entirely at random a suggestion which has occurred to me during the course of the discussion. If we go from a very large machine to a thing that is extremely small—namely, the motors that are employed in the Elihu Thomson motor meter—there is, as is known, a great advantage in the employment of silver for the commutator; the friction is enormously reduced. As a matter of fact, small motors with silver commutators instead of copper are to be found described some seventy years ago in Daniel Davis's book on magnetism. Has any one tried—it would not be really expensive if silver were cheap enough—making a silver commutator, to see whether there is any chance of really improving the friction, which in this type of machine appears to be serious?

Mr. A. C. EBORALL: I have read Mr. Pohl's paper with very great interest, and only regret that for various reasons I must content myself with raising a few points relating to the construction of direct-current turbo-dynamos. The first point to which I should like to draw attention is the absolute necessity for making a good mechanical job of these machines. I think it is pretty safe to say that a great part of the trouble that has been met with in the past with this class of machine has been due to purely mechanical causes. In this connection the cool, smooth, and vibrationless running of the commutator is of the greatest importance, for as soon as ever these conditions are departed from these high-speed commutators at once give rise to commutation troubles. Variation in the balance of the armature is a common cause of the commutator getting bad, and that is frequently due to the conductors moving. It is very important to design the armature so that those movements which are the unavoidable result of alternate heating and cooling are limited to directions which the designer had in mind when he designed the machine. Then, again, a uniform temperature throughout the armature should be aimed at, a low temperature rise measured on parts accessible to the user being far less important than a temperature-rise of perhaps greater amount, but which is reasonably uniform throughout the machine. This implies, of course, Mr. Eborall.

Mr. Eborall. a positive system of ventilation. Finally, electrical and magnetic symmetry of the whole construction is very important, involving a proper arrangement of equalising connections on the armature and also very careful design of the magnetic circuits. Perhaps I may be permitted to mention quickly the way in which we have tried in our designs to carry out these ideas. In the first place, as far as ventilation is concerned, the whole turbo-dynamo is totally enclosed, with the exception of a comparatively small opening at the top, and another at the bottom, of the field-magnet casing, the latter opening serving for the entry of cool air, and the opening at the top serving for the exit of the warm air. The cool air enters, therefore, at one end of the machine, it being drawn through from outside by reason of vanes placed on the rotating armature. Both the field and armature systems are laminated throughout, each being provided with a large number of ventilating ducts, and the stream of cool air, drawn from outside in this way, is made to circulate through the whole of the armature and field masses (copper and iron) and along well-defined paths which are not left to chance, before being ejected (as hot air) through the chimney-shaped opening at the top of the casing. There is not a single part of the machine which is not reached by the cool air, and the result is that for a certain definite temperature-rise the machine expands uniformly and equally, and our experience is that once a machine ventilated in this way has been properly balanced (it being understood that the conductors, and especially the end and equalising connections, are properly secured), it remains in balance permanently. With regard to the brushes, those in our machines are a combination of metallic and carbon brushes. The current is collected mainly on the metal brushes, and just ahead of the metal brushes small pilot carbon brushes are placed, the object of these being, not to assist commutation, but to clean and lubricate the commutator. We have no practical acquaintance with the flashing-over troubles which have been so feelingly referred to by other speakers, or with the accumulation of copper or metallic dust, which has been mentioned by Mr. Miles Walker; and with regard to flashing over, I may say that out of the very large number of direct-current turbo-generators we have built we have only had one case of flashing over, the cause of which was soon ascertained and duly remedied. The enclosed design of the field system I have described is very good in another way which I omitted to mention. It enables the machine to run silently, while, if there is any dust flying about it cannot readily get into the field system or armature. Another feature of these dynamos, which is unusual with direct-current machines, lies in the fact that we provide direct-coupled exciters for them all, so that the shunt windings are always separately excited. The object of this is to secure, under all conditions, a positive excitation in the shunt coils irrespective of what may happen in the main circuit, such as a return current. Incidentally, there is another advantage in employing direct-current exciters—it does not, luckily, often come into play, but still it is there—and that is that if, for instance,

the load is thrown off and something happens to the governor of the turbine so that the machine tends to race, the rising pressure of the exciter so appreciably increases the load on the exciter and the losses in the dynamo that a dangerous speed is hardly attained. With regard to the construction of the field system, we do not use polar fields, but employ the Deri field construction, which resembles the stator of an induction motor, that is to say, it is circular, formed of stampings, and is continuous all round the internal periphery with the exception of a number of partly open slots, which are of two sizes. The larger slots carry the shunt coils, while the smaller slots carry the compensating windings of heavy copper strip. There is in addition to this compensating winding a commutating tooth with an independent winding on it, in series with the compensating winding. With regard to the question of insulating the commutator shrinking rings, we are in general against the suggestion, partly because it is difficult to make a good mechanical job of this, but also because it does not get to the root of the trouble, but merely constitutes an attempt to evade it. As already stated, flashing over does not occur with a well-designed machine, unless, of course, it is improperly treated. With regard to the question of size, we have had machines of all sizes between 1,000 and 1,500 k.w. running for some years, and we are able to build dynamos up to 1,800 k.w. in output in a single armature. Mr. Eborall.

With regard to the questions which have been raised in respect to the wear and replacement of brushes, I may say that, although the former is certainly in excess of what one gets with, for instance, a dynamo driven by high-speed engine, it is not anything serious. About one set of brushes per annum is quite a fair allowance for a 1,000-k.w. dynamo when properly looked after and when running the number of hours usually met with in direct-current stations, more negative brushes being required than positive ones.

Dr. GISBERT KAPP: Dr. Pohl has made a successful attempt to give in a general way the limits of output for turbo-generators, when the commutation is either by contact resistance and forward lead or by the use of commutating poles. One can understand his favouring commutating poles because he has, for moderate speeds, been very successful in the design of interpole machines. Yet it is doubtful whether the output limit can be determined on the basis of commutation by so-called interpoles. The author himself mentions one disturbing influence, namely, the convexity of the characteristic of the commutating pole. There is, consequently, not the absolute balance between the impressed field from the commutating pole and the self-induced field in the slot which we require for perfect commutation at all loads. But there is another difficulty. Supposing that by very low saturation we could get a straight characteristic for the commutating pole, the machine would still spark when used on a circuit having very quick changes of load, say on tramway work. I have seen turbos with commutating poles made by a very good firm and originally intended for use on a tramway, but actually working on a lighting load, Dr. Kapp.

Dr. Kapp. because the engineer found that these machines could not commutate properly under the quick changes occurring in tramway service. On a lighting load the machines commutated perfectly. That is a comfortable sort of load, because it varies slowly; but with a tramway load, which may jump up and down by, say, 80 per cent. from the average in the space of a few seconds, it is very difficult for the commutating pole to follow. Generally the exciting coil of the commutating pole is made a little stronger than actually required, and the excess is shunted. Dr. Pohl has mentioned a shunt which would be either purely ohmic or may also have some self-induction. The latter is, of course, essential, and it is also essential that the relation between resistance and inductance should be the same in both circuits. The shunt must therefore not only be adjustable as regards resistance, but also as regards self-induction, a condition very difficult to fulfil in practice. I do not deny that shunted commutating poles may do very well when the oscillations of the load are not too quick; and where machines with commutating poles are used the limit of output which Dr. Pohl has given will probably be correct. But where such poles are not used and commutation is produced in a different way, the author's calculation no longer applies. Such a new way of producing sparkless commutation up to enormous overload has been devised by Messrs. Parsons and Stoney. I have seen machines built by Messrs. Parsons and Stoney without commutating poles working on tramway loads absolutely without sparking. The commutating field is produced in air, and as air is not a hysteretic material and has no saturation point, we get the absolute balance between the field in the slot and the impressed field necessary for commutation. Machines of that kind can be heavily overloaded. If we take the author's view and assume that the load is limited by sparking, and if we find that a machine commutating by a field passing through air can work with 100 or 150 or 200 per cent. more load than the interpole machine, then obviously the author's very ingenious calculation of load limit ceases to be applicable. By using commutating winding where the lines run through air we raise by a considerable amount all the output limits the author has given in his paper.

Mr. Evershed. Mr. S. EVERSHED: I should not have ventured to intervene in this discussion if it had not been for two remarks made by previous speakers. Dr. Thompson referred to the possibility of using silver commutators. No doubt silver commutators would work admirably when silver is about half its present price, or lower, but I must disabuse Professor Thompson's mind of one fallacy. The coefficient of friction between the silver brushes and the silver commutator is just about the same as that between copper brushes and copper commutators, that is to say, it is about 0·15. I am told that the true advantage of the silver is that the oxide, or sulphide, of silver is a conductor of electricity and not an insulator as in the case of copper. The other point to which I wish to refer is one which occurred to me whilst Professor Kapp was speaking. I think I was among the first in this country to

Mr. Evershed.

apply a commutating pole to a dynamo. About twelve years ago I was using a dynamo to run a number of motors. The machine ran very well when driven by a steam engine, but on replacing the steam engine by one of Crossley's "scavenger" gas engines fitted with a rather light flywheel the dynamo sparked badly. The machine was, in fact, subjected to very much worse conditions than those which are met with on a tramway load; the current varied during every cycle of the engine from about half-load up to something a good deal over full-load. I thereupon designed a commutating pole for the machine. It was a 2-pole dynamo, and as there was no room for an interpole between the field-magnet coils, I fixed the one commutating pole on the top of the pole-pieces. I anticipated the trouble to which Professor Kapp has referred. I realised that the larger part of the magnetic circuit of my commutating pole must be air in order that it might follow the variations of the current as closely as need be, and I further realised that the pole must be laminated. My commutating pole was nearly as large as the field magnets and had a somewhat eccentric appearance, but it worked very efficiently. I have only one other remark to make, and that comes back to something Dr. Thompson said. It occurred to me it is just possible that the flash-over limit has not been reached at 40 or 50 volts. I make a good many machines in which the commutators have 500 volts between the segments, and I have never had a flash-over yet, the reason being that I use air instead of mica between the segments of the commutator.

Mr. Ellis.

Mr. A. G. ELLIS: This paper emphasises the fact that high speeds are not desirable for the driving of continuous-current machines. The many difficulties referred to in the paper show that the additional devices, for which provision must be made, tend to complicate rather than to simplify the construction, and at the same time increase the cost of manufacture. Although for a considerable range of outputs the standard speeds of steam turbines permit of more or less satisfactory dynamo designs, it is not the case that the turbine speed corresponds to the most economical design, consistent with good operating quality, that can be obtained for a given output. The most economical designs are obtained at speeds considerably lower than the steam turbine speeds. In the diagram (Fig. C) the curve D is a representative curve for the speeds corresponding to the most economical design consistent with good quality. This curve is based on the general results obtained from a large number of preliminary designs. Curve B is reproduced from Fig. 2 in the paper, which represents the author's figures for the maximum output and speed. The curve D is considerably lower than the curve B, which indicates that although designs can be carried out for the speeds as high as those given by curve B, these designs are not the most satisfactory and do not correspond to maximum economy. Curve A is the curve for the Parsons' turbine speeds, and curve C for the Curtis turbine. It is interesting to note that the author's curve practically coincides with the Curtis turbine speed curve. Hence the Curtis turbine would appear to be more suitable for continuous-current

Mr. Ellis. machines than the Parsons' turbine. Curve E indicates roughly the speeds for which continuous-current machines can be designed with such commutation constants that no recourse is needed to auxiliary poles or special commutating devices. Beyond the speed indicated by this curve the cost of a machine for a given output decreases on account of the increased speed until the speed on curve D is reached. Beyond Curve D the cost increases on account of the expense of the auxiliary commutating devices and the considerably increased labour costs. The chief point to be noticed from these curves is that the continuous-current machine is not inherently a high-speed machine. In spite of this fact machines have to be built to meet the steam turbine makers' speeds, and among the latter there is now a tendency to even higher

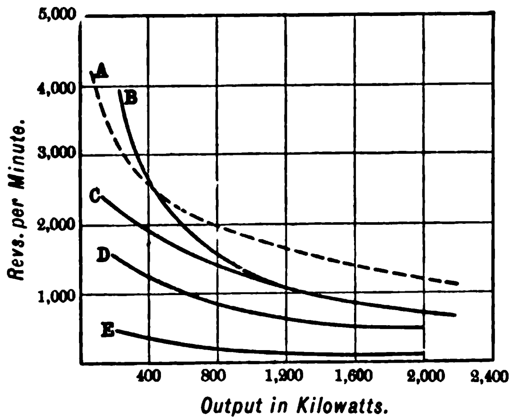


FIG. C.—Curves for Speed and Output of Steam Turbines and Continuous-current Generators.

A. Parsons' Turbine.
B. Dr. Pohl's Curve for Maximum Output.
C. Curtis Turbine.
D. Curve for Maximum Economy.
E. Limit for Normal Designs.
(Without Auxiliary Commutating Devices.)

speeds than the present standards on account of the increased economy at the steam turbine end of the generating set.

The second point I wish to mention has already been touched upon by Mr. Eborall. It is interesting to note how the coming of high-speed prime movers has compelled dynamo makers to go in for forced draught ventilation. This system of cooling has led to a much finer degree of subdivision of the parts of the machine to enable the air to circulate freely throughout the interior. In this way the temperature throughout the machine is much more uniform than hitherto. In older machines there exist temperatures, at certain parts, considerably higher than the mean temperature or the temperature of the hottest accessible part. The thorough ventilation gives a greater degree of

safety from breakdown of the insulation due to local overheating, and it may be worth considering whether a higher standard of temperature rise might not gradually come to be permissible in such machines on this account. Mr. Ellis.

The third point to which I wish to refer has already been touched on by Dr. Thompson, and relates to the weights of the homopolar machine. I should like to ask the author to give more information as to why a homopolar machine requires so much as ten times the weight of steel required for an ordinary machine. On page 247 the author states that "the poor utilisation of materials is mainly due to the low A S (ampere conductors per centimetre) values," but it would be interesting if he would explain why these values must necessarily be low. In connection with the question of weight, it may be of interest to quote a paragraph from Noeggerath's paper* on "Acyclic Dynamos." He says: "While the total weights of both types are about equal even for moderate turbine speeds, the very low copper weight, simple construction—less labour and a smaller total cost—combined with elimination of the commutating problems, should speak favourably for the new type."

One other point on which I should like to touch relates to the special armature windings on pages 250 and 251. In this connection I think it of interest to refer to the Punga winding described in a previous number of the *Journal*.† This winding is a multiplex winding in which the voltage between segments may be reduced to one-half or one-third according to the multiplicity. The author is to be congratulated on the ingenious device shown in Fig. 5 of the paper.

Mr. V. A. FYNN: I should like to ask a question about this new winding, because I was not quite able to understand the passage with regard to it on page 250. I think what the author means when he says that no current at all will pass through the connecting wires to the commutator is that the arrangement is to be considered as equivalent to doubling the number of segments and connecting up alternate segments only. If that is so, and if the width of the brushes is reduced as the number of commutator segments is increased, so that the brush always covers the same number of segments, say two, then I rather think that the reactance voltage would be four times as great as in the original winding and not twice as great, because first of all, by reducing the brush width to one-half while increasing the number of segments to double their original number, the commutation frequency is doubled. Then as each alternate segment is blank, only half of that reduced brush is effective, and the frequency of commutation is thus again doubled. In that case, and if it is possible to reduce the self-induction of these connectors to zero, the voltage between the segments will be halved by using this new winding, but the reactance voltage of the half-coil included in each short circuit will be equal to the reactance voltage of a whole coil in the original winding. I should like to hear Mr. Fynn.

* *Transactions, American Institute of Electrical Engineers*, vol. 24, p. 1.

† *Journal, Institution of Electrical Engineers*, vol. 39, p. 600.

Mr. Fynn. if Dr. Pohl agrees with this. Then with reference to the copper tubes in which he wishes to place his connectors, I do not quite see how they are going to help in reducing the self-induction of the latter. If the ends of the copper tubes were connected, say, by a wire so as to enclose the flux circulating around the tubes, then I could understand it, but as it stands I do not—in fact, if these tubes are effective in the proposed position and arrangement, they should also be effective if placed in the slots carrying the main winding of the machine, and this last arrangement would certainly be very much simpler. Of course, placing all these connectors in a hollow shaft is a most difficult problem, and I shall be very much interested to see how it is going to be solved, especially as connectors belonging to coils displaced by 180 electrical degrees are to be placed side by side. This will mean that the full potential of the dynamo will exist between adjacent connectors inside the shaft.

Communicated: The author has said that the winding shown in his Fig. 4 is old; it may be at least of some historical interest to state just how long ago that arrangement was proposed for the first time. As far as I know the idea originated with Elihu Thomson in July, 1886. The Fig. 2 of his American patent, No. 392765, issued in 1888, is identical with the author's Fig. 4. It is most interesting that Elihu Thomson should have addressed himself to this very problem at such an early stage. He states in his specification: "The object of my invention is to permit the generation of electric currents of larger volume for a given size of machine than is practicable with the machines at present in use." His first claim reads: "In a dynamo, electric generator, or motor, the combination, with each single turn or length of armature conductor, of two *or more* commutator leading wires, as and for the purpose described."

Professor Robertson. Professor DAVID ROBERTSON (*communicated*): Dr. Pohl's conclusions regarding the homopolar machines are especially interesting and instructive, as they are quite different from what, at first sight, one would expect. It is, however, doubtful whether his proposed winding is the simplest way of increasing the limiting output. Some years ago I suggested* dividing the commutator and putting half at each end of the armature, so as to commutate only half a turn at a time. This (which I believe has been since tried by a Continental firm) would reduce the reactance voltage much more than the arrangement shown by Dr. Pohl, and would rather more than double the maximum length of core in view of permissible sparking considerations. It would not, of course, alter the limit set by flashing over, but it ought to be possible to eliminate that by increasing the thickness of the insulation between the segments, putting in dummy segments if necessary, and by compensating the armature reaction. Flashing over under heavy loads is largely due to the fact that the armature reaction crowds the flux to the trailing edge of the pole, and so makes the potential difference between adjacent segments at certain places much greater than the average.

* *Journal, Institution of Electrical Engineers,* vol. 32, p. 452.

The flashing-over limit can certainly be much increased by compensating the armature reaction over the whole circumference (for instance, by Ryan's method) instead of only doing it locally by commutating poles. Professor Robertson.

The author indicates one way of raising the sparking limit, namely, by making a more exact adjustment of the magnetisation curve of the commutating pole. Another way is to seek a better brush material. A brush is wanted with a positive resistance-temperature coefficient so that the resistance at the trailing tips shall increase as they heat up. In these ways the permissible limit of reactance voltage may be raised. One can imagine a machine of large output in which nearly the whole active length of the conductors acts as a commutator segment. It would have to be a ring winding with overhanging core and brushes inside as well as outside. This hardly seems practicable, as the difficulties are considerable, but still the thing is not absolutely impossible. Other suggestions are to enclose the machine in an airtight case and keep the internal air at a high pressure, to put a magnetic blow-out or an air blast to extinguish the sparks, or to make a commutator which will not be damaged by them.

Dr. M. KAHN (*communicated*): The essence of Dr. Pohl's interesting paper is the fact that for a given output there is a maximum number of revolutions for which a turbo-dynamo can be designed, and he endeavours to find means by which this speed limit can be raised to suit the most economical speed of Parsons' turbines. The conditions limiting this speed can be expressed in the following formula, which is derived in a similar way to the figures given in the paper:— Dr. Kahn.

$$\text{Revs.} = \text{constant}\ \frac{v \times \text{A S} \times \text{E}_{\text{seg. max.}}}{\text{k.w.}}.$$

From this formula it can be seen that if maximum values for surface speed, segment volts, and A S are chosen, the revolutions per minute for any output are given.

As the reactance voltage E_r of a direct-current machine is proportional to $\text{A S} \times v \times L_c$ (L_c being corrected length of armature allowing for end winding), the above formula can be written as follows:—

$$\text{Revs.} = \text{constant}\ \frac{E_r \times \text{E}_{\text{seg. max.}}}{L_c \times \text{k.w.}}.$$

This formula clearly shows the electrical speed limits, reactance, segmental voltage, and heating.

It will be seen that the speed can be increased by decreasing the armature length. This dimension is fixed by the densities in copper, teeth, and core, which values are mainly settled by the permissible heating. The heating limit cannot therefore be considered as lying above the output limit. A very stiff shaft is essential on these high-speed machines, and as the outside diameter of the armature must be kept small on account of surface speed, there is very little radial depth

Dr Kahn. left for armature core and air inlet, in 2-pole machines at any rate. The iron densities must therefore be raised as high as possible.

On the high periodicities, which are inseparable from turbo-dynamos, these densities are chiefly limited by heating. This limit has recently been considerably increased by the introduction of low loss iron alloys, which may be mentioned as one of the means of increasing the speed of turbo-dynamos, as their use allows of raising the density in the core and teeth.

Of Dr. Pohl's two suggestions for increasing the speed of turbo-dynamos, the device of a shunt, parallel to the winding on the commutating poles, can only be used if care is taken that this shunt has the same reactance as the winding on the commutating poles. Otherwise, in case of sudden increase of load, all current will momentarily flow in the shunt circuit, and the excitation of the commutating poles will be insufficient to ensure sparkless collection. It is also rather doubtful if the increase of resistance of the iron wire will be rapid enough to ensure the proper working of this arrangement with sudden variations of load, which is the worst condition for flashing over.

The second suggestion is to decrease the segmental voltage by connecting the back of the windings to additional commutator segments. This is a very effective means of dealing with the difficulty if the mechanical problem of fixing the connections in these high-speed armatures can be satisfactorily solved.

The solution by means of multiple windings must not be entirely neglected, as these windings, if properly arranged, will give satisfactory results, provided that equalisers are fixed at intervals between the different windings. This has been proved by a number of turbo-dynamos which are running satisfactorily with this type of winding.

Mr. Catterson-Smith. Mr. J. K. CATTERSON-SMITH (*communicated*): There are one or two points in the paper to which, perhaps, I may be allowed to draw further attention. Dr. Pohl arrives at an armature core length of 43 cms. by adopting maximum permissible values for the volts per segment and the mean gap density. While agreeing with the value given for the former limit, I consider that a mean gap density of 5,000 is too low, in fact, anything up to 60 per cent. higher than this is to be preferred. Consider the reduction of rotating material, of length of shaft between supports, and it will be evident that a value of the core length which is 60 to 80 per cent. of that given by Dr. Pohl has much to recommend it.

It must not be forgotten in this connection that the diameter of the shaft necessary at these high peripheral and high rotational speeds is of the order of 35 to 40 per cent. of the armature diameter, and that the deflection increases rapidly with the length. Further, it is a matter of common knowledge that the higher values of the flux density in the core reduces the distortion, in other words the ratio $\frac{B_{g\ max.}}{B_{g\ mean}}$ is less, or the output is increased; due attention, of course, must be given to any increase in the iron loss.

Mr. Catterson-Smith.

No mention is made, I believe, in the paper of the use of a compensating winding in cases where the maximum volts per segment approach the limit; this winding neutralises the armature distorting force at all loads and so reduces the ratio $\frac{B_{g\ max.}}{B_{g\ mean}}$ without recourse to high saturation of the iron, and for this reason the value of a is not restricted to that given by Dr. Pohl. The curves shown in Fig. 2 will therefore intersect at a larger output. Evidence of this is given by the machines lately built by Messrs. Siemens, Brown Boveri, Parsons, and others.

In the section dealing with possible directions in which improvement may be sought, Dr. Pohl describes his method of straightening the magnetisation characteristic of the commutation poles by an iron-wire shunt with a high temperature coefficient. This is a very neat arrangement, but is open to criticism if proposed as an adjunct to a generator on traction, or other rapidly fluctuating load, as the inductance of the commutation pole winding tends to an excessive current in the iron shunt and consequent lack of proportionate increase in the commutation pole ampere-turns with sudden changes of load.

The tapped windings shown in Figs. 4 and 5, and the same with a greater number of tapping points described by Dr. F. Niethammer,* appear to me to be doubtful improvements, owing to the disturbance of symmetry, and to the difficulty previously mentioned regarding the diameter of the shaft. On a turbo-dynamo it is essential for successful operation that the armature winding be of the simplest type, and the greatest care must be taken to ensure that the symmetry is as nearly perfect as possible.

Turning to the homopolar machine, this is one of the types of the future, as undoubtedly the advent of a device for collecting very large currents at a contact velocity of 200 or more metres per second will put the homopolar on a competitive basis with advantages on its side. The value of 32 given for A S places the machine out of the question; it will be necessary to collect sufficient current per contact ring to give A S a value of 100 or 200. The whole problem being that of collection, I think that Table II. is of little utility, though of interest. I hope that this paper will help to show that turbo-generators are good practice; the true and final test of all machines is reliability, and in this respect the turbo-dynamo of to-day is not found wanting.

LEEDS LOCAL SECTION.

DISCUSSION, *January* 23, 1908.

Mr. Hartnell.

Mr. WILSON HARTNELL: The paper is necessarily somewhat terse, many supplementary facts being omitted. One great limitation of output left out entirely is, that when running at a high speed it is necessary to reduce the number of magnetic lines per square centimetre in the

* "Turbo-dynamos," 1906, p. 71.

Mr. Hartnell. air-gap. The heating effect in the iron of the armature with a high surface speed and a high flux density under the pole-face is far greater than is given in the usual formula, so that it is necessary to reduce the flux to about one-half of what is practicable in ordinary generators. If Dr. Pohl could give us some information on this point as to the limitations of output, it would be extremely interesting. Mr. Parsons continues to use surface windings. Is this because the weak flux which the heating limits enforce allows surface windings to be used without loss of output? The limitation of length due to mechanical difficulties considerably exceeds the limit imposed by commutation difficulties. The active length of the armature at ordinary speeds is chiefly determined by the requirements of sparkless commutation with carbon brushes in a fixed position, but this limit can be exceeded by means of perfectly arranged interpoles. One limit named is the difference of voltage between the segments, and the author here speaks of this difference being increased by the distortion of the field, which may cause the flux to be 25 per cent. more at one corner of the pole-face than at the other. The author was, I believe, present at the meeting at the University when Mr. Hoult showed the compensation winding underneath the pole itself, which may prevent the distortion and correspondingly increase the generator output limit. The compensation winding under the poles was, I think, first used by Messrs. Brown Boveri, and has been adopted by Mr. Parsons. I have made designs for homopolar machines, but I abandoned them as not being satisfactory. Dr. Pohl's new winding halves the voltage between the segments, but this requires as many commutator segments as there are surface conductors. The space between the discs and the shaft is not very large, as it is necessary to keep the diameter of the turbo-armature small and that of the shaft large. Inside this limited space there must be half as many conductors as outside. There are also radial connections at the end opposite the commutator which must be made secure. When Dr. Pohl has satisfactorily and economically solved these difficulties he will have greatly extended the present commutation limitations to the output of continuous turbine-driven generators.

Mr. Law. Mr. A. H. Law: Mr. Hartnell referred to some difficulty—I did not just catch what it was—about the conductors on the surface-wound armature. Perhaps he referred to the eddy currents that are met with in this type of machine. These, of course, occur in a surface winding, but if the conductors are properly laminated they can be reduced to a negligible amount. Mr. Hartnell also referred to the compensating winding being introduced by Messrs. Brown Boveri. As a matter of fact Mr. Parsons devised a compensating winding in 1885. I have recently seen the original drawings of this arrangement. It was used on small dynamos and consisted of a ring winding round the back of the pole. Unfortunately, the experiments were not continued, or a compensated dynamo would have been an accomplished fact long ago. Temperature rise is certainly an output limit, looking at the matter in some ways, but from Dr. Pohl's point of view it is not; at the same

time it is a matter that requires considerable attention in the designing of the plant. However, it is a difficulty that has been satisfactorily got over and at present does not seriously limit our sizes. The causes of flash-over are extremely complex. Dr. Pohl speaks of one which is undoubtedly a very high contributing cause to this trouble, and that is the voltage per segment round the commutator, but I would point out that there are a number of other things which have a great bearing on this question. Mr. Law.

In the few cases where Messrs. Parsons have had trouble in this direction I am happy to say that it has been entirely overcome by comparatively simple alterations in the machine, such as increasing the thickness of mica between the commutator bars. The sets referred to were large 4-pole plants, and as proof that the voltage per segment is not the sole factor in producing flashing over, I may say that these plants were put in and run for some months at half their normal voltage. This was owing to the fact that the supply company were changing over from 250 to 500 volts, and they had to run these large generators at just over half their proper voltage. The first flash-over occurred when working at that voltage, and was due to short circuits outside, the maximum voltage per segment being under 20. Some time ago Messrs. Parsons built an experimental plant which was tested up to a maximum of about 65 volts per segment as against the safe limit of 40 volts mentioned in the paper. At this point heavy loads were thrown on and a circuit breaker knocked out without producing any flashing over. I do not mean to convey that 65 volts per segment is a safe working figure; these are merely the results of tests which have been made to try and produce artificial flash-over. One reason why Messrs. Parsons are able to work their armatures at rather a higher specific loading is the fact that they use the surface-wound type of armature. We have experimented with the slotted armature and obtained fairly satisfactory running, but we have not got them to run as well as the surface-wound armature, and I think we are beginning to convince users that the latter type is in all respects a more satisfactory mechanical job than the slotted armature. This is due to the fact that the conductors are laid on the surface with a uniform layer of insulation below and a similar layer between them and the binding wire. There is thus no danger of the insulation receiving damage from sharp corners, and every part can be thoroughly inspected during construction. Referring to the various types of compensating winding and commutating poles, the difficulties with the latter are fairly well known. Excellent results have been obtained, but they have their limitations where it is desired to work with a large range of voltage and where heavy overloads have to be carried. Also in many types elaborate adjustment of the commutating poles is necessary, and the saturation of the iron of the pole is a difficulty. In Messrs. Parsons' compensating winding no iron whatever is used between the poles, and therefore no saturation can come in. The machines have been run at half their voltage and at their full current, and as far as commutation

Mr. Law. goes the running is perfect; this, I think, would be very difficult to obtain with a commutating pole. Dr. Pohl's curve of ampere-conductors per centimetre of armature circumference is very interesting, and is very much in line with what is regarded as good practice. Under certain conditions, however, these results may be exceeded. Many designers use the same constant in the form of ampere-turns per inch of core diameter, which is, of course, the same thing. Messrs. Parsons are for the present building 2-pole dynamos and practically no 4-pole machines, except for comparatively low voltages where there is no danger of flashing over and where the currents to be collected are large. The 2-pole type as at present constructed gives entire immunity from flashing-over troubles. The speeds are a good deal in excess of those given in the paper. There are 500-k.w. machines running at speeds of from 2,700 to 3,000 revs. per minute, and 1,000-k.w. sets running at 1,500 revs. per minute; we also have 900-k.w. plants running at 1,800 revs. per minute. The author's remarks about homopolar machines are very interesting. About four years ago I had a good deal to do with one of these machines (a 50-k.w. machine driven by a motor). It gave 5 to 7 volts and 10,000 amperes, and the very heavy current was successfully collected by keeping water running on the copper rings. At the time the question of higher voltages from homopolar machines was gone into, and results practically identical with Dr. Pohl's were arrived at, but we found the design practically prohibitive on account of its cost, the amount of iron and of copper was enormous, and the speed to obtain high voltages extremely low.

Messrs. Parsons have, in some cases, gone rather higher than 75 metres per second, and in some cases lower. The plants that were supplied to the Manchester Corporation went as high as 85–90 metres per second, but generally the figure selected by Dr. Pohl—75 metres per second—is not exceeded. I should like to compliment Dr. Pohl on his ingenious solution of a difficult problem mentioned at the end of the paper. A method of increasing the number of commutator segments is a thing that has been thought of by many, and if the arrangement turns out a success it is likely to be largely adopted. I can, however, see difficulties, such as carrying the connectors through the shaft, since the resistance and self-induction must be kept the same as for the connectors at the other end. Perhaps this may be accomplished by keeping the resistance and, if possible, the self-induction of the ordinary connectors very high, so as to be able to keep the area of copper of the other connectors as small as possible, and in this way to balance their resistance and self-induction.

Mr. Churton. Mr. T. Harding Churton: I must say that the machines of the type described by the author which I have seen have not been as satisfactory as they appear to be on paper. The wear of the commutator is usually excessive, and the brushes appear to require a considerable amount of attention. I find, further, that in spite of the elaborate precautions taken the armature conductors are apt to fly out as

Mr. Churton.

well as the end connections and the connections to the commutators. There certainly appears to be considerable room for improvement in the mechanical construction of turbo-generators. With regard to Dr. Pohl's suggestion of making back connections and doubling the number of commutator sections, I do not see why this, if found satisfactory, should be restricted to turbo-machines. Could it not be advantageously applied in the case of slow-speed machines in which the output is limited by sparking? In this connection I shall be glad if Dr. Pohl can give us any information as to the relative cost of turbo-generators and ordinary slow-speed machines of the same output. I shall also be glad if Dr. Pohl will say whether he advocates the bipolar or the multipolar type for turbo-machines, and, in this connection, whether the cost of manufacture is affected in the same manner as in the case of slow-speed machines.

Mr. Smith.

Mr. S. H. SMITH: I should like to ask the author for a little more information regarding the back connections. It seems to me that these connections will only carry current at the time they are undergoing commutation, the tendency being for the current to flow along the conductors on the surface of the armature and to miss the wires through the core centre. It appears, therefore, that the current density in the centre wires can be kept high, but that the reactance voltage will be reduced very little unless particular trouble is taken to keep the wires in pairs which commutate at the same instant. In a 2-pole machine only two of the centre wires carry a current at any instant. I should also like to ask Dr. Pohl whether these machines have ever been constructed with an external revolving armature, the windings being arranged in slots, after the style of the ordinary induction motor, the centre shaft and field being stationary with the brush gear. The commutator would be built up on the inside of a cylindrical ring, the brushes collecting from the inside of the segments. By building the armature in this way and encasing the discs in a steel ring, it seems possible that centrifugal force difficulties may be considerably decreased both on the commutator and on the armature.

Mr. Mylan.

Mr. W. F. MYLAN: This problem of the improvement of turbo-generators appeals to me principally from a commercial point of view. Are the makers going to give us a bigger and better machine at a less cost, or if not, what other advantages can they give? In handling such apparatus as a salesman, the question often crops up as to the benefits the users are likely to get by purchasing a turbo-generator instead of an ordinary reciprocating engine set, and it is extremely difficult for us to bring forward sufficient proof that advantages are to be gained by their adoption. The troubles that we have at present with turbo-generators are so excessive as compared with the corresponding reciprocating set that one finds it hard thoroughly to convince buyers that it is worth while to put them in. It is essential, therefore, in dealing with the problem of the limitations of turbo-generators, not to forget that if there are to be variations in the design the cost must not be increased without increasing the output, so as to balance

Mr. Mylan. the result. There is one limitation which has been overlooked, and that is, in dealing with the collection of current from the commutator, the common practice has been to use metal brushes. One example has been mentioned to-night, where the wearing of the commutator was so excessive that it had to be replaced after comparatively short service. We must design a machine which does not require its commutator to be replaced in a short time, and one of the means for obtaining this result will be by the use of some material for the collectors such that the brush will be worn away instead of the commutator. With this end in view the radial-type commutator has been applied and machines have been made which, when tested, gave most satisfactory results. Carbon or similar material brushes were used, and the commutation was all that could be desired. In the radial commutator the current is collected not from a surface parallel to the shaft but from one at right angles to it. However perfect the balance of the machine may be, there must be a certain amount of vibration at right angles to the shaft. If the collection of current is taking place on the surface parallel to the shaft, this vibration will cause more or less sparking, and spoil the commutation and in due course the commutator. In the case of the radial commutator the motion due to the whip of the shaft being vertical does not interfere with the commutation, since it does not tend to cause jumping or clattering of the brushes. The movement, if there be any, parallel to the shaft, is small, of low periodicity, and such as to be readily followed by a good-fitting brush with comparatively weak springs. I must say that the commutation of machines fitted with this type of commutator has been excellent and quite equal to that of the modern low-speed engine type of generator. The fight between the designer of steam engines and those of electrical machinery is of old standing and still goes on. The steam designer, for reasons of economy in his steam consumption, prefers a comparatively high speed, whilst the electrical engineer, for electrical and mechanical reasons, prefers a low speed. Alternators for turbines have fixed speeds, and in this country sixty periods being the maximum periodicity usually met with, 3,600 revs. per minute represent the maximum speed, however small the size may be. The Westinghouse Company have built, and have in successful operation, sets of from 300 to 600 k.w. at this speed or very little less. We have built direct-current sets of 750 k.w. consisting of two machines in tandem running at a speed, I speak from memory, of about 2,500 revs. per minute. It is interesting to note that this speed seems to agree pretty well with the curve submitted by Dr. Pohl in his paper. I would like to confirm the remarks of Dr. Pohl and others who have spoken this evening regarding the homopolar machine and its future. About two years ago I was much interested in a small unipolar generator built as an exciter for a turbo-alternator. We had considerable trouble on test, but we thought eventually we had got over the difficulties. We put the machine into commercial service, but eventually, owing to the never-ending troubles, we took it out and substituted an ordinary

commutating machine. The results of this machine led us to conclude that neither commercially, on account of manufacturing cost, nor electrically, owing to small clearances and the difficulties of getting a sufficiently high voltage generated, is there a field for the homopolar generator for any service where an ordinary machine can be used. I am indeed very sceptical as to such machines ever being built on a large scale. Mr. Mylan.

Mr. J. WAGNER: Dr. Pohl referred first of all to the limit given by the temperature rise. I suppose most of us have seen turbo-generators, direct-current as well as alternating-current, where the output is very considerably limited in that respect, so that it is impossible to run them any longer even with a light load. With regard to the reason for flash-over, I quite agree that one cause may be excessive voltage between two neighbouring segments, but it is not the only reason for the flash-over, as it may be possible to work with a fairly high voltage without tendency to flash-over. Dr. Pohl mentions the quantity (AS) which he calls the ampere-conductors per centimetre of circumference. I thought this quantity had been introduced by Professor Arnold. It does not, however, seem right to me to put this quantity down as a characteristic constant for all sorts of machines. This quantity in turbo-generators varies from 150 conductors per centimetre of circumference up to 315. In slow-speed generators this quantity is very low, whilst in high-speed generators this quantity becomes very high. In a 220-k.w. generator running at 105 revs. per minute and giving 650 volts, the ampere-conductors per centimetre of circumference, for instance, amount to 110, whilst in a 75-H.P. motor at 500 volts with 450 revs. per minute, the AS are 210. All these machines were working satisfactorily, so I do not think it would be right to use the AS as a characteristic for continuous-current machines of different sizes. If it is right to introduce some characteristic it would be better to use ampere-conductors per square centimetre of surface armature, because this would take into account the length of the armature. Dr. Pohl has given us a new idea for overcoming the difficulty of high voltage between the segments. The device reminds one of the old Gramme ring machine, in so far as the conductors are brought into the inside of the ring and there are just as many conductors as segments. There is one point about which I have some doubt: the author says in the paper that the number of groups may be as large as convenient, if only care be taken to have in each group wires which are displaced by 180 electrical degrees. That will be perfect so long as there is no distortion of the armature. The geometrical position of these wires is fixed and cannot be altered when once determined. Whilst they may be arranged to be perfectly balanced at no load or at a certain given load, so as not to produce an external field, all this may be upset by a variation of the load, and the altered distortion of the armature field, due to armature reaction or its counter-acting forces, as compensating and commutating windings, may produce E.M.F.'s and equalising currents of an undesirable value. Mr. Wagner

Mr. Bailie.

Mr. J. D. BAILIE: With reference to troubles with metal brushes on turbo-generators, to which one speaker referred, I should like to say that in fifteen years' experience with metal brushes, the chief fault I have found is that they are not always kept sufficiently flexible. Flexibility is important. Frequently, also, the gauze surrounding the wire is not cut sufficiently far back, or the brush is not far enough through the holder. When I have come across cases of this description, I have simply beaten the brush against the nearest bench, cut back the gauze or adjusted the brush in the holder, as the case might be, and it has run satisfactorily. One speaker referred to wear on the commutator. We have had machines of 1,800 k.w. (two 900-k.w. dynamos in tandem) running for several years, and the commutators are in very good condition, the wear being exceedingly small. There need not necessarily be heavy wear on the commutator with metal brushes, and we have never yet replaced a commutator of a compensated-wound machine. Mr. Churton has referred to troubles with a Parsons turbo-generator in Leeds; I presume he referred to the one at the Corporation Tramways Station. I think his information must be wrong, as I have no knowledge of that armature being re-wound. I was rather interested in Dr. Pohl's suggestion that flashing over is due to the voltage per segment being too high. I would like to mention that I was at a colliery the other day where we have some plant installed. They have there a pair of circulating-pump motors, each of 50 B.H.P. 500 volts, and running at a speed of from 750 to 1,000 revs. per minute. Their commutators have the narrowest segments I have ever seen, something probably from $\frac{1}{8}$ to $\frac{3}{16}$ in. wide, and flashing over has repeatedly occurred. I do not know exactly how many segments there were, but the voltage per segment must be unusually low. The trouble in this case may perhaps be due to the difficulty, with such weak segments, in building the commutators sufficiently rigid, thus permitting movement of the segments; or the relation between the mica insulation and the segments may not be correct.

Mr. Chapman.

Mr. F. E. CHAPMAN: I quite agree with the author as regards acyclic machines. Experiments with such dynamos, direct coupled to De Laval turbines, have not up to the present been successful, mainly on account of the large brush losses. With respect to the weights of acyclic machines, a slow-speed one (750 revs. per minute) recently constructed by my firm to give an output of 64 k.w. (16,000 amperes at 4 volts) weighed about four times as much as the direct-coupled motor which drove it. The machine is of the single-disc type, but for the same diameter and peripheral speed the ratio of weight to output is about the same for this type as for the cylinder type referred to by Dr. Pohl.

Mr. Holiday.

Mr. R. HOLIDAY: I am extremely interested in a paper of this description, as I wish to see what possibility there is of improving these machines. One may infer from this that my experience with them still leaves some room for improvement, but having started with

a very early type I found there was a good deal of improvement needed, but the later types of turbines have been all that could be wished for. I can confirm what Mr. Bailie said as to metal brushes: the whole secret is to keep them flexible; if they are allowed to get stiff the commutator wears off as much as the brush. In looking at Dr. Pohl's proposal to get more segments into the commutator, one only wonders whether in doing this from a practical point of view we shall be able to retain the simplicity of the earlier turbines. In a colliery skilled men are not available, and any repairs which are necessary are done by the ordinary colliery workmen, and it is owing to this that extreme simplicity of the machine should be retained. The reliability of these machines in collieries is essential, as if anything happens all the men come out of the pit. Obviously it is impossible to stop for half an hour for any little repair, and consequently it is necessary to have a machine that can be relied upon. Mr. Holiday.

Dr. R. POHL (*in reply*): I shall have to confine myself to the main points only which were raised in the discussion. As regards the permissible voltage per segment, Mr. Stoney considers it quite safe to go as high as 49 volts per segment, whereas Mr. Miles Walker recommends 20 volts. I believe that 40 volts, as stated in the paper, is a safe figure, which for reasons of reliability I should not like to exceed. Mr. Stoney's remark that nothing like such high potential differences per segment could be found in "slow-speed" machinery is not correct, as in modern variable-speed motors the voltage per segment becomes, at the top speed, sometimes as high as in turbo-dynamos. I quite agree with Mr. Stoney that it is at present possible to construct machines for slightly higher speeds than those shown in Fig. 2 of the paper by employing higher values of the voltage per segment or the velocity, or by means of a compensating winding adjusted so as to reduce the field distortion below the figure assumed in the paper, and I know of such continental machines. I was surprised, however, at his disagreeing with my remark that it is not possible to construct direct-current turbo-generators above approximately 500 k.w. suitable to run at so high a speed as the equivalent turbine demands. This fact was admitted by various other speakers, and Mr. Stoney himself admits it in his concluding sentence on the struggle that has always been going on between the turbine people and the dynamo people. Dr. Pohl.

I do not favour 2-polar designs for various reasons, one of which is that unless we go to very high densities in the armature core the shaft diameter becomes too small, with the risk of getting too near the critical speed of the shaft.

As regards homopolar machines, I was glad to receive Mr. Stoney's support to my statements. The reason why, in my opinion, it is not possible with ordinary, not artificially, cooled slip-rings to work with A S values much above 30 is the excessive temperature rise and great wear of the rings, or else the very great increase of the armature length necessitated by the rings. Mr. Ellis referred to Mr. Noeggerath's statement that the weights of homopolar and of commutating machines

Dr. Pohl. do not differ to any great extent. I have worked through Mr. Noeggerath's design as far as it was possible from the published data, and believe that his machine agrees fairly well with the figures given in the paper, so that I think his statement as to relative weights must be erroneous.

I quite agree with both Mr. Fox and Mr. Miles Walker as to the superiority of carbon brushes, and am convinced that the employment of suitable means for keeping the commutator cool and for thus permitting the use of carbon brushes will make a machine far superior in regard to sparkless running under heavy overloads and with little attention.

Mr. Stoney is afraid that the employment of back connections, such as suggested in Fig. 5 of the paper, would mechanically not be a sound job. I do not share that opinion, because these connections are not arranged on the armature circumference but close to the shaft, and are, therefore, not subjected to those enormous centrifugal forces.

Professor Kapp referred to the difficulty of constructing auxiliary pole machines for tramway work. I have designed a large number of machines for tramway purposes, but do not remember to have ever experienced any commutation trouble with the same. I avoid, as a rule, the use of shunts in such cases, but should think that if an inductive shunt is used the ratio of reactance to resistance of the shunt should not be equal but greater than the same ratio for the auxiliary pole winding, as this would lead to the flux of the commutating poles following more rapidly the fluctuations of the load.

Mr. Stoney's compensating winding, to which Professor Kapp referred, certainly possesses some very good points, and we have every reason to congratulate Mr. Stoney on his success. At the same time, however, I think it is a very expensive method, and it is possible, I am convinced, to obtain equally good results with much cheaper and simpler means.

Mr. Fynn pointed out that for a winding with highly inductive back connections (Fig. 4 of the paper) the reactance voltage would not be twice but four times as high as in an ordinary winding, provided that, with the adoption of the auxiliary segments, the width of the brushes is reduced to one-half of their former value. That is quite right. I did not, however, assume any reduction of the width of brushes, there being no justification for such a reduction, and the statement in the paper is, therefore, also correct. As regards the damping effect of the copper tubes, a special external connection of the tubes will not be required, as the electrical connection between all the tubes is supplied by the armature stampings or by the spider. I do certainly not intend arranging the connections in a bipolar machine in a hollow shaft, and quite agree with Mr. Fynn that it would not be mechanical. I referred to such an arrangement merely for the purpose of a theoretical explanation. In practice I prefer multipolar designs.

Dr. Pohl.

Professor Thompson made some very interesting remarks on output limits, and I am much obliged to him for the way in which he commented on my introduction of a "flash-over limit." As to the arrangement for mechanically preventing flashing over to which he referred, I am not sure whether it would be really effective, because if a spark from one spindle on one side of the commutator should reach the opposite segment it will still form a direct electric connection between the two sets of brushes. I have, however, no practical experience with such machines. I quite agree with Professor Thompson as to the advisability of insulating by means of annular rings of insulating material the side faces of the shrink rings, and believe that it is possible to make a sound mechanical job of such rings.

Mr. Eborall referred to unequal expansion of different parts of the machine, due to unequal temperature rise, which would lead to the machine getting out of balance. This is certainly an important point which requires careful attention. As regards the Deri winding which Mr. Eborall advocated, my opinions on this subject have been expressed before, and, on the whole, are very similar to those which I just expressed in regard to Mr. Stoney's winding.

In reply to Professor Robertson's interesting snggestions, I consider the arrangement of two commutators connected to one winding hardly suitable for turbo work. The main drawback of such windings, in my experience, is unequal current distribution between the commutators, which, it seems, cannot for all loads be put right by adjusting the brush position. But apart from that, to prevent flashing over with an armature of twice the present length, or about 80 volts per segment, by employing thicker mica seems hardly feasible.

Dr. Kahn's modifications of the output formula are very lucid. In using the same it must be remembered, however, that the permissible reactance voltage is not constant for all sizes of machines. I quite agree with Mr. Catterson-Smith as to the advantages of employing higher gap densities. Unfortunately this is rarely possible in turbo-generators on account of the excessive core densities which it involves, though the introduction of the new alloy sheets tends to make such densities permissible.

In the discussion before the Leeds Local Section, a number of speakers have pointed out that the voltage per segment is not the only cause of the flashing over. That is exactly my opinion, which I expressed in the paper by saying, "It is, of course, a matter of common experience that bad commutation, particularly in connection with a dusty commutator, can by itself be the cause of flashing over, and it is further well known that the kind of winding employed and the velocity of load fluctuations, which result in momentary increases of pressure, materially influence the phenomenon. Whilst fully recognising the importance of these subsidiary causes, it must be admitted that the maximum voltage per segment is the most important factor, which, for satisfactory performance, should not exceed a certain value." I do not

Dr. Pohl. think I could have made it much clearer. Mr. Hartnell referred to a number of further limits. The heating limit was mentioned, which was also referred to by a number of subsequent speakers. The heating limit is certainly an output limit for a finished machine with definite ventilating methods. In designing a machine, however, one is always able to provide cooling devices so effective as to push the temperature limit beyond the electrical output limits. For the considerations with which I am dealing in this paper I am justified in assuming that the temperature limit need not be considered as an output limit. I think Mr. Law agreed with me on this point. In machines with a surface winding there are always strong eddy currents on account of the very high periodicity, and perhaps it is for this reason that Messrs. Parsons prefer to make their machines of the bipolar type. Although the periodicity is thereby kept comparatively small, I do not think that the 2-pole machine will survive, if I may be allowed to express my candid opinion. The same reasons which in the design of slow-speed machines have led to the universal adoption of multipolar designs hold good for high-speed generators, and will, I believe, lead to the same result. I notice that Continental manufacturers are already adopting multipolar machines; for instance, a 1,000-k.w. machine running at 1,250 revs. per minute is designed with 6 poles, and a low-voltage machine for a similar output with 8 poles. The 2-pole machine, besides being more costly to build, has this disadvantage. The core must be considerably larger from the circumference to the internal diameter because the flux must be accommodated; consequently the space available for the shaft becomes small, and there is the risk of getting too near the critical speed of the shaft. That point is of importance, although I am aware that Messrs. Parsons have overcome the difficulty. I designed some time ago an 80-k.w. machine for 4,000 revs. per minute, and I endeavoured to make it a 2-pole machine, but finally adopted the 4-pole design as the superior one, and it has proved to be correct. Mr. Hartnell referred to the limitation of the armature length due to the reactance voltage becoming too high. I have tried to prove in the paper that the length of the armature is primarily limited by the number of lines of force per centimetre of circumference or by the voltage per segment, and having settled the length upon that basis I then proceeded to determine the permissible number of ampere-conductors per centimetre so as to prevent sparking.

I admit that the length of the armature is proportional to the reactance voltage, but the principal limitation for the length of the armature is, in my opinion, the flash-over limit. I have settled the armature length on that basis, and having made that limitation I now proceed to the A S, which is fixed with due regard to the existing length, the influence of which on the sparking I have thus taken into account. Mr. Law made a number of interesting remarks about Messrs. Parsons' machines, and mentioned that Mr. Parsons invented the compensating winding in 1885. If I remember rightly, the first patent describing a compensating winding is Menges'

of 1884, and the device was a few years later simultaneously reinvented by Ryan and by Fischer-Hinnen. Mr. Law also referred to a machine in which it was found that flashing over did not occur with a potential difference as high as 65 volts per segment. There is, however, a very great difference between a machine running at constant load and under test conditions and a turbo-generator running in a central station without continual attention. I believe the voltage of 40 volts, which I have adopted as the highest permissible, is a sound figure, and personally I do not like to exceed it. I dislike the idea of preventing flash-over by increasing the thickness of mica because it tends to weaken the commutator mechanically, shortens the time of commutation, and proportionately increases the reactance voltage. I say in the paper that if it were found possible by some mechanical means to prevent flash-over the result would simply be that it is possible to increase the number of lines of force by increasing the length of the armature, but that by so doing one would increase the reactance voltage and so be compelled to reduce the current to prevent sparking. A mechanical method for preventing flashing over does not help one very much, as what is gained on the one hand is lost on the other. Mr. Hartnell, in speaking about the winding which I proposed, thought there was a good deal of difficulty in accommodating the connections near the shaft. I can assure Mr. Hartnell that there is no difficulty whatever in this respect, for the simple reason which another speaker pointed out, that these connections only carry current during the time the respective segments are short-circuited by the brush; consequently they may be made very much thinner than the ordinary armature conductors without fear of undue heating.

I do not think it is necessary in practice to put extra resistances into the ordinary commutator lugs. I mentioned that it is advisable to keep the resistance of the back connections about equal to that of the ordinary commutator lugs, but when we consider the value of the reactance voltage and compare with that high voltage the extremely small voltage drop in the commutator lugs, then we will agree that 0·1 or 0·2 volts will hardly make any appreciable difference. The accommodation of the connectors near the shaft is mechanically a perfectly sound job. The arrangement of the connections at the back of the armature is the weakest point mechanically, but the difficulties which arise can also be quite easily overcome. I hope to be able in the course of a few months to give experimental data of a machine of this kind which the Phœnix Dynamo Company are now constructing. I did not in the least think of bringing this winding forward as the only possible or even as a definite solution. I simply wanted to point out clearly that considerable progress can only be accomplished by simultaneously reducing the reactance voltage and the voltage per segment.

Mr. Smith asked whether it was possible to construct a generator with an armature revolving outside the internal field. It may be theoretically possible, but I doubt very much whether it is so prac-

Dr. Pohl. tically. The armature diameter being limited by the centrifugal force, we are compelled to accommodate the magnets inside the armature in a space which is by far too small for the magnetic system. One speaker asked a question as to the commercial advantage in using turbo-generators. I think I have pointed out that the turbine set is only commercially superior to the reciprocating engine set if the speed is sufficiently high. At the present time I believe that 750 k.w. forms the limit; and from this limit the turbine set becomes equal, or superior, to the reciprocating set. If we succeed in building 500-k.w. sets satisfactorily for much higher speeds, that limit will come down and the 500-k.w. turbo will then be superior commercially and in performance to the 500-k.w. engine set. I was very much interested and pleased to hear that so many gentlemen agreed with me regarding homopolar machines being quite hopeless. The patent records show that a great number of people are working at homopolar machines, and I am inclined to believe that they are wasting their time. Mr. Wagner mentioned that in his opinion the specific electric loading or the A S value is not a characteristic constant for machines. Of course I do not at all put forward that this A S should be the same for all kinds of machines. Such "constants" are real constants for certain sizes of machines only, and the designer must know which value to employ in a given case. Mr. Wagner further contended that with the winding illustrated on page 251 a neutralising effect of the connectors would only be obtained for a certain load, but not for all loads. I do not think he is right in that contention, and it is easy to prove that the neutralising action is theoretically perfect at all loads. I quite admit, however, that unequal current distribution will, to a certain extent, upset the neutralising action, and I have referred to this point in the paper on page 251.

Mr. Law pointed out that the compensating winding used in the Parsons' generators has the advantage that no iron core is used for the commutating flux, and therefore there can be no saturation. While this is a decided advantage, the very large ratio of ampere-turns for compensation to ampere-turns of the armature which is thereby necessitated must not be disregarded. That figure is very much higher than the figure for machines with salient auxiliary poles. At the same time it must be remembered that the weight of copper for the compensating winding as compared with the weights of armature copper is much larger than the above ratio would indicate, on account of the greater length per turn and the lower permissible density. I think I have replied to the question as to whether multipolar designs are cheaper than 2-pole designs, and my opinion is that they are decidedly cheaper. As to the relative cost of slow-speed and high-speed machines, I cannot give exact figures, but I should think that a 1,000-k.w. generator running at 200 revs. per minute, which would be approximately the speed of the reciprocating engine, would cost about £1,000, whereas a generator for 1,000 k.w. at 1,250 revs. per minute would cost about £1,700. Of course ther is a considerable saving in the cost of the

prime mover, in floor space, steam and oil consumption by adopting the steam turbine, and it becomes cheaper for higher speeds. It was mentioned by Mr. Hartnell that by employing compensating windings it is possible to prevent the field distortion altogether, and that 25 per cent. more output can be obtained because the factor $\frac{B_{g\,max.}}{B_g}$ would become 1 instead of 1·25. That is quite right theoretically, but in practice one cannot be sure of preventing distortion altogether. For instance, with an over-compensating winding as employed by Messrs. Parsons, or of the Deri type, there is an excess of ampere-turns for compensation over and above the ampere-turns on the armature lying beneath the former, and consequently there is a field distortion. Even if the compensating ampere-turns are adjusted as nearly as possible, so as to obtain an undistorted or straight-line field, there may still be temporary fluctuations of the field density at certain points due to the varying relative position of the armature slots and the field slots. Dr. Pohl.

I know for a fact that Deri machines had, a few years ago, a very considerable field distortion, and that some firms using that kind of winding now employ a method for adjusting the compensating winding on the main poles accurately, so as to prevent distortion as completely as possible, and to give at the same time the proper excitation to the commutating poles. I believe that Messrs. Parsons distribute the number of turns equally all round, and the result is that there is also a field distortion, though it will be small on account of the large air-gap employed with surface-wound armatures.

Votes of thanks to the author for his interesting paper were unanimously accorded by the members at both meetings before which it was read.

Proceedings of the Four Hundred and Sixty-fifth Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, December 19, 1907—Mr. CHARLES P. SPARKS, Vice-President, in the chair.

The CHAIRMAN: Gentlemen, immediately on the receipt of the news of the death of Lord Kelvin, our President, on December 17th, the Council decided that all ordinary business, of which the usual notice has been given, must be postponed, and that instead thereof this meeting should be asked to place on record an expression of its sense of the great loss we have sustained. Through the death of our President we must all feel we have not only lost a friend, but the whole world is the poorer through the loss of his genius, which has been unexcelled by that of any scientific worker.

I now have to propose that the following resolution be sent to Lady Kelvin :—

"The Council and the members of the Institution of Electrical Engineers desire to convey to your Ladyship an expression of their great sorrow at the death of your illustrious husband, their revered President and Honorary Member, and to assure you of their deep sympathy with you in your sad bereavement."

All present rose in their places, and the resolution was carried in silence.

The CHAIRMAN: I have also to announce that telegrams of condolence with the Institution on the death of Lord Kelvin have been received from abroad, which, with the permission of the meeting, I will read :—

"To the Institution of Electrical Engineers, London. Deeply affected by announcement of Lord Kelvin's death, we partake of grief of colleagues of England and of whole world who honoured him as the

greatest genius of contemporary physics, and who admired his great qualities of mind and heart. We beg you to represent our Association at the funeral of our only Foreign Honorary Member.

"JONA, President,
"Associazione Elettrotecnica Italiana."

"To the Institution of Electrical Engineers, London. Kindly accept profound sympathy in passing of your President, the world's greatest scientist. Parshall representative at funeral.
"American Institute of Electrical Engineers."

"For Institution of Electrical Engineers, London. Capetown Local Section laments death of our honoured President.
"TAIT,
"Capetown."

The meeting was adjourned at 8.6 p.m.

The Local Sections of the Institution in the United Kingdom also passed resolutions of condolence on the death of Lord Kelvin at their next meetings, copies of which were sent to Lady Kelvin.

Proceedings of the Four Hundred and Sixty-sixth Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, January 9, 1908—Mr. CHARLES P. SPARKS, Vice-President, in the chair.

The CHAIRMAN announced that the Council, at their meeting just concluded, had unanimously elected Colonel R. E. Crompton, C.B., Past-President, as President of the Institution for the remainder of the Session in succession to Lord Kelvin, deceased.

Mr. Sparks then vacated the chair, which was taken by Colonel Crompton.

The PRESIDENT (Colonel R. E. Crompton, C.B.): It is with mixed feelings that I stand before you to-night. An hour ago nothing was further from my thoughts than that I should become your President. I cannot tell you how deeply I feel the honour conferred upon me by your Council in asking me to succeed the greatest man in our world, nor how sad I am that I owe my position to the world's great loss.

We have been so long accustomed to seeing Lord Kelvin moving amongst us, helping us and advising on our most difficult problems, that I think we only now are beginning to realise how much his unquestionable leadership of the world of electrical science has aided and helped all of us, his fellow-countrymen. I think that those who witnessed the impressive ceremony at his funeral in Westminster Abbey must have realised very strongly that by Lord Kelvin's death we have suffered an irreplaceable loss.

Personally I feel it, as I have been closely associated with him in the corps of Electrical Engineers and as Honorary Secretary of the International Electro-Technical Commission, of which he was the President. By his death the Presidentship of this Commission will no longer remain in our country.

The extremely short notice at which I have been called upon to occupy my new position must be my apology for the inadequacy of what I can now say, but in any case it is difficult to find words fitting to express what we all feel by the death of our President and great electrical engineer.

The minutes of the Ordinary General Meetings held on December 5 and December 19, 1907, were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members :—

W. J. Benton.
H. H. Berry.
A. J. Bloemendal.
H. G. Brown.
A. C. Cramb.

From the class of Associates to that of Members :—

Thos. H. Churton.

From the class of Associates to that of Associate Members :—

Andrew M. Niven.
N. R. Temperley.

From the class of Students to that of Associate Members :—

S. F. Barclay.
Albert Blanks.
O. H. Browne.
P. J. R. Fraser.
Geo. J. Bish.
L. S. Challis.
W. B. Cole.
C. B. Grace.

Donations to the *Library* were announced as having been received since the last meeting from L. A. Bauer, H. Borns, Messrs. A. Constable & Co., Ltd., Messrs. Gauthier-Villars, Professor A. Hay, P. Hunter-Brown, The Institution of Civil Engineers, A. H. Jackson, A. E. Kennelly, W. Maurice, W. W. Melville ; to the *Building Fund* from Major P. Cardew, H. C. Channon, A. A. Crawford, W. Duddell, M. M. Gillespie, S. E. Glendenning, R. Hardy, H. E. Harrison, Professor A. Hay, J. F. Henderson, D. Henriques, Sir H. B. Jackson, J. T. Morris; R. O. Ritchie, J. H. Rosenthal, T. C. T. Walrond ; and to the *Benevolent Fund* from Ivon Braby, H. W. Bowden, A. C. Brown, Major P. Cardew, J. Devonshire, W. Duddell, W. B. Esson, E. Fawssett, M. M. Gillespie, H. E. Harrison, A. P. Haslam, Messrs. Hawtayne & Zeden, A. J. Hersant, S. H. Holden, Sir H. B. Jackson, Rev. F. J. Jervis-Smith, P. V. Luke, C. H. Merz, G. H. Nisbett, W. H. Patchell, Sir W. H. Preece, W. L. Preece, S. G. C. Russell, Sir D. Salomons, F. Smith, A. A. C. Swinton, R. R. Todd, J. Toulmin, T. C. T. Walrond, H. M. L. Ward, C. H. Wordingham, to whom the thanks of the meeting were duly accorded.

The following paper was read and discussed and the meeting adjourned at 9.25 p.m.

COST OF ELECTRICAL POWER FOR INDUSTRIAL PURPOSES.

By JOHN F. C. SNELL, Member.

(*Paper received October* 11, 1907, *read in London on January* 9, *in Dublin on January* 9, *in Glasgow on January* 14, *and in Sheffield on February* 17, 1908.)

The fundamental elements and details of design of large power stations and transmission systems are now so generally accepted that there appears to be but little to add to the available information; but the author thinks there is yet a good deal to be discussed in connection with the economics of electricity supply, more especially now that a large demand has arisen for cheap supplies of power. Indeed, it may be said that electrical supply—whether under municipal or company administration—appears to-day to have reached a critical point. There is no disguising the competition of the gas industry, which has been thoroughly awakened by its newer rival, with consequent improvements by gas engineers both in its utilisation for lighting and also for power. This has, for the time being, checked the hitherto steady growth of electric lighting, and, together with other sources of power, has become a serious competitor with electrical power.

It will be wise to recognise this present position of electrical supply, and thus to enable us to develop undertakings along sound engineering and financial lines. To do this we must take a dispassionate view of the limitations of badly situated stations, and also of those power companies which require a heavy expenditure on transmission and distribution.

It is proposed to compare the costs of supply from town installations and power companies, and their relation to the cost of production from plant of various kinds installed in factories. For instance, if a manufacturer can instal plant wherewith he can obtain power at a cheaper rate than he can be supplied from an outside source, surely it is in the ultimate real interest of our industry to say so. Or again, if any town can obtain a supply from a power company more cheaply than it can hope to produce from its local station, then again it is in the interest of that town that the supply should be taken from without. Or if a number of suppliers can benefit their collective consumers by combination, then the Legislature should remove the present obstacles to such co-operation in the manner contemplated by the Electricity Supply Bill, recently promoted by the Board of Trade, which has, unfortunately, not yet passed into law.

The author suggests that the following economical axioms apply to electrical supply; these are the result of experience, and will be admitted by members who have had any extensive experience of supply from stations, and who also have investigated the economics of local plant :—

1. There is generally a critical load factor and size of plant within which no outside supply can hope to compete with a user's local plant ; that is, at and beyond this critical point a user can produce more cheaply from his own plant.
2. There is a real limit to the economical radius of distribution from any power station—whatever the pressure of transmission, or whether by underground cables or overhead lines. This is in the abstract, of course, self-evident ; but the author means that this radius is much less than is usually advanced by power company promoters.
3. Minimum outlay of capital is indispensable in power supply, and can only be obtained in the power house by the centralisation of large units. But this can be neutralised in any particular case, and the capital per kilowatt raised beyond the critical value by attempting transmission over too great a distance.
4. Successful power supply depends upon—
   (*a*) The geographical positions of the user and supplier, because the capital cost of the supplier's plant, plus transmission and transformation, may exceed the capital cost of the user's own plant; and
   (*b*) The resultant diversity factor arising from multiplicity of industries. Arising from this latter point, great care has to be exercised in the districts where the industries are all practically of one nature, for the diversity factor will be almost nil in such cases.

The competition of the older established rival, gas, must be met by improvements in, and the greater economy of, metallic filaments, or other new types of lamp rather than by the cheapening of the cost of production.

The effect of power supply superadded to lighting supply must of necessity reduce the general cost of production, because of the greater resultant diversity factor and consequent improved load factor, and in this country probably 2·0d. per unit may eventually be reached, as an average cost of lighting units. The price of lighting must always be considerably in excess of average power prices, both because of its lower load factor and also of the greater cost of low-tension distribution systems and requisite service connections. This reduction of price, however, cannot be compared with the importance of a probable reduction of energy per candle-power from 4 watts to, say, 1·5 watts.

No doubt this present competition is really healthy for the electrical industry, if hard upon the present workers; for from the struggle for existence will spring improvements which will result in the survival of the fittest, which for lighting and power—bias apart—must prove to be electricity rather than gas or other systems, if only from its inherent better characteristics affecting health, cleanliness, safety, and adaptability.

It is to power supply, then—after lamp manufacturers have evolved their best—that suppliers must look for increased output and steady growth. The author does not mean by this that power should be supplied at rash speculative prices—power supplied at any price, as it were—or at less than cost price, as so many electrical undertakers, particularly in London, are now doing, and some of them to their sorrow. What is meant is that power should be obtained as a result of a rational system of charges based upon an economically designed power station and distributing system, together with a sound foresight into the effects of such an added load upon any undertaking. Unfortunately so many of the present smaller systems have so high an expenditure per kilowatt installed that such reasonable charges cannot be made by them, except at the cost of other more profitable consumers. It is to show some remedies for this difficulty that this paper is attempted.

It will be well first to examine what are the economical possibilities from other sources of power, such as town gas, suction or producer gas plants, oil engines, and small local steam-driven electrical plants, and then to see what order of prices must be reached by electrical suppliers to enable electricity to compete successfully with the above-mentioned plants, and at what load factor or under what conditions the one will be generally more economical than the other. Such a comparison can only be made in general figures; but those given in this paper are based upon general rates for coal or gas, and average capital costs of plant. The author will then endeavour to apply these figures to various trades and industries, many of which are based largely upon his own experience. He then will give statistics of works of different kinds, which may be useful for reference, and will then set out the charges which must be adopted in order to compete successfully with these other sources of power. Finally, the probable chances of existing undertakers in London and the provinces meeting this competition successfully will be discussed, and also what steps must be taken by them to supply this demand for power at marketable charges while maintaining their systems on a thoroughly sound financial basis.

SECTION I.

Table I. sets forth the cost at various load factors of gas engines supplied with town gas, suction gas engines, oil engines, and small local steam plants of from 20 to 100 H.P. with generators and auxiliaries. These figures have been obtained from actual users, and the author has

taken great pains to ensure that the figures are reliable. On the one hand they are not merely test figures, which afford no real criteria of the commercial working of such plants; nor, on the other, are they figures of obsolete engines, which would be unfair to take as a basis.

TABLE I.

Ascertained Cost of Power per Unit Generated for Independent Installations up to 100 H.P.

Annual Load Factor.	Equivalent Hours per Annum.	Town Gas at 2s. per 1,000 cub. ft.	Suction or Producer Gas.	Oil.	Steam.	Average Costs.
Per cent.		d.	d.	d.	d.	d.
10	876	1·423	1·520	1·286	1·322	1·388
15	1,314	1·210	1·270	0·972	1·031	1·121
20	1,752	1·090	1·040	0·813	0·876	0·954
25	2,190	1·007	0·904	0·715	0·806	0·879
30	2,628	0·944	0·812	0·650	0·705	0·778
35	3,066	0·892	0·744	0·600	0·646	0·720
40	3,504	0·844	0·694	0·563	0·598	0·675
50	4,380	0·791	0·625	0·511	0·542	0·617
60	5,256	0·745	0·576	0·475	0·498	0·574
70	6,132	0·706	0·540	0·448	0·460	0·538
80	7,008	0·670	0·514	0·426	0·429	0·500

The author has taken town gas at an average price of 2s. per 1,000 cub. ft.; and in the case of suction or producer gas engines, anthracite coal at 22s. per ton, or small peas at 10s. per ton; and oil at 42s. 6d. per ton. In the figures water is taken at an average price of 6d. per 1,000 gallons; repairs and lubricating oils are taken from actual figures; and depreciation and interest on capital outlay are taken throughout at 10 per cent. These are the real figures with which manufacturers debit themselves, although it is fair to point out that in nearly every case there is no proportionate charge for supervision or general establishment charges, or for rating nor any spare plant. Attendance is frequently not debited to the cost of power, and, therefore, on all points the figures given are decidedly fair to these sources of power, and are even somewhat unfair to electricity supply.

Table II. sets out comparative figures for larger and less frequent installations of 100 to 500 H.P.

In the final column of each of the two tables the average results of these figures are given, with which comparisons are made later with the electrical schedule of charges.

The author sets out below actual statistics of independent tests taken from different industrial plants, and it is interesting to compare the actual figures of these several cases with those in Tables I. and II., when it will be found that the figures in these tables are, if anything, on the low side.

TABLE II.

Ascertained Cost of Power per Unit Generated for Independent Plant from 100 to 500 H.P. Installed.

Annual Load Factor.	Equivalent Hours per Annum.	Suction or Producer Gas.	Oil.	Steam.	Average Costs.
Per cent.		d.	d.	d.	d.
10	876	1·273	1·007	1·086	1·122
15	1,314	0·944	0·778	0·811	0·844
20	1,752	0·760	0·656	0·673	0·696
25	2,190	0·670	0·584	0·590	0·615
30	2,628	0·596	0·536	0·534	0·555
35	3,066	0·540	0·498	0·494	0·511
40	3,504	0·502	0·469	0·464	0·478
50	4,380	0·447	0·430	0·421	0·433
60	5,256	0·406	0·404	0·393	0·401
70	6,132	0·377	0·382	0·373	0·377
80	7,008	0·358	0·366	0·357	0·360

SECTION II.

Some actual data of different industries are now given, most of which were taken by the author himself through the courtesy of the directors and officials of the various works, and for the electrification of many of which he has either been responsible or is officially connected with.

Graving Docks.—As might be expected, some priority is given to shipyards, engine works, and docks, being those with which the author has been more intimately connected for so many years.

Take first the cost of an electrically driven pontoon, which is capable of berthing vessels up to nearly 4,000 tons displacement. The pumps are four in number and are electrically driven centrifugal pumps, each 18 in. diameter.

The following are official figures obtained when actually docking a vessel of 2,600 tons :—

Tons of water pumped	3,875.
Maximum head on pumps	27 ft.
Average head on pumps	16 ft.
Time taken to empty	25 minutes.
Cost of power supply (outside source) ...	1·375d. per unit.
Number of units consumed	123.

The actual total cost per ton of vessel docked was :—

Power	0·064d.
Wages, etc.	0·100d.
Interest and depreciation at 10 per cent...	0·159d.
	0·323d.

For comparison take another case, namely, that of a graving dock driven by two gas engines, 40 H.P. each, each driving horizontal centrifugal pumps, each 21 in. diameter, with a capacity of 9,320 gallons per minute, with a maximum head of 28 ft. and an average head of 17 ft. The following are the data when docking a vessel of 1,760 tons :—

Tons of water emptied	10,632.
Time taken to empty	128 minutes.
Cost of gas per 1,000 cub. ft.	1s. 6d.
Gas consumption	4,600 cub. ft.

The actual cost per ton of vessel docked was :—

Power	0·046d.
Wages, etc.	0·412d.
Interest and depreciation at 10 per cent. ...	0·587d.
	1·045d.

It will be observed in this comparison that the graving dock represented a much longer run and a better load factor, and yet the cost of docking by gas-driven plant was three times that of electrically driven plant. It is fair to say, however, that the electrically driven plant is of recent design, whereas the gas plant cannot be said to be modern.

Supply to docks will not, however, represent an important item, as they must necessarily be limited in number. Electrical suppliers will do well to adopt a method suggested by the author, which was to stipulate that during "peak" loads in the restricted winter months, only one-half of the pumps should be worked, the docking on these occasions, therefore, taking approximately twice the usual time. It is surprising, however, how few are the occasions when this restriction has to be enforced.

In the above-cited gas installation, the price being so low per 1,000 cub. ft., it is improbable that any great economy would have been gained from an independent suction or gas-producer plant.

Dock installations require an extensive system of jib and other cranes, capstans and other auxiliaries. These, however, are so demonstrably better than hydraulic cranes and capstans, both in speed of working and annual cost, that it is unnecessary to dwell upon them. The author has been associated with an extensive application of 3-phase power to travelling jib cranes and capstans, which have been both successful and economical. It is found that plant of this description has a very low annual load factor, consumes but few units, and brings in a very small revenue. Care has, therefore, to be exercised in fixing prices for such a supply.

Shipyards.—We are enabled to compare the actual costs of gas-driven and steam-driven local plant of two neighbouring shipyards. Both the yards were electrically equipped, the local generating plant

in the one case being driven by gas engines supplied from the town mains and in the other by modern steam engines.

In the gas-driven plant the following are the data given by the authorities themselves, and carefully checked by the author:—

Duration of test, 4 months; ordinary working conditions.
Number of gas engines, 3.
Total B.H.P., 360.
Total kilowatts, 225.
Belt-driven shunt-wound dynamos, 230 volts.
Maximum load recorded on switchboard, 207 k.w.
Total units generated, 83,125.
Load factor, 14 per cent.
Cost of gas, 1s. 6d. per 1,000 cub. ft.

The cost of power actually ascertained (but excluding any item of supervision, general establishment charges, or rating of machinery) was as follows:—

	£.	s.	d.
Gas, 3,000,828 cub. ft.	225	0	10
Engine and dynamo oils	24	1	7
Water at 6d. per 1,000 gallons	2	0	0
Waste and stores	5	9	10
Repairs (material)	5	16	8
Attendance and repairs (labour)	84	12	1
Interest and depreciation at 10 per cent. ...	60	0	0
Total cost for 4 months	£407	1	0

representing a total cost per unit of 1·173d. It is only fair to point out that the item "attendance and repairs (labour)" is an excessive one, but the figures are, nevertheless, a statement of fact. The plant was under the care of engineers and an engineering firm, and this high cost can only be ascribed to the fact that the plant was not so well maintained as it should have been.

Attention is also called to the large gas consumption, which amounted to 36 cub. ft. per kilowatt-hour, a large figure, even on so low an annual load factor as 14 per cent. The plant load factor was 48 per cent.

For comparison the following particulars may be taken of an independent steam-driven plant in a combined shipyard and engine works:—

Duration of test, 1 year; ordinary working conditions.
Four triple expansion condensing steam engines, high speed, enclosed, each 250 H.P. Total, 1,000 H.P., 672 k.w.
Cooling towers.

Boiler pressure, 160 lbs. No superheat. Marine boilers, close to engines.
Maximum load observed, 504 k.w.
Total units metered at switchboard, 891,655.
Load-factor, 22 per cent.
Class of coal used, small at 11s. per ton.
Lbs. of coal per unit generated, 7·3.

The following was the ascertained cost of generation :—

	£	s.	d.
Labour	665	0	0
Coal, 2,924 tons at 11s.	1,608	11	11
Feed water	59	0	6
Circulating water and make-up	31	19	6
Lubricating oils	58	8	5
Repairs and general stores	141	2	10
Interest and depreciation at 10 per cent. ...	1,042	6	4
	£3,606	9	6

representing a total of 0·97d. per unit generated.

It may be observed that a shilling per ton rise or fall in the price of coal per ton would have raised or lowered the price per unit by 0·039d., or 4 per cent. The condensing appliances were somewhat faulty, and better figures would no doubt have been obtained with an improved vacuum, and with superheat.

In another case of an engine works driven by modern enclosed engines, representing a total of 1,000 H.P. with a load factor of 24 per cent., and where there were both superheat and a good vacuum, the author obtained the total figure of 0·71d. per unit metered at the switchboard.

These cases are actual records.

It may be said that improvements could be made by users if they evinced greater care ; as in the above case 0·97 per unit generated, even with small coal, is a high figure, and could be reduced on test. The author's experience, however, leads him to think that whatever may be the results obtained on test—whether with gas or oil engines or steam—the consumers do not give that attention to the upkeep of their plant which might be expected, and the figures quoted are really reasonable average figures obtained in practice, and this is an argument in favour of generating plant being in expert hands in a centralised power station.

In a paper * read before this Institution some time since, Mr. Williamson gave the following interesting particulars of various stations installed at large works :—

* *Journal of the Institution of Electrical Engineers*, p. 925, vol. 32, 1903.

Plant Installed.	Capital Cost per Kilowatt.	Generating Cost per Unit.	Load Factor.
Kilowatts.	£.	d.	Per Cent.
375	26·25	—	—
600	22·50	—	—
640	20·50	0·716	37
750	24·20	—	—
1,325	25·80	0·675	—
2,000	23·50	0·970	—

Paper Mills.—The following are particulars of a paper mill which came under the author's notice. These figures could undoubtedly have been improved upon for the reason that some of the boilers were low pressure (80 lbs.), and could have been replaced with advantage by high-pressure boilers with resulting economy. On the other hand, this paper mill is a successful one, and typical, the author believes, of very many throughout the country which offer a field to electrical enterprise:—

Duration of test, one year; ordinary working conditions.
Number of engines, 25. Total B.H.P., 1,820.
Total coal consumed for power driving, 17,680 tons.
Average horse-power, 1,760.
Running hours per annum, 7,000.
Total H.P.-hours, 12,320,000.
Annual load factor, 79 per cent.

The cost of power was as follows:—

	£
Coal	9,724
Oils and stores	156
Wages	1,524
Repairs	700
Interest and depreciation at 10 per cent.	2,730
	£14,834

representing 0·29d. per H.P.-hour, or an equivalent of 0·41d. per unit generated.

In works of this class, however, it must not be forgotten that boilers have to be installed in any case, and much coal used, for boiling the esparto grass, heating and drying the pulp, and heating the calenders.

In this case, out of a total of 26,000 tons used per annum, no less than 8,320 tons were used for heating and drying purposes, the balance being required for power.

The works in question have in addition to the grass boilers, washers, and calendering machines, all of which require live steam, paper-making machinery, half-strip machines for straining, beaters, cutting and rolling machines. There are thus a number of machines which are usually at some distance from the main drive. These auxiliaries, which represent from 10 to 15 per cent. of the total power, and are usually from 10 to 30 H.P. each, could certainly be driven more economically by electrical means, either from generators driven from the main mill engines, or supplied independently from an outside source.

Works of this class have so high an annual load factor, and, in the author's opinion, are of such a size and nature that it becomes a difficult problem for a central power supply (unless of very large dimensions) to compete successfully with the manufacturer's own plant, more especially as the very nature of this day-and-night load does not permit of any improvement of the general diversity factor at the power station.

The author calculated that this consumer could have reduced the annual cost by 20 per cent. by installing high-pressure boilers, and by substituting motors for the wasteful steam engines situated at the outskirts of the mill. This would have reduced the cost of the manufacturer's own power from his own plant to an equivalent of 0·33d. per unit.

Reference to the schedule given later in this paper (Table XIV.) shows that with an 80 per cent. load factor and transformed 3-phase current a large power supply could hope to compete successfully with this low figure, but this can obviously only apply to very large power stations, and to mills situated at a reasonable distance only from such stations, and thus not involving a large special expenditure on cables.

Jute Mills and Cotton Mills.—A jute mill is another difficult class of factory for the successful competition of electricity, and is very similar to cotton and other textile mills. The running hours are usually about 2,750 per annum, and the load is practically constant. This represents an annual load factor of 32 per cent., but a plant load factor of 92 per cent.—thus the cost of power is exceedingly low.

In an actual modern mill having a total horse-power of 900 the annual costs were as follows :—

	£
Coal	1,444
Water	124
Wages and repairs	720
Interest and depreciation at 10 per cent.	1,350
	£3,638

which is equivalent to 0·37d. per H.P.-hour. On an annual output of 2,284,500 H.P.-hours this is equivalent to a price of 0·48d. per unit.

In this class of mill the main engines represent about 83 per cent. of the total power installed, the remaining 17 per cent. being made up of auxiliaries scattered about the place, which it would certainly repay to equip with motors, not only because of their isolation from the main

drive, but also because the auxiliaries generally require to run for longer periods than the main engines in order to keep pace with the output from the spindles, spinning frames, and general machinery. It is interesting to know that in a mill of this description the shafting losses represent 25 per cent. of the total power. Regularity of speed is absolutely essential, and a 3-phase motor is the most beautiful application possible, not only giving regular speed, but also freedom from fire risk, which is important in this class of works.

Mr. Wilson,* in 1905, quoted some figures of a new mill engine, whose full-load diagram was 738 I.H.P., and the light-load friction diagram 300 I.H.P., showing an apparent efficiency of only 59 per cent. In such a case there must obviously be a good opportunity for electrical driving. He went on to say that the total cost per I.H.P.-year comes out to something of the order of £2 5s. to £2 10s. for large mills, *i.e.*, equivalent to £3 to £3 7s. per kilowatt-year, and in the case of small mills from £3 7s. to £4 7s. per kilowatt-year. Although he does not give the figure, this really means that, based upon the usual running hours of 2,700 per annum, the cost per unit ranges from 0·257d. to 0·287d. for large mills, and from 0·287d. to 0·373d. for smaller mills. These figures appear to the author to be too low. It is obvious, therefore, that only by the greater efficiency of electrical driving, *i.e.*, by competing with the cost per useful horse-power at the spindles, etc., can these mills be successfully dealt with by electrical power. If the 59 per cent. efficiency mentioned above is, however, a usual figure, there can be no question that the cost of providing motors could be successfully met.

In the investigations of jute mills, however, the author has not been able to make the friction losses greater than 30 per cent., and information on this point from engineers who have to deal with mills will be of value.

Collieries.—Several figures have been quoted recently as to the cost of independent plants at collieries, but the author may be excused for quoting the following figures again in order to complete this list of possible large power users.

In a paper † read before this Institution, Mr. C. P. Sparks quoted the following figures for the installation at the Powell Duffryn Collieries:—

Power house, 3,000 k.w. installed.

Average horse-power of motors at work—

Haulage...	2,900 H.P.
Winding	180 „
Fans	380 „
Pumps	340 „
Screens	240 „
Auxiliary motors	480 „
	4,500 „

* *Journal of the Institution of Electrical Engineers*, p. 757, vol. 34, 1905.

† *Ibid.*, p. 477, vol. 36, 1906.

Total cost—

	£
Power house	11·5
Distribution and sub-stations ...	2·6
	£14·1 per k.w. installed.

Annual output in units, 4,800,000.
Average load factor, 36 per cent.
Coal consumption per unit, 3·7 lbs.
Cost of coal, 5s. per ton.

The cost of power was as follows :—

Running cost	0·18d.
Interest and depreciation, 10 per cent. on £34,000	0·17d.
Total cost	0·35d.

In such a case it would appear to be impossible for an outside power supply to compete successfully with the colliery's own plant; for reference to the schedule of charges in Table XIV. will show that for an annual load factor of 36 per cent. a price of 0·47d. should be obtained for transformed alternating current as against 0·35d. obtained by the local plant. It is estimated that this local cost will be reduced to below 0·3d. as the installation becomes completed and as the load factor rises to 46 per cent., which then will make competition from outside quite impossible.

While this is true, however, of so large a local plant as 3,000 k.w., it would not, of course, apply to smaller collieries involving an installation of, say, 1,000 k.w., in which case the power station would no doubt successfully compete as the capital and standing charges would raise the cost materially.

A reference to the Institution *Proceedings* (for 1906) will show the ascertained load factors of various parts of the equipment, which need not be repeated here.

Steel and other large Works.—The remarks that have been previously made upon the cost of supply to engine works apply generally to works of this description. Annual load factors of from 20 to 25 per cent. are obtained, and the prices are found to range between 0·7d. and 0·6d. per unit. Therefore, on referring to the schedule of charges, it will be seen that a large power company could compete successfully with works of this description. Of course some deduction must be made in certain cases for the utilisation by steel works of their blast furnace gases, which are utilisable in gas engines.

The load factor of such works can be improved by the adoption of some such system as the Ilgner and by kinetic storage, and thus prevent the abnormal fluctuations in the loads of the rolling mills,

Although this system is costly to instal, on the other hand, of course, it considerably reduces the amount of generating plant which would otherwise be required to meet the "peak" loads. That this fluctuation is important the author has found from the fluctuation of load on comparatively small rolls in shipyards, and the additional cost of such a levelling-out system will be more than repaid.

Rope-making Works.—In the case of a rope-making works which the author investigated it was found that the cost represented practically 0·5d. per H.P.-hour, equivalent to 0·67d. per unit, the running hours being 2,700 per annum and the load being practically constant.

The annual load factor being 30 per cent., and the equivalent price in the schedule quoted by the author being a little over 0·5d. per unit supplied, it would obviously be possible to supply such works from an outside source.

Breweries.—The author investigated the driving of a brewery equipped with modern plant. The prime movers were gas engines, developing in all 194 H.P.

The following were the annual costs:—

	£	s.	d.
Gas at 1s. 6d. per 1,000 cub. ft.	338	14	9
Wages and repairs	516	12	4
Oils and stores	111	4	0
	£966	11	1

Interest and depreciation not given.

The cost of driving the shafting amounted to no less than £270 per annum: thus the net cost of useful power amounted to £696 11s. 1d.

The shafting was very lengthy, but owing to the positions of the various machines this was unavoidable, and it explains the reason of so great an annual loss. The wages and repairs are certainly very high indeed, but nevertheless the figures are the real figures given to the author by the authorities of the brewery.

The electrical cost after conversion was as follows:—

	£
66,550 units at 1·5d....	416
Oils and stores	20
Wages and repairs	78
Interest and depreciation on new plant...	112
	£626

or 2·25d. per unit.

In addition there was a great deal of saving of room by clearing out much of the shafting, and absence of vibration and noise and of dirt from the oil cups of the shafting.

Saw Mills.—The author was responsible for the electrification of a small saw mill which was driven by steam engines, the cost being over 2d. per H.P.-hour, or, say, 2·6d. per unit. This cost was only approximate, because of the necessity of calculating the actual H.P.-hours. The calculation was, however, fairly close, as the subsequent ascertained cost of electrical driving proved.

Seven motors were installed, ranging from 4 to 50 H.P. each, with a total horse-power of 179.

After one year's run it was ascertained that the total consumption was 50,785 units. The suppliers found the static transformers and high-tension switchboard, the low-tension switchgear and fireproof transformer chamber being found by the user.

The cost to the consumer was as follows:—

	£	s.	d.
Electricity	264	10	2
Oils and stores	9	3	4
Interest on sub-station building	12	0	0
	£285	13	6

or 1·35d. per unit, exclusive of capital charges on motors and wiring.

Miscellaneous Works.—Turning now to the general smaller power users within towns, there is no question that it is possible to supply these successfully from a central source, as none of these works are likely to exceed the running hours of, or the load factor from, a saw mill and joinery works when the annual hours are 2,700 and the load fairly constant; and it has been demonstrated that the latter works can be supplied economically from a central source, and at a cheaper rate than it can be generated by local plant. In each case of small power supply the author has found it quite easy to compete successfully with local plant, whether steam or gas driven, when each case is carefully tested and dealt with. Moreover, in works of this nature, space is of the utmost importance, and the ability to switch the outside supply on or off at will constitutes a very great boon to the user.

In Sunderland at the time of the last census, for which the author was responsible, and omitting the large engine works and shipyards, there were 587 motors, representing a total horse-power of 1,634, or an equivalent to 1,220 k.w. The maximum demand on the station plant was only about 500 k.w. There was thus a diversity factor of 2·4, the total units for the year being 404,999, or 332 per kilowatt installed. This represented a consumption of 810 units per kilowatt demanded for this particular load, and was thus under an average load factor of 10 per cent.

Table III. will be of value as giving statistical information of the horse-power, which may be estimated for works of various kinds.

TABLE III.

Description of Works.	H.P. Installed.	Number of Persons Employed.	H.P. per Capita.	Annual Units.	Units per Capita per Annum.
Corn mill	378	43	8·79	—	—
Printing (daily papers) ...	118	32	3·70	15,770	493
Saw mills	190	84	2·26	50,780	604
Steel-rolling mills	712	448	1·59	87,117	194
Mineral waters	40	32	1·25	10,483	327
Engine and boiler works ...	485	412	1·18	—	—
Engine and boiler works ...	750	1,020	0·75	1,031,892	1,012
Glass merchants	10	11	0·90	—	—
Coppersmith and foundry ...	120	199	0·60	12,172	61
Printing (general)	37	69	0·53	21,249	308
Printing (general)	8	16	0·50	5,564	347
Laundries (average of several)	10·8	40	0·27	25,054	626
Shipyard	2,099	1,874	1·12	700,000	373
Shipyard	477	446	1·07	198,000	444
Shipyard (with compressors)	6,900	5,564	1·24	2,980,000	536
Paper mill	1,820	410	4·43	—	—

This will be found useful in prospecting any district, as, short of a complete census of the actual power installed, the only way is to compute the horse-power per capita of the several factories. It is interesting also to note the relation of the maximum demand in works to the motors installed, though such figures are difficult to obtain, as manufacturers are not generally inclined to give this information. The following particulars are, therefore, somewhat limited, and perhaps a discussion on this paper will educe particulars of other classes of works which must be of general interest to the industry.

TABLE IV.

Description of Works.	1. H.P. of Motors Installed.	2. H.P. of Transformers Installed.	3. Maximum Demand. H.P.	— Ratio $\frac{2}{1}$	— Ratio $\frac{3}{1}$
Shipyard	1,576	1,200	900	0·76	0·57
Engine works ...	5,538	3,200	2,400	0·57	0·43
Printing	37	—	37	—	1·00
Saw mills	179	200	140	1·12	0·78

There is an enormous scope in this country for electrical applications among all classes of works. In the preparation of particulars for the recent London County Council Power Bills, the author and his colleagues ascertained that in the area of Greater London alone there are factories utilising altogether some 400,000 H.P., while in the evidence brought forward in support of the Administrative Company's Bill, 1906, this figure was given as 535,742 H.P., of which only 29,053 H.P. was then supplied from electrical undertakings. In most of these it would obviously pay the consumer and be in the interest of London and of the nation that these works should be electrified. In course of time this is bound to be. If Greater London were cleared of factory chimneys, coal dust, and ash wagons, and if mechanically propelled vehicles replaced horse vehicles, there would result an enormous change for the better in the physical conditions of life. Moreover, a cheap supply of power would assist to prevent the migration of power users and the consequent reduction in rateable value—a factor which reacts unfavourably on the remaining inhabitants.

As there are some 400,000 H.P. in factories to be equipped within the Greater London area, what must be the enormous total to be electrified within Great Britain?

That a very great impetus has been already given to the adoption of electrical power there is no question. In the town of which until recently the author was engineer, within four years the power load connected had grown from 2,000 to over 10,000 H.P. Other towns must have experienced a like growth, particularly cities like Manchester and others, where a proper and flexible system has been adopted, such as a high-tension 3-phase transmission, and every encouragement has been given to the power user.

Those towns are fortunate which have installed a flexible system wherewith, at a minimum cost of transmission, to reach large power users situated at some distance from the power stations; but those other towns, where only low-tension systems are at present available, will certainly have to face the problem of adopting a high-pressure system in order to reach works situated at some distance away, and it is these which will find the problem most difficult.

It is these smaller boroughs, and the undertakers in and about London principally, to which additional means must be given to enable electricity to become available for industrial purposes. These means are discussed at a later stage in this paper.

SECTION III.

A modern power station of important magnitude and favourably situated can be now equipped completely for £12 or £13 per kilowatt installed. One cannot imagine that this figure is likely to be reduced materially. A transmission system in which the cables are designed

to allow for diversity of loads supplied over a large area could be laid down also for £12 per kilowatt supplied; thus the all-round cost of an electrical installation is some £25 per kilowatt, excluding transformers.

The author's experience obtained in providing and equipping a large number of sub-stations has been as follows:—

TABLE V.

Cost of Static Transformer Sub-stations.

Kilowatts Installed.	Kilowatt Demand.	Cost of Buildings, Plant, and Switchgear.	Cost per Kilowatt Installed.	Cost per Kilowatt Demand.
		£	£	£
150	100	501	3·34	5·01
900	600	1,557	1·74	2·59
		Average ...	£2·54	£3·80

Cost of Rotary Sub-stations.

Kilowatts Installed.	Kilowatt Demand.	Cost of Buildings, Plant, and Switchgear.	Cost per Kilowatt Installed.	Cost per Kilowatt Demand.
		£	£	£
650	400	3,238	4·98	8·09
1,000	750	5,553	5·55	7·40
1,400	1,150	5,968	4·26	5·19
		Average ...	£4·93	£6·89

In new ground, of course, it is in the interest of every one concerned to use static transformers, not only because of their lower capital cost, but also because of the minimum space required by them, the absence of attendants, and the saving in running stores and repairs. Incidentally, the automatic arrangement of Mr. Berry should prove useful in factories, for, as a general rule, either offices in works require to be lit after the main power is shut down, or small repairing shifts are required at night, and lights required for watchmen in case of outbreaks of fire. The transformer losses in the larger sizes then become an important item if allowed to continue on circuit to supply these trivial loads.

There are, however, many large works which are already electrified and supplied from their own plant. If direct-current motors are already installed, then a rotary sub-station must be put in at a greater cost to both supplier and user. It then becomes a matter for calculation to the latter whether it will better pay him to replace his existing motors by 3-phase, or to pay a necessarily higher annual sum for transformed and converted current. In other words, the user has to decide whether the capital charges on the change-over of his motors will be less or more than the increased annual charge from a rotary as distinct from a static sub-station. Here, again, in power supply the difference between an all-round 85 per cent. efficiency for rotaries and 95 per cent. for statics cannot be overlooked.

The total capital cost per kilowatt installed at the power house, therefore, from the power station up to and including the user's sub-station, will be as follows :—

TABLE VI.

Cost of Stations, Transmission Systems, and Static Sub-stations.

Capacity of Sub-station.	Cost of Power House per Kilowatt.	Cost of Transmission System per Kilowatt.	Cost of Static Sub-station per Kilowatt.	Total.	Equivalent Cost at 1·66 Diversity Factor.
	£	£	£	£	£
100–250	13	12	3·33	28·33	18·33
250–1,100	13	12	1·75	26·75	16·75
			Average	£27·54	£17·54

Cost of Stations, Transmission Systems, and Rotary Sub-stations.

Capacity of Sub-station.	Cost of Power House per Kilowatt.	Cost of Transmission System per Kilowatt.	Cost of Rotary Sub-Station per Kilowatt.	Total.	Equivalent Cost at 1·66 Diversity Factor.
	£	£	£	£	£
250–500	13	12	5·20	30·20	20·20
500–1,500	13	12	4·26	29·26	19·26
			Average	£29·73	£19·73

These are, however, not the real figures to take when fixing a schedule of charges, because of the influence of diversity factor. This figure will be found to vary in different localities and among different industries. For instance, the ratio of the sum of maxima observed at the

several sub-stations in Sunderland to the actual observed maximum demand on the plant supplying them was only 1·25, because the industries supplied were very much of the same nature; but it is more general to find a diversity factor of 1·66 in a more varied set of industries.

The author is here speaking of the diversity of sub-station maximum loads compared with the observed maximum at the power station. Of course the ratio of the sub-station load to the horse-power of motors installed in factories is really only about 50 per cent., representing a diversity factor of 2.

The following statistics may be useful:—

TABLE VIA.

	Total Kilowatts Installed.	Maximum Load Observed.	Ratio.	Diversity Factor.
Engine works	1,574	750	0·50	2·00
Engine works	1,090	434	0·40	2·25
Shipyard	310	200	0·66	1·50
Shipyard	950	450	0·47	2·12
Shipyard	357	175	0·50	2·00
Dock cranes, etc. ...	223	56	0·25	4·00
Saw mills	135	88	0·65	1·50

Obviously the capital cost per available kilowatt at the power station depends very much on the diversity factor, for the figure which may be taken as the assessable value of a power house and transmission system in the one case is £25 ÷ 1·25, or £20; and in the other case, £25 ÷ 1·66, or £15. Thus the total capital cost of plant, including a static transformer sub-station, will be either £22·54 or £17·54, and the total cost of a rotary sub-station £24·73 or £19·73, according to size.

It is obvious that some differentiation must be made between a low-tension supply of direct current and one of alternating current when one is preparing a schedule of charges, for at any load factor, say 20 per cent., the capital charges as between the two systems represent an additional 5 per cent. for the rotary over the cost of static sub-stations, without counting the addition to standing charges from attendance or from stores, etc., required by the former type or the less efficiency of rotary sub-stations.

It is of little use straining to reduce the capital outlay per kilowatt on the power house and distribution system, if one is going to sink an additional £2 or £3 per kilowatt on rotary sub-stations, so that there

can be no doubt as to the wisdom of applying static sub-stations to all industrial purposes, wherever possible; and it is useless designing a highly efficient power house and transmission system if one throws away 10 per cent. at least in a sub-station.

Space is of great importance to the manufacturer, and so much less is required, of course, by the static sub-station (about 0·8 sq. ft. per kilowatt installed, as against 1·75 sq. ft. required in a rotary sub-station).

Power suppliers will do well, therefore, so to arrange their transmission system that works requiring 100 k.w. or more shall be supplied whenever possible from their own local static sub-stations. If the works can be grouped, a centralised sub-station can be put down with overhead low-tension mains to each of the works supplied from it, so as to minimise expenditure on this item.

In the published tables of 1907 of the London County Council Electric Supply Bill, a differential tariff was adopted by the advising engineers to the Council, but in no other Power Bill has this been specifically quoted. It is true that all Power Bills have maximum scales of charges, and can differentiate to various classes of consumer within those limits; but that is not the same thing as actually publishing a definite tariff which takes into account the differences above mentioned.

Of course the lighting charges will represent a curve higher at every point than the power curve. This is obvious from the fact that one does not get the same diversity factor with lighting consumers as with power consumers.

It is a comparatively easy matter to estimate closely the cost of a power station, and even of the transmission or distributing system and sub-stations; and the one factor which is really unknown, but upon which the scale of charges so much depends, is this diversity factor. 1·66 may be all right in one district, but if applied universally will certainly lead to trouble, for the revenue would be too small if the consumers were charged on a scale dependent upon a diversity factor of 1·66, if in practice this factor only proved to be 1·25. Therefore, in fixing maximum scales it is wise always to base the *maximum* rates upon a diversity factor of unity, and the actual commercial scales which will thereafter be charged will, no doubt, depend upon the experience which is gained by the supplier after some years of working.

To some extent the load factor can be raised artificially and by arrangement. In one locality the author got manufacturers to modify their dinner hours, so that instead of all shutting down synchronously, approximately half shut down from 12 to 1 p.m., and half from 1 p.m. to 2 p.m. Some power companies are also arranging to supply chemical works, in some of which certain processes can be shut down during restricted hours. This all helps, of course, and, as a matter of fact, is really about the sole reason why such very small charges are made to such consumers.

SECTION IV.

The severest handicap to power supply from existing London and provincial stations—whether administered by companies or municipalities—is, in the majority of cases, their present high capital cost per kilowatt. It is true that with each extension this figure is reduced; still, it will be found very difficult, especially in the case of London undertakers, to reduce their total much below £60 per kilowatt, or, in the case of large provincial stations, to reduce their capital much below £40 per kilowatt, or smaller stations to £50, taking into account all existing capital outlay.

This is where the modern power company comes in with the benefits of latest electrical practice, much cheaper buildings, larger and cheaper units of plant, and, generally, an altogether lower cost per kilowatt. Even with a power company, however, there is a danger that the capital, while minimised in the power house, may be unduly raised by the cost of transmission over too great a distance.

The present capital expenditure per kilowatt on London municipal systems averages £93, and London company stations £103; while the present expenditure on provincial municipal stations is £70·8, and provincial companies £90.

Table VII. shows the average prices charged by existing undertakings for power in 1906 within the area proposed to be covered by the various London Power Bills. Side by side are given the figures which ought to have been charged, assuming that the average load factor of power supply is 20 per cent. and the diversity factor 1·66. The average load factor will certainly not be more than 20 per cent.; and if it really has a lower value, then the author's criticisms tell so much the more against the present suppliers. It will be seen that in the case of the South Metropolitan Company the average price charged for power was 1·048d. per unit, whereas the real charge should have been 2·367d.; or take the Westminster Company: the charge was 1·83d., whereas it should have been 2·4d. Or take the municipal cases: Battersea, where the average charge is 1·5d., and the real charge should have been 1·975d.; Islington, where the charge was 1·37d. and the real charge 2·2d.; or St. Pancras, average charge of 1d., and the real charge should have been 1·83d. This means, obviously, that the power user is being supplied by these undertakers at the expense of their lighting consumers, who are paying an unduly high rate for their supply, or in the case of certain London municipalities at too high a cost of public lighting. This system may do very well where there is only a small amount of power to be given, and where the proportionate revenue is only a small fraction of the gross receipts; but let there be a big increase in the amount of power supplied at the same rates (and despite its effect in the general reduction of generating costs), it will be found then that the total receipts are not commensurate with the total costs, and the result will be an unsuccessful undertaking.

Table VII.

Comparison of Actual and Correct Charges for Power by Local Authorities.

Local Authority.	Actual Charge.	Correct Charge.	Probable Charge from Power Supply Company.
	d.	d.	d.
Barking	1·686	1·747	
Barnes	1·658	1·677	
Battersea	1·500	1·975	
Bermondsey	2·060	1·792	
Croydon...	2·500	1·775	
Ealing	2·351	2·361	
East Ham	1·570	1·535	
Erith	1·581	1·595	
Fulham	1·000	2·024	
Gravesend	2·000	1·638	0·94 for direct current;
Hackney	1·240	1·384	
Hammersmith	1·240	1·201	
Hampstead	2·090	1·839	
Hornsey...	1·270	2·225	
Ilford	1·324	1·440	0·765 for transformed alternating current
Islington	1·370	2·214	
Kingston-on-Thames ...	2·000	2·370	
Leyton	1·623	1·857	
Poplar	1·430	1·686	
St. Pancras	1·000	1·838	
Shoreditch	1·400	1·958	
Southwark	1·900	2·034	
Stepney	1·000	1·549	
Walthamstow	2·044	3·129	
West Ham	1·234	1·340	
Willesden	1·431	2·988	

Comparison of Actual and Correct Charges for Power by Metropolitan Companies.

Name of Company.	Actual Charge.	Correct Charge.	Probable Charge from Power Supply Company.
	d.	d.	d.
Brompton and Kensington	1·000	1·979	
Charing Cross (City) ...	1·940	2·198	
Ditto (West End)	1·940	2·511	
Chelsea	1·500	2·388	0·94 for direct current;
City of London ...	1·700	1·554	
County of London ...	1·800	2·267	
Kensington and Knightsbridge ...	2·140	2·082	
London Electric ...	1·860	1·862	0·765 for transformed alternating current
Metropolitan	2·460	2·093	
Notting Hill	3—⅛	3·328	
St. James' and Pall Mall	2·370	1·928	
South London	1·780	1·518	
South Metropolitan ...	1·048	2·367	
Westminster	1·830	2·404	

It is very unwise of central station engineers and managers to hoodwink themselves by thinking they are going to supply a small section of power users at a nominal cost, and they forget that the Nemesis must come if those power consumers extend so as to become an important and dominating proportion of the total. In the case of many London boroughs and London companies it is really quite an impossibility for them to supply power at anything like the cheap rates which will on the one hand induce power users to purchase electricity from them, and on the other leave their supply undertakings on a sound commercial basis.

That there is only one way in which to deal with the London problem must be obvious to all engineers—that is, through some means, a large power station must be erected at a minimum cost, which, without prejudicing the present capital involved in the existing undertakings, will enable electricity to be supplied for all purposes at considerably less rates than now obtain. The capital sunk in existing undertakings cannot, of course, be ignored; it constitutes a deadweight upon electrical supply in London, which will remain until this original capital shall have been redeemed. That cannot be for several years yet, and, therefore, the only compromise is to provide this supplementary low-cost station to co-operate with and assist those already in operation.

The effect is apparent even now, for the more successful companies, whose able administration cannot be questioned, find that since they have had to reduce their scales of charges they are not able to declare the same dividends as they were able to do a few years back; and this effect is likely to get worse rather than better. Or take the case of some London municipalities who have so very foolishly contented themselves by merely putting aside a sinking fund on a 42-years basis, well knowing that their plant could not possibly exist for so long a time. Those municipalities who have wisely added a depreciation fund to this long-period sinking fund are, of course, in a better position, but they are, unfortunately, the exceptions. It will certainly be admitted that twenty-five years is the maximum period which engineers would be wise to allow as the equated life of their plant. Such municipalities who have allowed depreciation merely at the rate of a 42-years' life have, therefore, a good deal to make up for past years, and they simply cannot put themselves in a sound financial position and at the same time offer their power users such a low scale of charges as will promote a large revenue from the sale of electricity for power. They, again, can only carry out the duties imposed upon them by their orders by a wise co-operation with some central scheme.

Turn to the provinces. Table VIII. shows the true cost of production in some of the larger provincial municipal stations. These figures are based upon the official published results for 1906 or 1907. In every case the capital charges have been taken as requiring interest at an average rate of 3½ per cent. and depreciation at 2¾ per cent. In other words, a modest 6¼ per cent. has been taken upon the total capital

expenditure. The author thinks that no one will quarrel with him as to this figure, which, if anything, is on the low side.

TABLE VIII.

True Cost of Production in Larger Municipal Stations for the Year 1907.

Annual Load Factor.	Manchester.	Nottingham.	Leeds.	Glasgow.	Average.
Per cent.	d.	d.	d.	d.	d.
5	5·03	4·93	5·24	4·70	4·97
10	2·71	2·72	2·82	2·59	2·71
15	1·93	1·98	2·02	1·88	1·95
20	1·55	1·61	1·62	1·54	1·58
25	1·32	1·39	1·37	1·32	1·35
30	1·16	1·21	1·21	1·19	1·19
35	1·06	1·14	1·10	1·08	1·09
40	0·98	1·06	1·01	1·01	1·01
50	0·84	0·94	0·89	0·90	0·89
60	0·77	0·86	0·81	0·83	0·82
70	0·73	0·82	0·75	0·78	0·77
80	0·69	0·79	0·70	0·74	0·73
No. of units sold	47,564,903	9,530,096	11,368,956	21,536,425	—
Plant installed ...	33,800 k.w.	10,043 k.w.	10,440 k.w.	18,493 k.w.	—
K.W.D.	25,180	5,765	7,470	16,304	—

TABLE IX.

True Charges for Power Supply, based on Cost of Production and 1·66 Diversity Factor.

Annual Load Factor.	Manchester.	Nottingham.	Leeds.	Glasgow.	Mean.
Per cent.	d.	d.	d.	d.	d.
5	3·19	3·16	3·32	3·02	3·17
10	1·79	1·83	1·86	1·75	1·81
15	1·32	1·39	1·38	1·33	1·35
20	1·09	1·17	1·09	1·11	1·16
25	0·95	1·04	0·99	0·99	0·99
30	0·86	0·95	0·89	0·90	0·90
35	0·79	0·89	0·82	0·84	0·83
40	0·74	0·85	0·77	0·80	0·79
50	0·67	0·81	0·70	0·73	0·73
60	0·62	0·73	0·65	0·69	0·67
70	0·59	0·70	0·62	0·66	0·64
80	0·56	0·68	0·59	0·64	0·62

Table IX. shows the scale of charges which could be adopted for power after allowing the diversity factor. There it will be found that

the average rate for, say, a 20 per cent. load factor consumer is 1·16d., whereas a power company ought to be able to supply such a consumer

Table X.

True Cost of Production from Smaller Municipal Stations for the Year 1907, at Different Load Factors.

Annual Load Factor.	Aberdeen.	Dundee.	Cheltenham.	Coventry.	Darlington.	Oldham.	Average.
Per cent.	d.	d.	d.	d.	d.	d.	d.
5	5·66	4·43	6·20	4·90	4·25	6·07	5·25
10	3·04	2·40	3·42	2·67	2·79	3·32	2·94
15	2·17	1·72	2·50	1·93	1·71	2·40	2·07
20	1·74	1·39	2·03	1·57	1·39	1·94	1·67
25	1·46	1·18	1·75	1·34	1·19	1·67	1·43
30	1·30	1·05	1·58	1·19	1·06	1·49	1·28
35	1·17	0·95	1·43	1·08	0·98	1·36	1·16
40	1·08	0·88	1·34	1·01	0·91	1·25	1·07
50	0·94	0·78	1·19	0·90	0·81	1·12	0·95
60	0·86	0·71	1·11	0·82	0·74	1·04	0·88
70	0·80	0·66	1·04	0·77	0·70	0·97	0·82
80	0·75	0·63	0·99	0·73	0·67	0·90	0·77
No. of units sold	4,280,248	3,439,231	1,701,370	2,522,110	1,095,287	4,133,615	—
Plant installed ...	3,810 k.w.	3,010 k.w.	1,980 k.w.	2,400 k.w.	748 k.w.	5,232 k.w.	—
K.W.D.	2,633 k.w.	2,367 k.w.	1,316 k.w.	1,533 k.w.	740 k.w.	2,400 k.w.	—

Table XI.

True Charges for Power Supply based on Cost of Production and 1·66 Diversity Factor.

Annual Load Factor.	Aberdeen.	Dundee.	Cheltenham.	Coventry.	Darlington.	Oldham.	Mean.
Per cent.	d.	d.	d.	d.	d.	d.	d.
5	3·58	2·82	3·99	2·53	2·73	3·87	3·25
10	2·00	1·59	2·32	1·49	1·58	2·22	1·86
15	1·47	1·19	1·76	1·14	1·16	1·67	1·56
20	1·21	0·98	1·48	0·97	1·00	1·39	1·17
25	1·05	0·86	1·31	0·86	0·89	1·23	1·03
30	0·95	0·78	1·20	0·80	0·81	1·12	0·94
35	0·87	0·72	1·12	0·75	0·74	1·04	0·87
40	0·82	0·68	1·06	0·71	0·70	0·98	0·82
50	0·74	0·61	0·98	0·65	0·66	0·90	0·76
60	0·69	0·57	0·92	0·63	0·62	0·85	0·71
70	0·65	0·55	0·88	0·60	0·58	0·81	0·68
80	0·62	0·52	0·85	0·58	0·56	0·77	0·65

successfully at not more than 0·8d., or a reduction of 30 per cent. This is the result of the larger stations. And if one turns to the smaller provincial stations, which are given in Tables X. and XI., the results are very much the same. It must not be overlooked, however, that

the larger districts will almost certainly have a higher diversity factor, and thus the author is quite probably wrong to assume the same coefficient in both tables. These figures, of course, will improve as years go on—or at least it is hoped so—because with extensions of the stations, as has been said before, the capital cost per kilowatt should be reduced. This will apply with more force to the larger systems. But a warning should be noted by station engineers that the future will bring, in all probability, a very large proportionate increase of low-price power units, and with improvements in lamps and their reduced energy consumption, the proportionate lighting units may even fall below their present figure.

This all means that the average revenue per unit will be considerably lessened. The only possible way, therefore, to meet this is to watch every penny of capital spent, and not to waste money on ornate buildings and expensive land, or to instal too much spare plant. It is only by these means that electrical supply will be able to hold its own.

The author may be criticised for taking only 6¼ per cent. in the case of the large undertakings, while he has taken 10 per cent. in Tables I.

TABLE XII.

Load Factor.	Newcastle Power Company.	
	Average Cost of Production, year ending Dec. 31, 1905.	Real Cost for Power Supply, 1·66 D.F.
Per cent.	d.	d.
10	2·31	1·49
15	1·62	1·07
20	1·28	0·86
25	1·07	0·74
30	0·98	0·65
35	0·83	0·60
40	0·76	0·55
50	0·65	0·49
60	0·58	0·44
70	0·53	0·42
80	0·50	0·39

and II. for the smaller local works plant; but he is perfectly justified in so doing, and for this reason. In the supply stations the plant has been built to careful specifications, is under expert supervision, and there is a reasonable proportion of stand-by plant with consequent relief in the running hours per unit of plant. Whereas in the case of a local factory plant, not only is there considerably less supervision, but the installation includes no stand-by plant. Therefore a depreciation of 5 per cent. is taken instead of 2½ per cent., and interest is allowed at 5 per cent., this being the figure which manufacturers generally

require as against the 3½ per cent., the average value of municipal stocks.

Take the case of the Newcastle Power Company—which at present

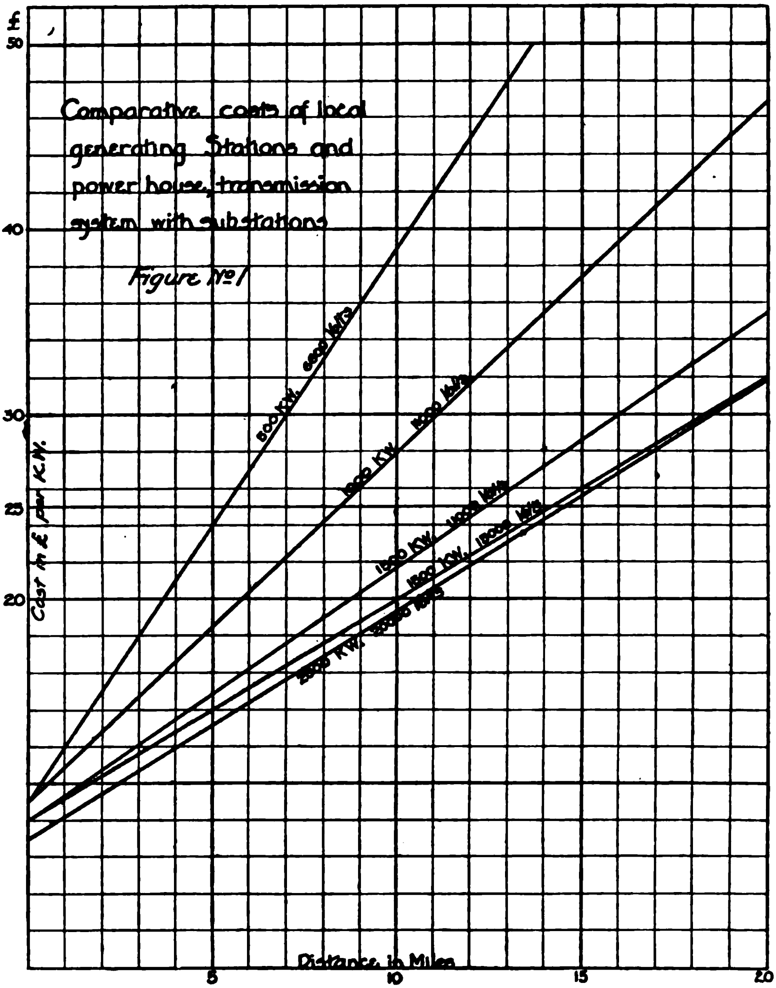


FIG. 1.

is the only one operating over a large area—and on the scale on which it is expected power companies will work. Table XII. gives the real cost of production based on the figures for the year ending Dec. 31, 1905.

Side by side is the power scale of charges based on 1·66 diversity factor. This company has an expenditure of £59·6 per kilowatt

installed, of which the cost of the stations has been about £33 per kilowatt; mains and sub-stations, etc., £26. It is interesting to note that the plant installed in sub-stations amounts to about 35,000 k.w., supplied by generating plant installed at the station with a total capacity of 20,000 k.w. Now it cannot be said that even this company, having so large a capital expenditure involved, and operating on such a big scale, is entirely satisfactory in its finances with a depreciation and reserve account amounting to only 1·07 per cent. for the year. The question arises on such an undertaking whether undue expenditure has not been made upon the transmission system.

The author read a paper in 1904 which dealt with the question of transmission distances in this country, and then showed that at a pressure of 10,000 volts the economical distance for transmitting a load of 1,500 k.w. was only 11 miles, where the displaced local station cost £20 per kilowatt. A new diagram (Fig. 1) is now given which shows the effective radii of distribution for different loads, and different costs per kilowatt. An explanation is necessary to make this curve perfectly clear.

It has been assumed that every 100 k.w. installed at the power station will supply 166 k.w. outside; and the cost of the power station is taken at £12 per kilowatt, or £7·2 per effective kilowatt supplied. Each curve represents a consumer, or group of consumers situated at the respective distances and supplied through high-tension cables laid in duplicate and static transformer sub-stations. The horizontal lines show the costs of alternative local stations per kilowatt. The point at which the two curves cross is that at which both systems are equal in cost. It shows, therefore, that so far as capital is concerned the effective radii of transmission in miles are as follows:—

TABLE XIII.

Kilowatt.	Voltage.	Local Station Costing per Kilowatt.				
		£20.	£25.	£30.	£40.	£50.
		Miles.	Miles.	Miles.	Miles.	Miles.
500	6,600	3·7	5·3	7·0	10·3	13·8
1,000	11,000	5·8	8·5	11·2	16·5	22·0
1,500	11,000	7·8	11·5	15·0	22·0	—
1,500	15,000	8·5	12·4	16·2	—	—
2,000	20,000	9·7	14·2	18·5	—	—

These distances, of course, only refer to a comparison between a supply from a central source and local plants installed within factories, which usually cost from £20 to £25 per kilowatt.

TABLE XIV.

Estimated Charges from Bulk Supply Undertakings.

Annual Load Factor.	I. London County Council, 1907. Estimated Average Charges to Authorised Distributors. (a) E.H.T.	(b) T.Alt.	(c) D.C.	II. London County Council, 1907. Maximum Prices to Power Users. (a) T.Alt.	(b) D.C.	III. Administrative Co., 1906. Maximum Prices.	IV. Newcastle. Real Cost based on 1906 Figures and 1·66 D.F.
Per cent.	d.	d.	d.	d.	d.	d.	d.
10	1·002	1·159	1·453	1·738	2·179	1·50	1·49
15	0·728	0·839	1·042	1·258	1·562	1·16	1·07
20	0·591	0·679	0·836	1·019	1·254	1·02	0·86
25	0·508	0·583	0·713	0·875	1·069	0·94	0·74
30	0·454	0·519	0·631	0·779	0·946	—	0·65
35	0·414	0·474	0·572	0·710	0·858	0·81	0·60
40	0·385	0·439	0·528	0·659	0·792	0·75	0·55
50	0·344	0·391	0·466	0·587	0·699	0·72	0·49
60	0·317	0·359	0·425	0·539	0·638	—	0·44
70	0·297	0·337	0·396	0·505	0·594	—	0·42
80	0·282	0·319	0·374	0·479	0·561	0·64	0·39

Transmission by overhead lines would still further increase these radii, of course, but there is still much difficulty and great expense in obtaining the necessary easements for high-tension lines in this country.

Table XIV. sets out the estimated charges of various power companies. In the first column are given the estimated average charges to authorised distributors which were proposed in the London County Council (1907) Bill, column I*a*. being for extra-high-tension untransformed energy at the consumer's terminals, column I*b*. for transformed alternating current at any pressure, and column I*c*. for transformed and converted direct current at any pressure. In the next column are set forth the maximum prices to power users under this Bill; in column III. the maximum price proposed by the Administrative Company of 1906; and in column IV. the real tariff which could be charged by the Newcastle Power Company, based on their 1906 results and with an estimated diversity factor of 1·66, is set out again for convenience.

SUMMARY.

The costs of various prime-movers installed in factories of various kinds have been given, and examples from different works. The present inability of most undertakings to meet a large demand for power at such rates as to hold their own with local plants has been discussed. What, then, is the remedy to remove this somewhat precarious state of things? It must be admitted that the indictment is true, generally speaking.

If Table XIV. is compared with Tables I. and II., it will be seen that a power company can compete successfully with any of these other prime-movers if the power company supply transformed alternating current, but if the power company is constrained to supply direct current, then it is only smaller plants—*i.e.*, 100 H.P. or less—with which it can hold its own.

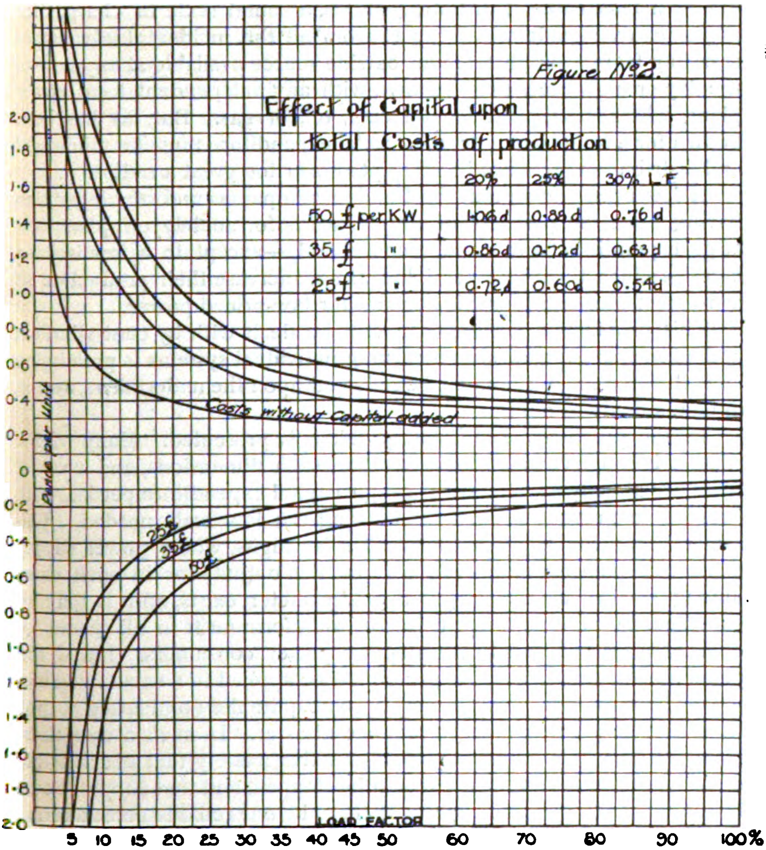


FIG. 2.

It is not suggested that the figures in this table are the ultimate minimum charges for power which power companies can afford to make, but they cannot be reduced materially.

The influence of additional capital cost per kilowatt is shown in Fig. 2, in which successive curves are shown adding to each the capital charges due to the increased expenditure.

It will be seen that the difference in cost of production, at 20 and

25 per cent. load factors, between a system costing £25 per kilowatt and one costing £50 per kilowatt, is 32 per cent.—and at 30 per cent. load factor, 29 per cent. This demonstrates very clearly how important it is to minimise the capital involved.

The author believes he has made out a case for showing that in all the larger power stations where the capital expenditure is reduced to a minimum and the supply is produced on a sufficiently large scale but within a reasonable distributing radius, a sound scale of charges for power can be made which (apart from all the incidental advantages resulting from a supply from an outside source available at any hour of the day or night) can hold its own with any independent local power plant, whether supplied from steam, oil, or gas. But, as has been known for a long time now, the capital expenditure must be minimised and distribution must not be attempted over too great a radius.

It has been shown that the London companies and municipalities, having been the pioneers of supply, are unfortunately crippled by a large expenditure, and cannot conceivably—except in a few isolated cases—reduce their cost per kilowatt nor increase their production so as to meet economically this demand for low-priced power.

This problem in London can only be dealt with by centralisation, and all further expenditure on existing individual systems is prolonging an evil which must ultimately be reflected upon the undertakers, and is, in fact, already so doing.

In the provinces a different problem is presented. Many of the smaller boroughs cannot hope to attain such dimensions, and many of them are already saddled with a high capital expenditure per kilowatt, such as to prevent them from offering the low scale needed. This remark does not apply to the smaller boroughs, where power supply cannot become a dominating part of the output, for in such cases among smaller users a larger diversity factor exists, and it may be expedient to sacrifice the principle of a uniform tariff to all classes by offering a special rate to such sparse power consumers. But if this principle—or rather want of principle—be applied in a small industrial town where the power supply may become quite the dominating output, then such a policy can only produce disaster. In such a case there appears to the author to be only one alternative, namely, that all extensions of the local generating plant should be stopped, and that supply should be taken from a power company, if there be one reliably constituted in the neighbourhood, as has been arranged by the Middlesbrough and Tynemouth municipalities, for instance; or a joint Board of local towns could co-operate at a very considerable saving to the respective partners if sufficiently proximate to one another. If this be too Utopian, then possibly the County Council of the district could take the matter up, or by the aid of legislation the principal borough of the group could undertake this responsibility.

It will be found economically more sound to concentrate plant in the larger town and transmit to sub-stations in the surrounding towns than to go on increasing each station. That station offering the better

facilities for circulating water, coaling, and cheap land and assessment, would naturally be selected.

Electricity supply differs materially from gas supply in the want of economical accumulation, and to attempt to store electrical energy at the present time on too large a scale would necessitate too great a capital cost. Where separate gas undertakings in small boroughs—having operated for so many years without opposition, and having either paid off their debt or saved a very large reserve fund—are able to score is in this storage and consequent higher works load factor, representing, of course, a minimum cost of producing plant. By the multiplication, however, of small electricity stations, the initial charges of starting the undertakings, the smaller units of plant, the unnecessary amount of stand-by, neither of which would be so accentuated in one common station—all tell against the successful competition of the electricity undertaking, opposed, as it probably is, by the already established gasworks.

For small local boroughs, therefore, to go on adding to this cost, and therefore increasing their difficulties, is to the author's mind a grave mistake. It is of no use arguing that the small gas undertakings paid their way and have now become successful even on the small scale referred to. The conditions are so utterly different, for the gasworks had an open field.

Looking back, it would have been much wiser if the controlling authorities—namely, the Local Government Board and the Board of Trade—had prevented some of the smaller local boroughs from adopting their separate electricity stations. The only remedy the author can suggest, after an extensive study of this problem, is the co-operation of neighbouring boroughs who already have plant; the supply of neighbouring townships from a central borough, which must be effected despite municipal jealousy; and in the case of new boroughs requiring —as they will require—an electrical supply, an arrangement with a neighbouring borough or with a power company for this supply. This does not apply to municipal boroughs only, but to companies as well. The author hopes that the results of discussing this paper will not only establish in the minds of factory owners that electricity supply from outside with all its advantages can be made successfully and in competition with local power installations of any kind in the majority of cases, but will also awaken engineers responsible for town installations to the advisability of co-operation rather than extending small stations individually at greater cost.

Discussion.

Mr. J. H. Rider: The importance of Mr. Snell's paper can be gauged to some extent by the large number of station engineers who are present with the intention to criticise him severely, and as I am rising to support what Mr. Snell has said, I am afraid subsequent speakers will probably find fault with me as well as with the author. I am not Mr. Rider.

Mr. Rider. going to discuss the paper in its entirety. I am going to confine my remarks to a few of the points on which I have had most experience. Taking page 290 of the paper first, Mr. Snell points out that the effect of high capital expenditure is to prevent many installations from being able to charge at a reasonable price for power, and almost at once he opens out into what I would call the old discussion as to whether the supply of power should be taken on by an existing station, equipped originally for lighting, as a "by-product," or whether it should be taken on as a proper addition to the load and treated in a way fair to the lighting customers. In my opinion the supply of power from a power station is one thing, but from a station which I will call for the moment a lighting station it is another thing; but it should not be another thing from the point of view of the fairness of the charges. It is rather, I am afraid, a common habit in the case of a lighting station with a poor load factor to try to encourage the power load by offering it at a very low price. It is then attempted to make up whatever loss may accrue from the cheap power supply by charging a higher price than should be charged under the circumstances for private lighting, and more commonly for street lighting, particularly when the station belongs to a municipality which is itself the street lighting authority, and can charge itself practically what it likes. We have all heard the story of the fair proprietor who, to induce people to come into the show-yard, charged a very low price for the swings, his theory being that, although he lost on every swing, he made it up on the roundabouts. That is all very pretty, and will pay so long as there are enough roundabouts to make up the loss; but if the demand for the swings is going to become very great, preponderantly great, then the roundabouts will not make up the loss on the swings. I am very much afraid—I agree with Mr. Snell in this particular—that many stations are charging for the power supply at a price which is too low, taking all their costs into account. They are making up their losses by charging a higher price to the private lighting consumers and for the public lighting than they should. The only true basis on which to charge is the load-factor basis tempered by the diversity factor. It is all very well to say it is impossible to get custom if one attempts to charge individual consumers on the maximum demand or on the load-factor basis. Difficulties may be met with in that way, but they can partly be got over by treating the consumers as a class and by applying the maximum demand principle to the fixing of the price. Then it will be found that the private lighting, the street lighting, and the power users are each being charged on the proper basis; they are each returning the proper amount of profit to the undertaking, and whichever class of consumer should increase their demand it will not make any difference to the commercial stability. With a lighting load which is bringing in a good return and a power load which is not quite paying on a certain basis of charge, if that power load increases it may bring the costs down slightly, but it may go on increasing until one is in the

Bankruptcy Court because too little is being charged for the power supply. On page 306 the author gives some interesting figures with regard to the diversity factor which he has found from experience in certain classes of work; those are set out in Table VIA. From his own experience in Sunderland he had a diversity factor of 1·25. I presume in this case he is speaking of sub-stations which are supplying lighting and power. It may be of interest to the meeting to know that, even with such a constant character of load as the London County Council tramways, we have a diversity factor of 1·25 among our various sub-stations. I myself am very much surprised at that, considering the fact that the tramway load increases fairly generally over all parts of the system at the same time. We have our peaks, of course, but the diversity factor is 1·25. Consequently, if one can only get a lighting load, plus a street-lighting load, plus a power load, I think the figure the author gives of 1·66 may be even too low, but it will be all the better for the giving of a cheap supply. The author emphasises the great importance of attempting to give a power supply by the cheapest possible means, namely, by 3-phase alternating current through static transformers. He quite properly emphasises the greater expense of using rotary transformers. I have been through these figures and checked them with my own experience, and I can corroborate the relative prices he gives between the static transformers and the rotaries. I think all people who are trying to sell power cheaply should go in as far as possible for alternating-current motors, the only disadvantage of which in certain cases may be the difficulty of getting variable speed. In Table XIV. the author gives some very interesting comparative figures of proposed charges from bulk supply undertakings, and also in column IV. of that table his understanding of the correct costs of the Newcastle Company. The first three columns give the estimated average charges for supplying in bulk; the next two columns give the maximum prices proposed for private power users, which are really 50 per cent. higher. It is very interesting in connection with that table, at any rate it is interesting to me, to be able to compare column I. (*c*) and column IV., where we have in the first case the estimated average charges for low-tension direct current in the London County Council Bill, and in the last column the actual cost, according to the author's basis, of the Newcastle Company's working. Comparing the two columns it will be noticed that all through, from a 10 per cent. to an 80 per cent. load factor, the prices are remarkably close to one another, in fact only varying something like 25/1000d. It shows, at any rate, that the basis of the curve adopted by the London County Council engineers was a true one, because the actual costs of the Newcastle people compare right through from beginning to end. I agree strongly with Mr. Snell's statement where he says that the true way of getting at the scale of charges is to fix the maximum price on a unit diversity factor. We are then safe whatever the character of the load, and we can then charge our actual price according to the Mr. Rider.

Mr. Rider. diversity of our various classes of consumers. I also agree with him strongly in one of his remarks on page 318, where he says that the problems in London can only be dealt with by centralisation, and that all future capital expenditure on individual systems or local power houses is wrong. I know that some of the gentlemen present will think I am prejudiced in that connection; I might have been, but I am not now, because I have no axe to grind. I am giving it as my honest conviction that it is wrong to perpetuate existing individual systems—I say "systems" advisedly, because there are about thirty of them in London. We cannot possibly get a cheap power supply unless we centralise, co-operate, and do away with the extension of individual generating stations and systems.

Mr. Taylor. Mr. A. M. TAYLOR: The first point that strikes me is that the author almost totally ignores the question of the supply to all consumers smaller than 100 H.P. Thus in Table VI. he considers nothing smaller, as a direct-current sub-station, than 250 k.w. Where, I would ask, does he provide for the cost of delivering power to small users who are already equipped with direct-current motors and for whom the direct-current low-tension network has already been laid down? Some one has to bear the cost of this, even if the London County Council supply the energy to the existing authority.

Again, in Table VII. he compares the average charges made by certain London companies to all sorts of small consumers with, I presume, the costs at which the London County Council would deliver power to consumers large enough to pay for the cost of bringing extra high tension on to their premises.

He should have added to these latter costs those of distributing from sub-centres at low tension, amounting to, at the very minimum, £20 per kilowatt of maximum demand—more often £40 per kilowatt.

Then as regards his Fig. 1 and the limiting distance to which he can supply, he takes a cost of generating station only to be attained by a concern of the size of the London County Council scheme (say 100,000 k.w.), according to the curve of capital costs given by him four years ago in his paper before the Institution of Civil Engineers.* Even with this he virtually fixes the limit of supply at 10 miles, in order to compete with private plants of moderate size. He advocated there accumulator sub-stations, but he now passes them by with a casual remark. I should like to point out that in many cases of the supply of power over a large area, such as by the power companies or others, the introduction of accumulators (on a proper basis) will not only pay for all their costs of upkeep, but save a sum annually which could be employed to double, or even treble, the distances which he suggests as the limits of profitable supply.

As I hope to be saying something on this question shortly in a different quarter, I will only say here that the subject of accumulators has, up to the present, had only scant justice done to it in this country. It is possible to put in cells to give 6 k.w. output, on a 2½ hour

* *Proceedings of the Institution of Civil Engineers*, vol. 159, p. 143, 1904–5.

basis, on one square yard of floor space; or, if in two tiers, 12 k.w. per square yard. The capital costs can be got down to £10 per kilowatt output on a 2½ hour or £5 per kilowatt on a 1 hour basis. The fact that in the United States and Germany hundreds of thousands of kilowatts are supplied by accumulators at the peak of the load, and are displacing generating plant to that extent, surely means that we in this country are not alive to our opportunities in this respect. Mr. Taylor.

I wish especially to direct attention to Tables VIII., IX., and XII. I submit to the meeting that Table VIII. does not give sufficient credit to our municipal undertakings. The figures for Manchester in Table VIII. are worked out on the basis of 0·39d. per unit, plus £8·48 per kilowatt of maximum demand (at the station).

I submit that 0·20d. per unit, plus £9·98 per kilowatt, is much nearer the mark (see my paper on "Central Supply Station Economics").* This assumes that only the "true" running cost is taken in the 0·20d., and that all wages are debited to standing charges, which latter is Mr. Snell's own basis. This would give 0·54d. per unit for 80 per cent. load factor instead of 0·69d. But to compare with his London County Council figures we should deduct quite £3 per kilowatt for distributing expenses from sub-centres, giving *1·15d.* and *0·44d.* per unit for 20 per cent. and 80 per cent. load factors respectively, as against his figures of 1·55d. and 0·69d.—a very material difference.

As regards Tables IX., XI., XII., and XIV., I submit that these tables are very misleading, if indeed they are not on a totally incorrect foundation. Referring first to Table IX., the impression conveyed is that the author's first column refers, as in Table VIII., to the load factor of the station. This, however, it certainly does not do. But the two columns of prices of Table XII. are calculated in precisely the same way as are the Manchester columns of Tables VIII. and IX., and it cannot be denied that here (Table XII.), at any rate, he intends the station load factor to be conveyed in both instances.

Assume, however, that the author is not consistent with himself and that he replies that the load factors in Table IX. are either the average consumers' load factors or the individual consumer's load factors. In either case it is obviously incorrect to consider a uniform diversity factor throughout Table IX. It should be sufficient to point out that the diversity factor of 1·66 is only obtained on the basis that each consumer, on the average, only runs his plant at full load, during the hours of peak load, for six minutes out of every ten. This he can only do by reducing his load factor for that period from 20 per cent. to 12 per cent., and this latter is probably also the value of the average consumers' load factor, which should correspond with the 20 per cent. station load factor in Table VIII. Not only so, but as the consumers' load factor increases (that is, as we go down the table in Table IX.) the diversity factor of the general load goes down, till at 30 per cent. it is

* *Journal, Institution of Electrical Engineers*, vol. 39, p. 367, 1907.

Mr. Taylor. unity; or, if a night shift be run, it is unity at 60 per cent. consumers' load factor. In other words, Mr. Snell should have added a seventh column to Table IX., giving varying values of the diversity factor, corresponding with improvement in the class of load for each particular station.

Table XII. contains the fundamental heresy of dividing the charge per unit (on account of fixed charges), as obtained at the station, by the diversity factor. Mr. Snell cannot get away from the fact that the left-hand column of Table XII. is based upon a running charge of 0·24d. per unit, plus £7·6 per kilowatt of station maximum demand, while the right-hand column is based upon 0·24d. per unit plus £4·8 per kilowatt of consumers' maximum demand, the latter figure being the result of dividing £7·6 by the diversity factor. In other words, the equation for the standing charge in the left-hand column for, say, 80 per cent. load factor is—

$$\frac{\pounds 7{\cdot}6 \times 240}{1 \text{ k.w.} \times 8{,}760 \times \frac{80}{100}} = 0{\cdot}26\text{d. per unit}, \quad . \quad . \quad . \quad (1)$$

while for the right-hand column it is—

$$\frac{\pounds 4{\cdot}58 \times 240}{1 \text{ k.w.} \times 8{,}760 \times \frac{80}{100}} = 0{\cdot}15\text{d. per unit}, \quad . \quad . \quad . \quad (2)$$

giving respectively (when added to the 0·24d. running cost) his figures of 0·50d. and 0.39d. per unit.

It will be obvious that equation (2) is simply equation (1) divided by the diversity factor, which is the heresy alluded to. Both columns of Table XII. purport to be costs for the *same* station load factor.

I submit that the right-hand column of Table XII. is of no value whatever, and should not appear in Table XIV.; also that Mr. Snell's column of "Correct Charges" for load in stations in Table VII. is probably vitiated by the same mistake. An example taken at random from actual results on a station will make my meaning clear.

Take the published figures for Poplar for 1906–7:—

Station load factor	24·2 per cent.
Average consumer's load factor... ...	7·95 "
Kilowatts connected	6,037
Kilowatts demanded at station	1,980
Diversity factor	3·05
Total cost	£30,800
Fixed charges	£20,950
Running charges	£9,850

$$\begin{array}{c}\text{Standing charge}\\ \text{(as at station)}\end{array} = \frac{\pounds 20{,}950}{1{,}980 \text{ k.w.}} = \pounds 10{\cdot}6 \text{ per kilowatt.}$$

Mr. Taylor.

$$\text{Standing charge (as at consumer)} = \frac{£20{,}950}{6{,}037 \text{ k.w.}} = £3{\cdot}47 \text{ per kilowatt.}$$

$$\text{Running charge (ex. wages)} = 0{\cdot}56\text{d. per unit.}$$

$$\text{Cost per unit (as at station)} = \frac{£10{\cdot}6 \times 240}{1 \text{ k.w.} \times 8{,}760 \times \frac{24{\cdot}2}{100}} = 1{\cdot}2\text{d. per unit.} \quad . \quad . \quad (1)$$

$$\text{Cost per unit (as at consumer)} = \frac{£3{\cdot}47 \times 240}{1 \text{ k.w.} \times 8{,}760 \times \frac{7{\cdot}95}{100}} = 1{\cdot}2\text{d. per unit.} \quad . \quad . \quad (2)$$

According to Mr. Snell, the second equation should have been—

$$\frac{£3{\cdot}47 \times 240}{1 \text{ k.w.} \times 8{,}760 \times \frac{24{\cdot}2}{100}} = 0{\cdot}4\text{d. per unit.} \quad . \quad . \quad . \quad (3)$$

So that the true cost to the consumer should (according to Mr. Snell) be $0{\cdot}4 + 0{\cdot}56 = 0{\cdot}96$d. instead of $1{\cdot}2 + 0{\cdot}56$d. $= 1{\cdot}76$d., the actual charge made to the consumer.*

I put it to the meeting what sort of a reception Mr. Bowden would have had from his committee had he come to them and said, "Gentlemen, our standing charges last year were £20,950, but through following the advice of a very eminent engineer I have only charged my consumers one-third this amount, and am in consequence £14,000 short."

It would be interesting to know whether the London County Council prices in Table XIV. were compiled on this basis. I submit that the author has in this otherwise excellent paper made a very serious mistake on this point.

Mr. Seabrook.

Mr. A. H. SEABROOK: There are one or two statements in the paper which I cannot reconcile, but which I have no doubt the author will be able to reply to at the close of the discussion. Turning first of all to the tables in Section I. of the paper, which give the ascertained cost of power per unit generated for independent plants of small and large size, it appears to me that in the last column the average costs are too low. Even allowing for the difference in the cost of coal between the North and South of England, it is contrary to the experience we have been obtaining recently; but supposing they are correct, I do not quite see how any power company will undertake to do much in the way of taking them over, because there is not much margin between the average cost of 0·696 with a load factor of 20 per cent. given in that table, and the figure given in Table XIV. sub-table (*b*) of 0·679. It seems to me there is something wrong there. If a big power station of 12,000 or 15,000 units cannot get a bigger margin or difference than 0·02, there must surely be some explanation for it. I have no doubt it is a

* *Electrical Engineering*, July 4, 1907.

Mr. Seabrook.

matter that can be easily explained. With regard to the details of the individual tests, they also do not support the figures in Tables I. and II., but they are nearer to the experience that we have had, correcting, of course, the cost of the fuel as between North and South. In Section III. we have our old friend—I am afraid he is rather an elusive friend—the £12 per kilowatt station. A good many of us have heard a good deal about him in a room not very far from here. I would like to know whether it is possible to build a station at that figure, or even at double the figure. The Newcastle Power Company have been credited with cutting these things as finely as possible; but as the author admits himself the cost of their stations is £33 per kilowatt, and as they do not appear to be extending their main power station, but extending it on another site on the Tyne, one would assume that they have found the limit of economical power stations. We have had no explanation of that. Of course that is an assumption. Then the figures for the cost of sub-stations appear to me to be high. Comparing a great many that we have constructed recently, the cost per kilowatt given for a static sub-station is quite double the cost per kilowatt of those constructed as it actually came out, and the same thing applies to the cost per kilowatt of rotary sub-stations. With regard to the rotary sub-stations, it will be noticed that the cost per kilowatt is a very large percentage of the cost per kilowatt for the main power station, which seems excessive. With regard to the space taken up by the static chambers, which is given as 0·8 sq. ft. per kilowatt, that seems very high indeed, because the best place for the static transformers is out of doors, where they can get plenty of ventilation, or in the roof of some building out of the way entirely.

With regard to Section IV. of the paper, which contains some criticism of the present London suppliers, I have no doubt, with regard to the author's opinion as to what is to be done for London, that later on some engineers will join in the discussion who have had a great deal of experience in this matter, and will put their views clearly before the meeting. Personally I do not believe that a large power station will solve the difficulty. The problem is quite a different one from that of taking over power stations in the provinces, supplying half a million to a million units per annum. But here we are dealing with power stations selling from 12 million to 20 million units per annum, and operating over small compact areas. For instance, the City Company has an area of 1 square mile. With regard to these tables giving the true costs of production for the various power stations in London and the provinces, Table VIII. is rather striking, which in its last column gives the average cost of production of some of the largest power stations in the country at various load factors. I have been very much interested in comparing the figures in that table with the figures in Table XI., which gives the average cost per unit for private installations. Apparently Manchester, Nottingham, Leeds, and Glasgow have a cost of production double that of a private installation of from 100 to 500 k.w. Newcastle, also, in Table XII. shows a considerably higher cost per unit than the small manufacturers' private installations. On page 313 the author gives a

Mr. Seabrook.

warning to station engineers, "That the future will bring, in all probability, a very large proportionate increase of low-price power units, and with improvements in lamps and their reduced energy consumption, the proportionate lighting units may even fall below their present figure. This all means that the average revenue per unit will be considerably lessened." I should like to ask, What does it matter if it is? If I sell a million units for lighting at 3d., and the next year I sell an additional million units for power on an improved load factor for 1d., my average revenue is brought down from 3d. to 2d. If I have calculated my charge for power correctly, there is no earthly reason why, because I have a lower average receipt per unit, I should have a lower net profit. Of course it is quite obvious that if the power units are incorrectly calculated it is disastrous. It would be disastrous in the station for which I am responsible, as I suppose for this year four-fifths or five-sixths of our output will consist of low-priced units. But I do not anticipate anything like the result the author warns us against, because I am convinced that the cost at which we have put the production of these low-priced power units is correct. Anyway, time will show. It may be said that our present balance-sheet is not a very brilliant affair, but that was due to other causes, not to reductions to power consumers, but mainly to reductions to lighting consumers, based on an estimated increase of consumption which was not obtained in the year during which reductions were given, but which is being obtained during the current year. Then there appears to me to be a greater difference between the three sub-columns of the first main column in Table XIV. than there should be. I cannot quite reconcile, for instance, on the 25 per cent. load factor, why an extra high-tension price of 0·508 rises to 0·583 with transformers. The very high efficiency of modern transformers and the small cost at which they are obtainable surely cannot cause such a difference as that. The author says on page 317, "If Table XIV. is compared with Tables I. and II., it will be seen that a power company can compete successfully with any one of these other prime movers if the power company supply transformed alternating current, but if the power company is constrained to supply direct current, then it is only smaller plants—that is, 100 H.P. or less —with which it can hold its own." Considering that the bulk of the small boroughs and the small installations consist of direct current, it does not seem to me that if the power companies can only take 100 H.P. and under, that the small boroughs with their direct-current power stations are going to receive much benefit.

Mr. Jeckell.

Mr. J. A. Jeckell: I am very much afraid that if Mr. Snell's figures are correct there is not very much chance for many of us station engineers inducing customers to take a supply of energy from us. If we refer to Table IX., we see that with a load factor of 25 per cent., Manchester is bound to charge 0·95d. per unit for energy for power purposes, Nottingham 1d. (perhaps there are reasons for that), Leeds 0·99d., and Glasgow 0·99d., the mean coming out at 0·99d. I venture to think that, though the consumer who has only about

Mr. Jeckell 100 H.P. may be willing to pay this price, the consumer who wants 500 to perhaps 5·000 H.P., will certainly not pay this price. There is no chance of his doing so, because he can manufacture it much cheaper for himself. I feel sure that somehow or other Mr. Snell has prepared these tables upon a wrong basis. I will take an extreme case, the case of the town with which I am associated. Supposing we go back to the time when Coventry had no power supply, when it had only a lighting supply. The cost of production, according to Mr. Snell, came out at over 6d. a unit. What chance was there for anybody to induce any consumer to turn out a steam engine or a gas engine if he had to charge anything like that price? Fortunately, possibly, I did not prepare my calculations in the same way as Mr. Snell has done. I gather that in Table VII., in arriving at the cost at which the various boroughs in London can supply current, he has taken into consideration the large amount of capital that they have already expended, and in considering the probable charge that the power supply company can supply energy at he has supposed that the power company puts down a brand new station with all the modern improvements. Mr. Snell does not debit against the cost of energy supplied by a new station the capital charges on the old stations. The position I took up was this. We had a lighting station; we put beside it a power station, but we did not propose to charge the power consumers the capital charges which were incurred on the lighting plant. In other words, supposing, let us say, a manufacturer takes over a business for the manufacture of cycles, on which he finds £100,000 has been spent. It is not making a profit. He has plenty of ground, so he decides to put down by the side of the existing buildings some more buildings, using the same offices, etc., for the manufacture, let us say, of motor-cars. Against the price that he is going to charge for his motor-cars he does not propose to debit the capital charges on the money spent on the cycle machinery; he debits against the new undertaking only the capital charges which are incurred on that new undertaking. If he makes money on the new undertaking he can depreciate if he pleases the plant in the cycle part of his premises which has become, to a certain extent, obsolete. To put the thing the way in which it has come out in actual practice, as a matter of fact, we had at Coventry, we will say, £75,000 spent on a lighting station, where the cost of production came out at over 6d. a unit. We spent another £75,000, and the cost of generation by the new plant comes out at 0·89d. per unit. For this we obtain about 1½d. a unit. The combined result of the two separate departments means that the actual cost of production comes out at 1·63d., which happens to be less than the price we obtain for what we are producing. But had we started on the supposition that our power consumers had to be debited with the capital charges on the old lighting station, then we never could have offered to supply power at a price which I venture to think the ordinary consumers would have taken it at. I think it is this one point which makes Mr. Snell's tables, I hardly like to say unreliable, because they are extremely reliable, but let us say mis-

Mr. Jeckell. leading; I am afraid that many station engineers will not be able to obtain customers at the prices at which they are figured there. There is only one point which the author has, I presume, intentionally omitted to mention, and that is, however desirable it is to reduce the capital charges on a station, it is equally necessary to reduce the manufacturing charges or the station charges. I think we shall find that, if £10 per kilowatt more is expended on a station and there is a load factor of 22½ per cent., which is a convenient one simply because it means that one sells 2,000 units per kilowatt demanded at 7 per cent. capital charges, the extra cost per unit owing to the £10 extra capital means 0·1d. per unit sold. Taking the published Board of Trade returns, it will be found that between stations of somewhat similar size there is a difference in the station costs of 0·3d. or 0·35d., which means that the one station could have spent £35 per kilowatt more than the other, and yet the total charges, the station charges, plus capital charges, would have come out the same. We have also to remember that we cannot reduce the capital charges of an existing station unless we can put down another station by the side of it, but we do know that we can start and reduce the station costs possibly without a single penny of expenditure. I do think that if we are to sell power and obtain a large demand for it, we have first of all to put our house in order, not only as regards capital charges on our new plant, but as regards station charges on our existing plant. If we are to obtain customers, if we are to compete against various other prime movers, such as suction gas and gas sold by local authorities, at, I am sorry to say, in my own case the actual cost of production, without any charge for capital added to it, we are compelled to sell at a very low figure; and if we are compelled to sell at a very low figure we are compelled to manufacture at a very low figure; and if we are compelled to manufacture at a very low figure we have to look to those two points, not only the capital charges, but the station charges. I think Mr. Snell must really have made a mistake, as one other speaker has already stated, in the cost of the static sub-stations. In Table VI. he has stated that with the capacity of a sub-station of from 100 to 250 k.w. the cost of the static sub-station per kilowatt is £3·33. I am sure that they do not spend so much money as that in the North of England on those sort of things. £2 per kilowatt will well cover it. It can be checked in this way, that in our outside area, which fortunately Parliament has allowed us now to supply—we go into the power company's area and the power company does not come into our area—we are proposing to give a supply by means of kiosks with transformers in them. The whole cost of two 50-k.w. transformers in a kiosk, that is 100 k.w. altogether, is £200, which comes out at £2 per kilowatt. I think therefore there must be some special reason for this £3·33 per kilowatt.

Mr. Patchell. Mr. W. H. Patchell: Like Mr. Rider, I have now no axe to grind. However much we think that Mr. Snell is skating over somewhat thin ice in some parts of his paper, I most cordially

Mr. Patchell. agree with him at the end, where he says that he wishes to awaken engineers responsible for town installations to the advisability of co-operation rather than extending small stations. That was the creed pressed in 1905 by Mr. Fladgate, the chairman of my old company, and I think if it had been loyally worked to by the companies and local authorities interested we should have settled this power question once and for all in London long ago. In some places Mr. Snell has dropped into figures which are very difficult to follow, so I would rather deal with a few general points. First of all I will deal with the question of the cost of supply, to which Mr. Snell has referred. Some people have put a great deal of money into their stations and mains with the intention of giving a supply, not only within the Board of Trade limits of variation of pressure, but considerably less. I lately saw this question put in all seriousness, "What lamps had I best use when my pressure varies from 90 to 115 volts?" That was not put by a London engineer. On a varying voltage the metallic filament lamps score. Many of the public supply mains, which now give an indifferent supply for carbon filament lamps, would give a very good supply for metallic filament lamps, and the restriction of the Board of Trade at the present time with regard to the variation of pressure, which is on the carbon filament basis, might be modified to suit metallic filaments with advantage to the supply companies and their clients. On page 291, in the table of small stations, I do not quite see how one can fairly look at the cost of a station on the same basis, whether it is for 1 H.P. up to 100 H.P., or, in the next table, from 100 H.P. up to 500 H.P., because the cost of labour affects the question so largely. On the same page Mr. Snell, after spending much time on arithmetic, states, "There is no proportionate charge for supervision or general establishment charges, or for rating." Really that discounts the value of those figures materially. If I may say something more about that table, I would suggest, "What is the use of averaging the cost of steam, oil, and gas?" I do not quite see the value of that last column, because one does not run a three-line engine, one on steam, one oil, and one gas. One must decide which to have before starting! If that last column had been filled by a statement on a power company scale, such as £6 per kilowatt and a farthing a unit, or some such scale, one would then have something definite for comparison, and we would have seen how the load factor affects the cost when buying on a sliding scale. It is very unfair, I think, to speak in the same breath of the cost of a private plant which only runs 54 hours a week and the cost of supply from a station which gives a 24 hours a day service. There have been very tempting offers made by people who have been suggesting that they can put in gas engines and give a supply for "half nothing," which was much cheaper than the price at which the local authority would give the supply. The poor man who is having such an offer made to him never dreams that the supply would only be for 8 or 9 hours a day, and that there may be a considerable extra charge if he had to run overtime. On page 293, in the gas-engine costs, Mr. Snell

Mr. Patchell.

gives the horse-power in connection with the pumping station, but in the previous case he does not give it. The reason that the work is being done more economically electrically than by means of gas may be because the plant chosen is better suited for the work. On page 294 Mr. Snell raises a very interesting point, where he says, "The plant was under the care of engineers and an engineering firm, and this high cost can only be ascribed to the fact that the plant was not so well maintained as it should have been." We have heard that the shoemaker goes the worst shod. I heard of another case lately where two brothers, one a doctor and the other an engineer, bought identical motor-cars. The doctor does not know how his motor goes; he winds it up, it runs, and it is available day or night. But the engineer's car, on the other hand, is never out of the repair shop! So if we compare the cost of those cars on the theory that Mr. Snell suggests we get exactly the opposite result. On the question of paper mills there is something said about the steam heating being kept separate. I think it is sometimes misleading to take the figures given by people who use a very large quantity of their steam for boiling and a very small proportion for power. If we wish to tempt a hotel manager to shut down his private plant, we have to argue it with him, and he always suggests his kitchen costs practically nothing for steam and his heaters are run on exhaust steam! But if he shuts down his electrical plant and gets his supply from the company he begins to realise what his kitchen really costs him. I think a good deal of the disappointment the power companies have experienced has been due to the ridiculously small amount of energy which is taken to run some works. When the power schemes were first thought of in London the horse-power in boilers was converted into horse-power in machines, and that, I believe, was gravely used as the basis of the horse-power available for electrical driving in London. Certainly some works do take a ridiculously small supply. I have some figures here of a wagon shop which is turning out 500 wagons a year. There are seven motors in that shop of an aggregate 150 H.P., and the units used per month per horse-power installed are only 17. In a brewery with 16 motors, aggregating 167 H.P., the units used per month per horse-power are 28. Colliery work is mentioned by Mr. Snell, and in an instance I have there of 9 motors, aggregating 1,060 H.P., 60 H.P. of which is in two 30-H.P. pumps, the units used per month per horse-power are 36. Another case which will make the mouths of London "undertakers" water is an 85-H.P. motor driving a fan which takes 580 units per horse-power per month. This last is a special case even for colliery work, but when one gets into the manufacturing trades the units used for driving the works are often surprisingly small. With regard to such data it might be useful to members who wish to follow the question further to refer to E. W. Lloyd's Report to the National Electrical Light Association on the "Purchase of Electrical Power in Factories." It was published by the National Electric Light Associations Convention in June, 1905.

Discussion at Meeting of January 16, 1908.

Mr. Sparks.

Mr. C. P. Sparks: There is only time to touch upon a few of the important points raised by this paper.

Taking Tables I. and II., dealing with the cost of independent supply in the first case up to 100 H.P., and in the second up to 500 H.P., Mr. Snell puts a new position before us. For some years

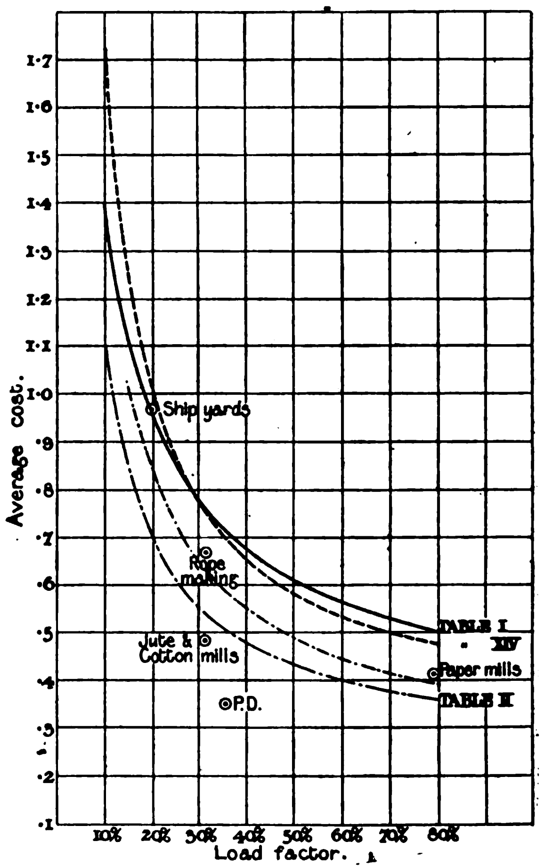


Fig. D.

past it has been almost recognised as an axiom that the most economical method is to take a power supply from a large bulk station, and the figures the author now puts forward, until they have been carefully examined, appear to show that small independent plants can produce a unit at a lower rate than it is possible for a large bulk undertaking to supply it at. I have plotted on a diagram (Fig. D) the

author's figures from Table XIV., column II. (*a*), which give the suggested prices for supply of alternating current to power consumers; and from column IV., giving the author's estimated prices at which the Newcastle Supply Company are supplying in bulk; and I have filled in the load factor and price for individual plants from Tables I. and II. From this diagram we see that the proposed "bulk" scheme could only supply consumers up to 100 H.P. when their load factor is above 30; it can touch no other class of business at all. Taking the Newcastle figures, we find that, while they could supply consumers up to 100 H.P., they cannot supply the consumers between 100 and 500 H.P. at any load factor. I think, although Mr. Snell has spent a great deal of time and pains upon Tables I. and II., that careful examination will show they are inaccurate. They are, in my opinion, the kind of costs that one gets with plant in perfect order. It is difficult for the ordinary power user to devote sufficient attention to seeing that the possible qualities of the plant are always at his disposal, and consequently the cost of production given in Tables I. and II. is too low for the average case. In the next place, an important figure is missing, and that is the figure for obsolescence. How many of these private plants put in ten years ago are to-day in such order that the people using them can produce at anything like these figures? Independent plants are constantly being replaced by new plant or by public supply. I think when these two factors have had consideration that the prices in Tables I. and II., so far as the smaller users and the average users are concerned, must be put on a much higher level; otherwise it appears that the supply business, whether by public authorities or by power companies, is entirely on a false basis. Mr. Sparks

The second point I wish to make is in connection with mill work. On page 297 the author gives some figures as to jute mills. I have information before me with regard to similar practice both in this country and in the United States. I agree with the author in his estimate of the gearing loss. In well-arranged modern mills driven by steam engines I think the figure of 30 per cent. for gearing loss is, if anything, on the high side. My experience goes to show that it varies from 25 to 30 per cent. The advantage of electrifying a mill, so far as increased gearing efficiency is concerned, is small. There may possibly be some saving, but as a general rule it is not practical to drive the actual textile machine with individual motors. In consequence the saving in the cost of power when electrifying a mill is small. But there are other reasons for electrification, namely, the question of the smoother drive, the fact that the power is delivered close to the machine, and that there is less slip, which allows the driven machinery to give a greater output. It is for these reasons that electric driving is growing, and not from any question of the saving in cost of power. From my experience I think that figure of 0·48d., or something near it, is the kind of result that can be obtained by independently driving a mill of this size.

With regard to collieries and larger power users the author has extracted some details from the paper I read before this Institution, which

Mr. Sparks. show how difficult it is for a power company to supply an independent undertaking requiring a large power. On this point I would like to refer to Fig. 1, which deals with the question of the capital cost per kilowatt. At the side it will be noticed that the first figure stated is £20 per k.w. While £20 may be a fair figure for a small undertaking, in the case of a concern having from 1,500 to 3,000 k.w. of plant installed, the cost ranges between £13 and £11 per kilowatt. In consequence, an examination of Fig. 1 will show that there is hardly any chance for a power company supplying a large consumer, except at a very limited radius. In the case of a colliery, the coal cost, which is plotted on that curve at 11s. a ton, is a further difficulty in the way of the power company, and in my opinion Fig. 1 shows it is almost impossible for a power company to supply a really large colliery undertaking. With regard to Table III., I am surprised to see the small number of units put down per horse-power. The author takes as his basis the units used per head. I think the basis of units per horse-power is in every way preferable. Just before Table III. the units sold in Sunderland are given as 404,999, with 587 motors of 1,634 H.P. That only works out at 250 units per horse-power per annum. There must be some reason to account for this. Possibly the motors were not connected for the whole of the year. From figures from various parts of the country I am quite sure that 250 units per horse-power is exceptionally low.

Table VI. gives the cost per kilowatt of power house and distribution system at £13 and £14 5s. respectively. With regard to the cost of distribution I think the author must put forward a definite scheme before he can make this point good. The figure suggested for distribution capital cost is unknown in this country and at the moment only exists on paper. The Newcastle Company's distribution capital cost is given by the author at £26 per kilowatt, and any attempt to halve this figure would be found most difficult.

Diversity factor is dealt with on page 306 of the paper. I regret that there is no time to deal fully with this point, as I consider that the author has credited too much of the advantage to the power house and transmission system, and too little to the sub-station. This discrepancy has an important bearing on the power house and transmission cost.

Table IX. deals with the estimated cost of production of the larger municipal undertakings. These municipalities are supplying at lower figures than those set out in the table, and if they looked at the matter from the point of view of the author, they would see the bulk of the new business pass them. Fortunately in most cases they have considered what the additional capital and running costs would be for supplying the future additional output, and have made a profit by supplying at lower figures than those suggested by the author.

Before closing my remarks I would like to say a word with regard to the lighting business. On page 313 Mr. Snell writes : "But a warning should be noted by station engineers that the future will bring, in all probability, a very large proportionate increase of low-

price power units, and with improvements in lamps and their reduced energy consumption the proportionate lighting units may even fall below their present figure." I disagree with that statement. Had we been limited to the carbon filament lamp there might have been something in it, but, fortunately, the metallic filament lamp has passed the experimental stage. My experience of this lamp during the last year shows that the decreased cost of lighting has at last put in our hands an effective weapon with which we can fight our competitors. It is true that the demand made by the lighting consumer will fall considerably, but the units will not fall proportionately, and that is the important factor. The fact that we are in possession of a lamp which gives us, roughly speaking, three times the candle-power per unit has provided us with a far more effective weapon than if we had been able to obtain a bulk supply at a much lower rate than anything yet spoken of. I think the lighting question is going to be of ever-increasing importance. There are hardly any of us who have done more than scratch the surface of possible business. Mr. Sparks.

Mr. R. HAMMOND: The problem of the cost of electrical energy for industrial purposes is one with which Mr. Snell is very well qualified to deal. This paper will, I think, be remembered as one which has brought prominently before this Institution the very great effect of diversity factor in the cost of electrical energy when supplied from central stations. For years we used to dilate upon the best means of encouraging long-hour consumers. As we were only dealing with one class of consumer, we deemed that to be almost the only way of improving the load factor. Now with the supply of power to varied industries, electrical engineers are agreed that the diversity factor has as much effect upon the station load factor as long hours of use, since the station load factor is the product of the average consumer's load factor and the diversity factor. I should like to draw attention to the marked way in which the diversity factor affects the cost to the consumer. Let us take, for instance, an illustration, that £7 per kilowatt of the station maximum load is found to be the sum that is required to cover all the standing charges. Now it is obvious that with a diversity factor of 1·66, the charge to the consumer to cover those standing charges, instead of being £7 per kilowatt, is reduced to £4 2s. There is, then, a saving of £2 16s. per kilowatt, and on about a 20 per cent. load factor that figure yields an allowance to the consumer of about 0·35d. per unit. That is the figure of which he gets an advantage by drawing supplies from a central station, and taking the full benefit of the diversity factor as compared with the cost if that diversity factor did not exist. It must, however, be borne in mind in speaking of the 0·35 or any other figure with which the consumer may be credited, that giving each consumer the same allowance is obviously unsound, because a consumer, for instance, whose maximum load coincides with the top of the peak does not help the diversity factor at all; on the contrary, he injures it. There is a worse diversity factor when he comes on than before he came on. Mr. Hammond.

Mr. Hammond.

Instead of having an allowance he should absolutely be charged more than the figure which represents the standing charges of the station. He should be fined for his misdemeanour! The time may come possibly when a scientific scale will be thought out by means of which each consumer will receive that allowance which his effect upon the diversity factor warrants. That may seem to be a prophecy or an aspiration that is not likely to be fulfilled, but I would remind members that at the present time it exists as far as the load factor is concerned, and I hope that at no distant time our scale of charges will be such that it will take into account the diversity factor as well. I have said the diversity factor has a very marked effect upon costs, and that being so, one might suppose that it would go without saying that it would be best for all consumers to obtain their supplies from big stations where the full effect of the diversity factor was felt; especially when, be it remembered, in the case of big stations there is the greater advantage of the saving in the capital expenditure on much larger plants, there is the saving in respect of more efficient plant, and there is the saving in wages and in management. In the case of London there is also the advantage of being able to choose a site on the river, where there is ample water for condensing, and where steamers with seaborne coal can come alongside. Before, however, taking it for granted that the argument in favour of centralisation is unanswerable, one looks at Mr. Snell's paper and one is somewhat staggered, as Mr. Sparks hinted, at the very low costs which are named as those obtainable by power users having their own plant. I am rather afraid that Mr. Snell, with his usual desire to be extremely fair to the other side, has looked at these figures far too leniently. I hope that in his generosity he has not parted with the family inheritance, for I am one of those who firmly believe in the central-station idea. I should like to draw attention to one or two points in the figures set out in his tables. In Table II., which deals with loads from 100 to 500 H.P., the price which Mr. Snell sets out as the figure for a load factor of 25 per cent. is 0·59d. I certainly would like him to tell us at the end of the evening whether on reflection he is still willing to stick by the 0·59d. as the average price at which a power user with such a small load as 200 k.w. can produce his own electricity, if he takes into account all the charges that should be included therein. I am rather inclined to think that Mr. Snell will feel that his generosity has carried him a bit too far, especially when I find in the paper an instance of what a particular power user finds that his electricity does cost him. While 0·59d. appears in Table II., 0·95d. appears on page 295 as the actual cost of a particular installation with a maximum load of 504 k.w. Then also we can check Mr. Snell on the 35 per cent. load factor, the cost for which as estimated in the paper is 0·494d. We remember Mr. Williamson's paper, of which the author has made good use. He quotes from Mr. Williamson's paper, and he shows that Mr. Williamson, with a load factor of 37 per cent., has an actual cost of 0·716d. Why 0·494d. in the table

if Mr. Williamson finds, with all the great advantage of the low costs of operation at Vickers, Sons, and Maxim's works, that it costs him 0·716? I will take it even a step further and point out that in Tables VIII. and IX. Mr. Snell deals with the actual cost to the big power undertakings of the country which are supplying in very large quantities, quantities ranging from 3,000 to 23,000 k.w., which is now the power supply in Manchester as well as in Glasgow. When we turn to those figures we should expect to find a very great improvement upon the 0·59d. Not at all. In the case of Leeds, an installation with which I am particularly familiar, the cost is set down at 1·37d. Why this? If a private consumer with a 200-k.w. plant can produce a load factor of 25 per cent. at 0·59d., why should not a large central station with thousands of kilowatts be able to produce at less than 1·37d.? Can it be said that the mere moving of a plant into a central station at once doubles its cost of working? Even the strongest opponents of municipal trading will hardly say that! Of course I am aware that the 1·37d. covers a proportion of the standing charges due to the distributing mains, and to the extent to which the mains are used for the power supply the 1·37d. must include that figure. But in Leeds the mains cost about £24 a kilowatt upon the basis of the diversity factor of 1·66. Ten per cent. on that, taking the author's own figure, is £2 8s. £2 8s. on a load factor of about 25 per cent. is about a farthing a unit, and that is the sum included in the 1·37d. as represented by the mains. If the 1·37d. were correct, what would become of our Leeds business? How is it that in Leeds, a great industrial centre where coal can be obtained at a cheap price, we supply such a big firm as Kitson's? How is it that we have a big concern on the mains whose consumption runs into two million units? There must be a little error somewhere, but it is not on Mr. Snell's part. He has faithfully transcribed what power users have given to him as their costs, but I never knew a power user yet who did know what his costs were. The power user in making up his costs adds the coal and the wages and makes a wonderful sum, but he omits the proportionate standing charges. He does not count this and does not count the other. Mr. Hammond.

The President: What are Kitson's charged? The question is not what are the costs of the station, but what Kitson's are charged. The President.

Mr. Hammond: But surely what we are discussing is cost of production, not the selling price. In reply, however, to your question I am not free to name the price to any particular consumer. You will understand the reason, but there is no consumer of power in Leeds who is charged less than 1d. a unit. That is the lowest price. At 1d. a unit we are supposed by this hypothesis to be supplying people who can make a very much smaller quantity at 0·59d. I believe that the reason is that to this 0·59d. must be added many charges which do not appear upon the surface. The author and I entirely agree on this, that it is not only the question of cost, but the immense convenience there is in getting the supply from outside works which produces a better price to the central station. When a big manufacturer has not to bother his Mr. Hammond.

Mr. Hammond. head about engines or dynamos there is a great weight off his mind, and his works manager is able to devote himself to the real production of the product which he has for his own specialty rather than to the production of power. Another point is that it is possible to increase a plant very much more easily when the power is obtained from the central station.

I will conclude by saying that this paper comes at a most opportune time, as it is the prelude to a discussion before the Committees of the Houses of Parliament in the coming Session, which will, it is hoped by some, finally settle the important question of the power supply to the largest industrial centre in the world.

Mr. Cowan. Mr. E. W. COWAN: Mr. Snell apparently holds the view that a differential charge as between lighting and power consumers, which is

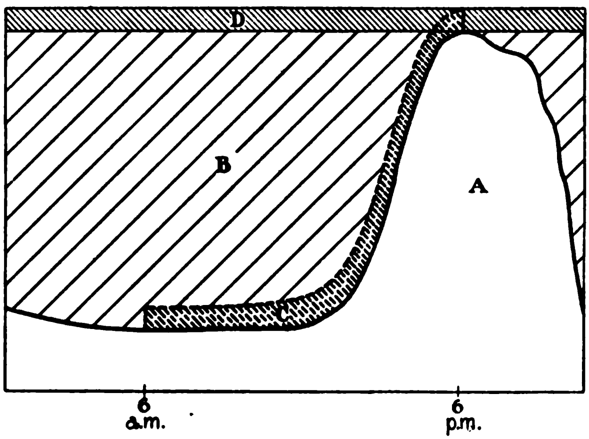


FIG. E.

not based upon the effects of their demand upon the load and diversity factors, and consequently upon the cost of production, results in supplying one class of consumers at the "expense" of another (see particularly page 308 of the paper). Mr. Rider, in opening the discussion, expressed the same view, characterising any departure from such a principle as unfair. I submit that a differential charge may be made independently of load and diversity factors, which does not necessarily involve any injustice as between one class of consumer and another. I consider that the true test of whether the commercial administration of an undertaking upon the lines condemned by Mr. Snell and Mr. Rider is justified or not is to ascertain whether the result of the combined business shows a better return upon the capital invested than if their principle be adhered to.

In the accompanying figure (E) I have drawn a load diagram. The area A may be taken to represent *active fixed capital*, and the shaded

area B *inert fixed capital.* This shaded area represents the by-product of the works in that its existence is incidental to the principal product of the works. It may be compared to the by-products of gas works, such as coke, tar, etc., and it follows that it will pay to sell this by-product for whatsoever it will fetch. It differs in kind from the gas works by-product because it is not a complete production, but only one element of it. That, however, does not affect the principle involved. It must be noted that the supply of this by-product is limited, and its sale at by-product prices must be correspondingly limited. Mr. Cowan.

If the diagram be turned the other way up and the area A be shaded while the area B represents the load, the two diagrams will depict an ideal distribution of load, because the diversity element of the second load is such relatively to that of the first load that 100 per cent. load factor will result.

Such a load being unobtainable, let us suppose that an applicant for power supply comes along and that his demand is represented by a uniform load from 6 a.m. to 6 p.m., as shown by the area C. Our inert fixed capital area must now be increased by the rectangle D. It will be noted that, the effect upon the works diversity factor of this customer being *nil*, the total area of inert fixed capital is greater than before, because the area added is greater than the area utilised or rendered active. The proportion of shaded area is now, however, less than before, or, in other words, the load factor is improved. Consequently it would appear that the principle of charging such a consumer according to the effect of his demand upon load factor and diversity factor is sound, and, so far as it goes, I agree that it is; but it appears to me that it does not go far enough in that it ignores another factor, namely, *the intensity of demand.* By intensity of demand I refer to the variation in the appraisement of the value of the supply by different classes of consumers. It might be called the utility factor, but whatever it is called its existence has an important bearing upon the commercial results of an undertaking. The utility of a unit of electricity is greater to a lighting consumer than it is to a power. This difference in utility is expressed by the price each class of consumer is ready to pay. He will take care that there is a balance of satisfaction in his favour or he will not trade.

Factors that affect the cost of production do not affect the intrinsic qualities of a unit of electricity, and consequently do not affect its exchange value. A charge should be examined with reference to cost of production, but fixed at the maximum which will result in a sufficient trade. Otherwise capital will be deprived of a portion of the earnings to which it is entitled. The intensity of demand can be stimulated by advertisement, and is weakened by competition. I contend that it is rational to base all charges to consumers, as far as may be practicable, upon the basis of the intensity of their demand. By dealing in more than one market at varying prices the best return upon the capital can be obtained without injustice to any one.

In an abstract sense a *justum pretium* is more nearly secured when

Mr. Cowan. charges vary according to the intensity of demand, as the fruits of a new industry are thereby more equitably distributed. This standard is unattainable in practice, but an approximation should be sought after.

As time goes on the fruits of progress fall into the lap of the public. Competition, compulsory purchase clauses, conditions in provisional orders, etc., provide for this, but in the early stages of a new industry the natural profits should, according to right principle, accrue to those who have contributed and risked their capital. Our industry has conferred a great benefit upon the community, but capital and enterprise have been insufficiently rewarded in the past.

Mr. Addenbrooke. Mr. G. L. ADDENBROOKE : It is a very difficult matter to discuss this paper at short length. It divides itself really into two problems: one the supply of London, and the other what we may call general supply or supply in the provinces. In the first place we must all thank the author for the results he has given us of the costs of customers' local installations. This is one of the most important things to know in power work, and it is one on which it is surprisingly difficult to get thoroughly satisfactory data. Most of those who are engaged in power work have accumulated a certain number of such results, but they are mostly given by manufacturers in confidence, and are therefore not available for publication. I must confess, like Mr. Hammond, that I do not quite understand the basis upon which Mr. Snell has worked out his first two tables. Has he taken 100 H.P. and 500 H.P. as his basis, and said that everything under that must necessarily cost more per unit ? or has he made an average ? I should like to know this because it does not seem to me that there is any real basis upon which one can make an average, since the number of engines of different horse-power and different size does not correspond in practice, as there will be a much larger number of the smaller sizes as compared with the bigger ones. I can go perhaps a little further than Mr. Hammond on the question of the cost of working a 500-H.P. installation. Besides the first example that Mr. Snell gave, he gave another one of 1,000 H.P., in which he says the plant was modern, worked at about 500-k.w. load, with superheat and vacuum, and I think 24 or 25 per cent. load factor. In that case the total costs come out in practice to 0·71d., whereas in Mr. Snell's Table II. for the same load factor he gives, as Mr. Hammond said, 0·59d. My impression is that 0·71d. per unit is nearer the best practice for a local installation, and I think it is nearer than the 0·97d. mentioned. In dealing with all these matters of power supply there is a point which has not been alluded to by previous speakers. Such tables as the author's may be quite correct in themselves, but if a considerable power area is considered, there will be found a certain number of new engines, which represent a comparatively small proportion of the total power of the area, and there will be also a certain number of engines which are old and very uneconomical, and easy to replace; but English manufacturers are not so behindhand as some people would make out, and a very large portion of the plant throughout the area

will be in engines which are fairly economical, according to present practice, and in good working order. If a manufacturer whose plant is of the latter character is approached with such calculations as the author has put before us, he at once objects to the charges for the interest and depreciation on his plant, saying that if he scraps his plant and puts in an electric supply he will get nothing for it, or a very small sum. He therefore takes up the position: "I may as well write off that sum anyhow. Some day my engines will break down, and I will come to you then, but in the meantime it will not pay me unless you can supply me at a figure comparing with my present working costs and actual repairs." I do not say that that is a correct position, but it is a position which in the past has been taken up very largely by manufacturers, and it has accounted in a considerable measure for the progress of the power companies being slower than many people anticipated. Of course, this is not an argument against power companies, because sooner or later that plant will break down, or some alteration will be required, and the manufacturer will come to the power company. As regards colliery work, in the discussion on Mr. Sparks's paper I gave a table of the prices at which I considered a large power company could supply within a moderate distance, and on that basis the power, even at such a radius as Mr. Sparks was talking of, could very well be supplied from a 20,000-k.w. station; indeed, I think the matter is fairly evident from the fact that even in Mr. Sparks's case there were very considerable lengths of transmission. If 2 or 3 miles are added to the distance of transmission the extra capital cost per kilowatt involved will be inconsiderable, and if a circle is taken round the power station again it is pretty clear that, at any rate within that radius, say 6 to 8 miles, power can be supplied at the same prices as Mr. Sparks quoted, provided one can set to work without having incurred too heavy capital charges in obtaining Parliamentary powers and securing the necessary finance. These are handicaps which are difficult to get over, to a certain extent at any rate, in the early stages of power supply, and it is almost impossible to prevent their forming a serious proportion of the total capital; this is, however, a difficulty which will diminish with time. The next point is that people talk about a power company as an entity, but what a power company can do depends almost entirely on what size it is. For instance, a power company with a load of about 3,000 k.w. requires, in an ordinary industrial district, to obtain an average price for what it sells of about 0·8d. for the ordinary industrial load factor; but if that power company can increase its load to something like 60,000 k.w., which many of the industrial centres would warrant, power could be supplied with just as good a return to the shareholders, and with as large a margin for depreciation, at about half that figure, namely, about 0·4d. per unit; and for larger load factors, say of about 80 per cent., at about half those prices. This is the real fundamental fact behind the principle of electric power supply; only in one case in this country has a power company so far got on what I consider

Mr. Addenbrooke.

Mr. Addenbrooke.

to be a real working basis, though other companies are fast progressing towards it.

Following on this there is another point which needs to be carefully borne in mind in considering what consumers' plants can or cannot be economically replaced by central-station supply. To replace economically good modern plant within, say, 5 to 6 miles of the generating station, it is a good rule that the consumers' plant should not be of above one-tenth the capacity of the generating station. Thus from a 5,000-k.w. station consumers with plants up to 500 k.w. can be offered prices which will tempt them to change. On the other hand, a 10,000-k.w. station can generate more cheaply and can quote prices which will make it commercially practicable to replace local plants up to 1,000 k.w. Of course, such a rule must have many exceptions in practice, but it is a good one to bear in mind in considering what proportion of the power in an industrial district a given sized power station can compete for commercially, and what will be its prospects as it grows in the future.

Mr. Wordingham.

Mr. C. H. WORDINGHAM: I think we are very much indebted to Mr. Snell for attacking this subject in an unbiassed way. We have heard so much about power companies, and are used to hearing so many fallacies in connection with them, that we are apt to believe those fallacies. Most of the figures that we hear talked about, I think, originated in the committee rooms of one or other House of Parliament, and they related really in large measure to prices which it was proposed to charge for a portion of the cost to the consumers rather than to the cost of generating the energy. Mr. Snell's paper bristles with figures. My experience is that arithmetical figures, like other figures, are not always "what they seem," and it is usually futile to discuss them; there is always something left out of account which vitiates the most careful calculations. I think this subject is really best dealt with on broad lines. There is one point which I should like to call attention to, and that is the expression "diversity factor" which has crept into this discussion. I do think we are very apt to pick up a phrase and use it without asking ourselves what it means, and that, I think, is particularly the case in connection with this diversity factor. How many central station engineers really know what their diversity factor is; how many know what is the maximum demand of all their individual consumers? Unless they know that, they do not know what is the diversity factor of their station. It seems to me that in this discussion there has been some confusion on this point. What meaning can be attached to the "diversity factor of a consumer"? That was mentioned by one of the speakers, and I confess I did not understand it. I cannot help thinking that a great deal too much is being made of this diversity factor. The important thing is the load factor. Undoubtedly the diversity factor does affect the result—one would be far from denying that—but there is a diversity factor applying even in a small works, and I very much question whether the diversity factor accounts for anything like as much as is made out in these power company stations, of which one hears so much and of which one sees so

little. I should like to say one more word in conclusion, and that is, that I think essentially a power company depends upon the kind of town. I cannot conceive that the tacking together of a number of different towns, very similar as they are and must be in the areas of many of these power companies, can possibly improve the diversity factor, or whatever it may be called, to any material extent. Where the full gain is realised is in a rather large town where the central part and the suburban and residential part can be included without having to go too far for the load, and without having too great a load at the outer circumference. I think in London the greatest possible mistake is made in looking upon the matter as a new problem to be solved. The fact must be taken into account that so many authorities and companies have already sunk their money, and the total cost, including the money already spent, must be considered when reckoning the cost of supplying London in the future. Mr. Wordingham.

Mr. W. H. Booth: The author refers to the question of heat, which it seems to me is one of the greatest difficulties in the way of supplying power. I have with me some particulars in reference to an Oldham mill. I would like to ask Mr. Snell for some information about this question of main engines and auxiliaries. I think he puts down too high a percentage of the main power for an ordinary factory in the way of auxiliaries. Cotton mills, to take an extreme instance of a heat user, are heated both night and day, including Sundays, almost all the year, excepting a few hours in the summer, when during the day-time it is really hot weather. Those hours, however, are comparatively scarce. Of the whole of the calorific power of the coal burned for power purposes, about 10 per cent. is converted into heat by the machinery, so that the mill is not only heated by its own machinery, but also by the steam pipes specially supplied for the purpose. If an Oldham mill spinning twenty-eights to thirties be taken with 70,000 spindles, the I.H.P. will be about 1,000. There will be three boilers, 30 ft. by 8ft. 6 in., two of which will be at work, and they require one fireman. If heat must be supplied to that mill, there is still required that fireman, and there is still required an engineer, and that makes the cotton mill a difficult mill to supply with power. The coal required to heat that factory in winter is from 12 to 14 tons a week. That heat has got to be supplied from somewhere. At the Acme Mill at Pendlebury there are two mills close to one another. One is electrically driven by current supplied from the station at Radcliffe, and the other mill is driven by the ordinary method and supplies heat to the electrical mill, so that this latter does not feel the inconvenience of the heat supply. It is a moot point in Manchester as to who is losing on the contract, whether the suppliers of the electricity or the users of it. But it is claimed in favour of the electrical method of driving that the percentage of output is considerably greater. In Manchester they claim it is 7½ per cent. for electrical driving, so that that will pay something like £500 a year extra profits in a 10 per cent. concern. The coal used in these Oldham mills runs from 1½ to 2 lbs. per horse-power-hour, and in winter from 1¾ to 3 lbs.; that is to say, Mr. Booth.

Mr. Booth. the summer coal consumption is from 37½ to 50 tons, and the winter, from 43¾ to 75 tons; that is about 2,500 tons a year, which at 10s. a ton comes to £1,250 for 1,000 H.P. That does not agree with the author's figure for coal. There is another mill in Manchester belonging to some fine spinners, and I am told they are about to take 2,000 H.P. from the Manchester Corporation Stuart Street Station, and they are to pay considerably less than ½d. per unit. I do not know whether Mr. Snell's figures warrant such a price as that, but it seems to me if it pays them to supply at less than ⅜d. per horse-power-hour Mr. Snell's total cannot be altogether right, because he shows very much higher figures for Manchester. I think the whole point with regard to electrical supply will turn upon treating the public in a very ordinary sort of way. If a man goes to insure his life with a company, they do not tell him that he is the first man in that day, and that he may die to-morrow, and therefore they must charge him £400 for a £500 insurance. On the contrary, they thank him, because if he dies the next day it will cause all his friends to come in and join. In power supply we are always very apt to think that the man who has a poor load factor of demand must be charged heavily. The result is that instead of giving him a flat rate of 2d. we ask him to pay considerably more—in one case about 8d.—and ever so many limitations are put upon him. The author refers to these limitations; he advises some people to spend twice as much time in docking a ship. If we are to supply current we must offer everybody the current at a low rate, and we must get the thousands in. If we get the thousands in we shall get a level load at the generating station. We ought to ask ourselves, first, Is there a large demand to be supplied? secondly, What price will the thousands of users pay? Then if satisfied there are the customers, we should start business just like the baker opens a shop and sells loaves to all who come, irrespective of what time or how often they come. If a selling business will not pay on these business lines, is it worth while embarking capital in so speculative a venture? The public always does object to the imposition of special charges to cover the inabilities of supply tradespeople. I would, of course, always differentiate light from power. Light supply is practically determined by the sun, and price of current has thus a natural law to determine it. Electricity will always command a sale as a lighting agent so long as people live in unventilated rooms and gas engineers neglect the proper methods of installing gas lighting to give ventilation, as can so easily be arranged.

In comparing the cost of purchased electricity with an owner's plant suppliers are far too prone to make what they regard as correct commercial estimates. They put down a charge for land and buildings just as though they were building their own power station in a field. They debit the power user with a proper proportion of rent and rates and taxes, with half or some other fraction of a man's time and so on. They refuse to recognise that the users' own plant is put into a cellar for which he has no use whatever, that he already pays an odd man who

is not one-fourth occupied and yet cannot be dispensed with or replaced by a boy. The rates and taxes are not altered, the roof will supply all the water needed, and the user, therefore, properly makes out a very different balance-sheet, and small gas plants or oil plants are not wasteful or uneconomical like very small steam power plants. Mr. Booth.

Thus compounded with the heat supply problem the question of competing with the small power user is a most difficult one.

Mr. G. W. PARTRIDGE: There are only two points I propose to touch on. The first is the diversity factor for the London area, and the second is the cost of transmission. I have very often argued these questions with promotors of London Electric Supply Bills as to the actual cost at which they are going to lay down the various plant and mains, and just when one thinks one has proved that their price is much too low they turn round and say, "There is the diversity factor," and the argument has to begin all over again. I do not believe, and I am speaking with some twenty years' experience, that the author would ever get a diversity factor of 1·66 in London, and I should think it would be very much nearer his own figure for Sunderland of 1·25. If this is the case, as far as I can see, it alters Mr. Snell's calculations and figures throughout the paper. Mr. Partridge.

From my experience of the industrial area of London, although the factories make different material, the working hours are nearly always the same, and workmen, wherever they may be, cannot be induced to alter their dinner hour to suit a supply company's convenience.

The most important point is the cost of transmission, which in Table VI. is shown at £12 per kilowatt. I think that this figure is very much too low. It must be remembered that in laying out such a scheme as the author talks of, it would take a great number of years before he had many consumers connected, and he would also have to lay mains to pick up a large number of small consumers in side streets. After many years of experience I find that nobody can be induced to take the supply until one has absolutely got the main outside his door. It is no use going round and saying, "I have a fine station at Barking, and I can give you a 3-phase supply at ½d. per unit; will you sign this agreement?" The man says, "Let me see what you have got, and then I will sign the agreement." And very often, even after the mains have been laid, it takes two or three years before the consumer can be induced to take a supply. I know this to my cost, as I have been induced to lay a lot of power mains during the last three or four years, many of which are still lying idle.

The other point which would increase the cost of distribution is the cost of repairs and general road work of London, which is very much higher than in the provinces. The author would also have to allow for spare ducts in the first instance for extra mains, so that it would take a great number of years before he would ever get anywhere near the figure of £12 per kilowatt for distribution. I should say, instead of £12 per kilowatt, it would be nearer £30 per kilowatt for

Mr. Partridge.

the first two or three years. If we take the Newcastle Supply Company, mentioned on page 315, who are looked upon as knowing something about their business, it will be seen that the cost of mains and sub-stations is £26 per kilowatt. Now Mr. Snell allows a figure of about £3 for his sub-stations, which leaves a cost of about £23 for the transmission. If it costs £23 to lay a main in a district like Newcastle, surely it would cost very much more to lay the same in London with the expensive road work. I should like to hear Mr. Snell's opinion on these points.

Mr. Corringham.

Mr. G. H. CORRINGHAM: If the author's figure of 2d. per unit for lighting be reached, then with a lamp like the osram the consumer will be able to have 30 c.p. for fourteen hours for 1d., which is bound to stimulate greatly the lighting branch of the business, and in the transition stage the station will benefit by a reduction of peak. There are not many cases in which a large accession of power consumers does not bring down the prices to all classes. The usefulness of Table I. is considerably impaired by the omission of the wages for attendance.

I will take, first of all, the case of a 60-k.w. plant out of Table I.; if £75 per annum is added for the wages, which is about the most favourable case in the table, it gives 0·17d. per unit for wages alone, and the smaller plants in the same table would cost correspondingly more money. In Section II. a comparison of the graving docks is not as favourable to the power supply from an outside source as it might be, considering that in the first case the rate of doing work is twice that of the second, so that it looks as if the depreciation costs, if there are any on the electric plant, ought to be halved. The results obtained in the paper mills mentioned on page 296 are so remarkable that it would be worth while to know if the author has verified them, considering that some of the boilers are at 80-lb. pressure and there are twenty-five engines. With regard to the diversity factor mentioned on page 301 for Sunderland, I may mention that at Bermondsey the diversity factor, reckoned on the same lines, comes out at 2·7 as against this 2·4. In Table VII. the figure given for one of the London stations for the actual charge is over 2d. a unit. I do not know how this has been obtained, but in the year ending March, 1906, the actual figure for power units in that station was 1·45d., and is now, or was in the last published figures, 1·17d. Then in Table IX. the particular mean rate which the author gives for a 20 per cent. load factor is evidently wrong; but, I take it, it is a clerical error, because it is neither the mean of the preceding four columns, nor is it the correct average by multiplying those prices by the number of units in their respective stations and dividing by the total. With regard to Fig. 1, it appears to me that that is not applicable with regard to obtaining financial results unless the annual load factor be taken into consideration. It seems to me when running at full load, say for twenty-four hours a day, the fact that the loss of energy is constant will make a difference, as all the other curves are falling curves, both the capital costs per unit and

Mr. Corringham.

the production costs per unit. Then again, on page 315 the author says, "It has been assumed that every 100 k.w. installed at the power station will supply 166 k.w. outside"; and the cost of the power station is taken at £12 per kilowatt, giving £7·2 per effective kilowatt supplied. That can hardly be correct, because a station cannot be run without spare plant, and if so, that does away with the £7·2 per effective kilowatt supplied. In Bermondsey the present size of the plant is 1,475 k.w., and there is room to put in 4,000 k.w. more, probably at not a greater cost than about £10,000 for each 1,000 k.w. of plant; and if that is so, I do not see where the advantage comes in of having plant at, say, Barking Creek, at £12 a kilowatt, and adding on the transmission and transformation charges.

Mr. Andrews.

Mr. LEONARD ANDREWS: There are many points I should like to discuss in this most interesting and instructive paper, but I will confine my remarks to one point only, namely, to use the author's own words, "the inability of most undertakings to meet a large demand for power at such prices as to hold their own with local plants." I do not propose to deal with the competition with the smaller stations, because I do not think there is so much difficulty there; it is rather with the larger consumers taking, say, 500,000 units a year and upwards, that I think competition is going to be chiefly felt. Other speakers have dealt with this question by attacking the author's figures showing the cost at which these local stations can produce their power. No one appears to have referred to the question from the other end. Is the figure which the author has given as low as we can hope to reach? The author himself says he does not think there is much prospect of power companies materially reducing their charges for power supply in bulk below 0·68d. for transformed alternating-current supply under conditions of a 20 per cent. load factor, and 1·66d. diversity factor. I should like to ask Mr. Snell if, in arriving at this conclusion, he has taken into consideration the recommendations made by Mr. Stott, in a paper read before the American Institute of Electrical Engineers in 1906."* In that paper Mr. Stott showed that the cost of supplying current from a large power station could be greatly reduced by dividing the generating plant into two portions. In one portion, which would be reserved for the peak of the load and only run for a few hours per day, economy in running would be sacrificed to low capital cost. The other portion would deal with the flat part of the load curve, and, since it would be running several hours per day, economy in running was obviously of paramount importance. Mr. Stott concluded his paper by tables showing the relative capital and running costs of five different methods of generating power. I have reproduced Mr. Stott's conclusions, which were given in percentages only, and have added some further deductions of my own to bring Mr. Stott's estimates into line with the estimate given by Mr. Snell.

I have assumed that Mr. Snell's estimates were based upon the assumption that the generating plant would be steam turbine-driven

* *Transactions of the American Institute of Electrical Engineers*, vol. 25, p. 1, 1906.

Mr. Andrews.

alternators, and have plotted Mr. Stott's estimate of the capital cost of that type of plant to correspond with Mr. Snell's figure of £13 per kilo-

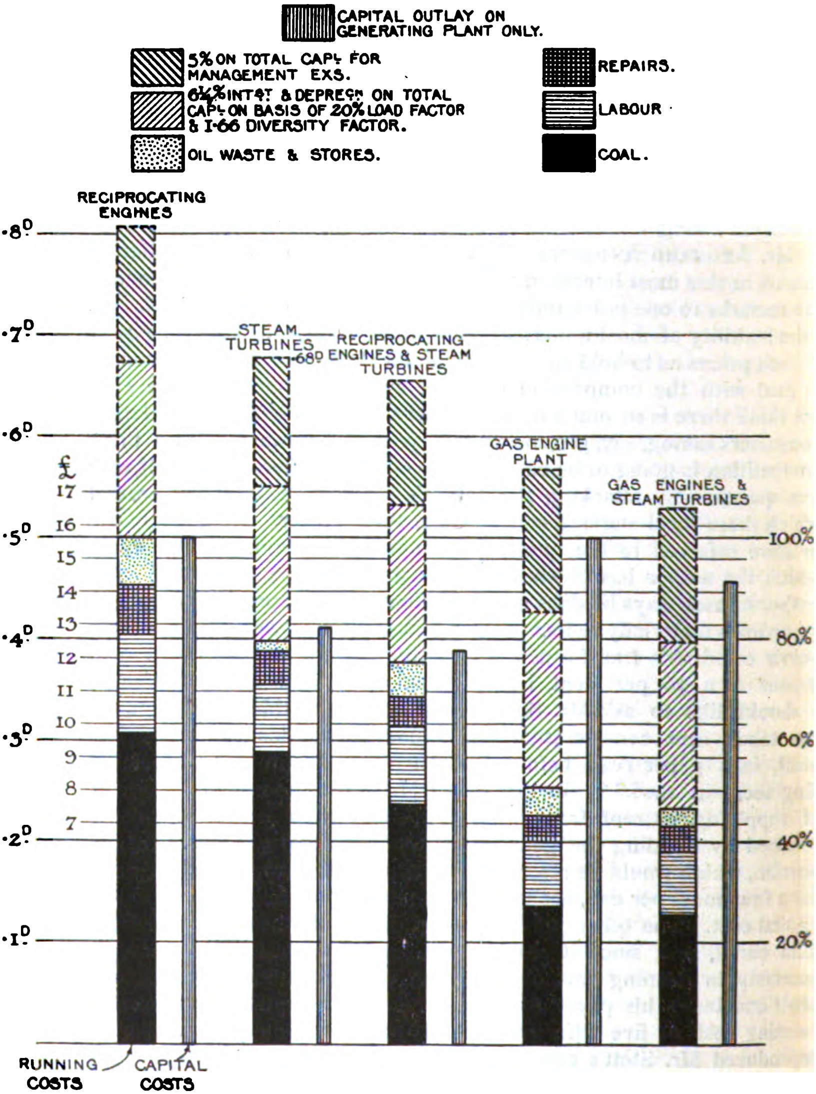


FIG. F.

watt (Fig. F). The relative capital costs given by Mr. Stott of other types of plant are plotted to a similar scale. To ascertain the running costs

I have taken Mr. Snell's figure of 0·68d. for steam turbine generation and have deducted an item of 0·157d., which corresponds to Mr. Snell's capital charge of 6¼ per cent. on the cost of the generating plant, mains, and transformers, with a load factor of 20 per cent. and a diversity factor of 1·66. A further deduction of 0·125d. (corresponding to 5 per cent. on the capital outlay) to cover rent, rates, taxes, management expenses, etc., leaves a balance of 0·4d. to cover running charges, and Mr. Stott's estimates have been plotted to that scale. The capital and running charges for other systems are plotted in a similar way, Mr. Snell's and Mr. Stott's figures being strictly adhered to in each case. The results appear to show that by adopting Mr. Stott's recommendation and combining in one generating station the most economical plant obtainable with a plant having the lowest capital cost, it would be possible for a supply company to reduce the figures given by Mr. Snell to the extent of approximately 22 per cent. As a practical illustration of what has already been done in this direction, I may mention that I had an opportunity of visiting a supply station in Germany a few weeks ago where a combined gas and steam plant were running in parallel, and, I was told, were supplying power in bulk to a town about 30 miles distant by overhead lines, where it was being sold at an average price of 0·5d. per unit. Mr. Andrews.

Mr. G. H. COTTAM (*communicated*): I must acknowledge the much greater fairness of Mr. Snell's figures than of those put forward in the preliminary reports of some other bulk supply bills for the Metropolis, and as I look upon this paper as being only the precursor of another, I venture to draw attention to some points which I think should not be overlooked, as I take it the chief object is to bring matters down to a common basis, upon which we can all agree, without spending so much money on legal expenses before Parliament. Mr. Cottam.

I would like to ask: Is it fair to those who have borne the burden and heat of the day, and who have buildings and land capable of considerable extensions, to be told that Parliament is going to be asked to ignore the preliminary expenses incurred in working up their connection, also to prevent them from extending their plant or putting down larger, which is acknowledged to be more economical? Or as some have asked, that the promoters should be exempt from the County Council Building Acts, while the local authorities are still to remain under them.

Then, as in another scheme, it was asked that their bulk supply mains should be exempt from local rates and taxes. Is this either free, fair, or just trading? Can any one look upon it in any other way but that of asking for protection for the promoters to come into districts where they believe there are plums which they wish to pick.

One company made a great show in advertising their preliminary scheme in giving their price for power at 0·22 of a penny, which when it came to be looked into at our load factor, meant for converted energy at our switchboard 1d. per unit we having to supply,

Mr. Cottam. house, and maintain the converting machinery, and it was they who asked exemption from rates for their mains.

Has bulk supply been so successful in general as to encourage promoters in this country?

I think the condition of the Midland Power Company and that of South Wales are known to a good many of us. There are others known to some, no doubt. Within the last week I was told that the Durham Colliery Power Supply Company, with turbine machinery, day and night load, presumably cheap coal, land, buildings, labour, and low rates, were selling at ¾d. per unit, yet their total costs were 2¼d. Perhaps some one can correct or explain these figures, if wrong.

Coming nearer home, Lot's Road when put down was, I think, one of the largest stations of the world, had the latest machinery, up-to-date boilers, coal conveyors, etc., unique condensing plant, geographical position for obtaining cheap water and water-borne coal, and not only a good load factor, but one they could estimate more nearly than many when putting down plant. Have they done such wonders as were expected? If rumour does not err, the cost per unit of their energy in Hampstead is greater than the local authority charges them for lighting their stations. Yet they had not to open ground especially to lay their cables. Another power station, namely at Neasden, which has a good deal of similar machinery and similar load, but has not the geographical position I understand Mr. Snell seeks, I am told can easily beat Lot's Road.

History repeats itself. If stations put down ten or twelve years ago are now obsolete, do bulk supply promoters by putting down an up-to-date one to-day hope to stop progress and to be ahead of it for all time, or would they appreciate a new scheme being brought before Parliament in another ten or twelve years for which exemptions might be asked which were not granted to them? Also for the new comers to ask Parliament to prevent present promoters from extending, because under certain conditions the new ones hoped to be able to supply a little cheaper?

A great point is made of the preliminary expenses connected with existing stations in putting in plant which is now too small. What about the preliminary expenses of the bulk supply authorities? Would not those of the past few years pay for a large amount of small plant, and if any of them obtain the powers, will they not want to make their promotion money remunerative?

I think the engineer to the Administrative Bill stated he could put down a station in London somewhere between £11 and £12 per kilowatt based on his experience at the Newcastle Power Company. Mr. Snell has given the expenditure of this company as £59·6 per kilowatt. Is this any proof that in practice he could not? Some of the London stations could come within this real figure if they were allowed to utilise the whole of their available buildings and land.

The gist of the whole matter is, do the promoters wish to be considered philanthropists as doing something for the public good, or as

business men seeking concessions and advantages for their capital, which that of the older pioneers does not enjoy? Mr. Cottam.

In conclusion, I would ask, Have the various schemes put before Parliament in the past few years done any good to any one excepting a few consulting engineers and a large number of the legal profession?

I believe it is acknowledged that they have done considerable injury to the industry in general, upsetting councillors and shareholders, and making all exceedingly chary in spending or subscribing any money, and would-be consumers from coming on, because they think by waiting they will obtain energy so much cheaper owing to the misleading figures that have been put forward through the daily press.

Mr. W. C. P. TAPPER (*communicated*): With many of the author's conclusions I am in cordial agreement, although on one or two points I must join issue with him. While a column of average costs per unit may very well suit a non-technical Parliamentary Committee, I must enter a very strong protest against this method of expressing such values before a technical audience. Take Table VII.: the figures given, apart from being, in my opinion, incorrect, are utterly meaningless without reference to the load factor and diversity factor taken by the author in calculating them. It is now the usual practice to refer to charges for power at so much per kilowatt per annum, and so much per unit. Such a statement is complete in itself, and fully takes into account the load factor. If the tables had been prepared in this manner I venture to think they would have been very much more useful. Mr. Tapper.

I cannot say how far Table VII. is correct so far as other stations are concerned, but certainly the figures for Stepney would have appeared very differently if stated in the form I suggest. The charges in that district would be more correctly stated as £4 per kilowatt per annum plus 0·5d. per unit, with a maximum charge of 1d.

In fixing a scale of charges it is obviously necessary to determine very closely the actual cost of production. Here, again, I would strongly urge the advisability of stating the cost in terms of so much per kilowatt per annum and so much per unit. To illustrate my meaning, for a certain year the costs at Stepney were as follows:—

Per kilowatt of the maximum demand on the station	£9·5
Per unit sold	0·495d.

These costs were determined on the methods laid down by Mr. Arthur Wright in his well-known paper read before this Institution some years ago.

Now the power diversity factor in Stepney is somewhere in the neighbourhood of 3·5 or 4. Taking the lower figure, the actual cost per kilowatt supplied would equal, say, £2·7.

Thus the cost and charges may be stated as follows:—

Mr. Tapper.

	Per Kilowatt per Annum.	Per Unit.
Charge to consumer...	£4	0·5d.
Total cost of production	£2·7	0·495d.

It will be observed that there is a margin of profit under both heads, and therefore on this scale, no matter at what average price any consumer may be obtaining energy (even if below the average cost), no loss would be incurred. The great advantage of this method of determining the actual scale of charges to adopt is that it can be seen at a glance whether a profit or loss will follow the adoption of any particular scale, no matter what the individual or collective load factor may be. It is, of course, essential, as pointed out by the author, that the right diversity factor should be used.

In conclusion, I should like to ask the author to state definitely in his reply the cost per kilowatt and per unit which he has taken in each case in preparing Table VII.

Mr. Pearson.

Mr. G. PEARSON (*communicated*): I have read Mr. Snell's paper with considerable interest, and am in agreement with many—indeed, I think I may say the majority—of his general conclusions.

The only weak point I notice in it is that the power companies have not yet spread themselves over the country as was first expected they would, and I fear that it will be many years before small towns will be able to command the services of large power companies in their immediate vicinity. Assuming a power company to be in existence, and the charges to be as foreshadowed by Mr. Snell, then I think it would be an advantage to the small towns to accept the services of such companies rather than to generate themselves, but I do not think that a power company is ever likely to be of a very great deal of service to the larger municipalities, nor do I think it possible that a power company can exist except in the midst of a thickly populated district, and I am inclined to agree with Mr. Snell that the estimated distance over which power companies can economically supply has been somewhat overstated. We, in Bristol, are now arranging to supply 8 miles from our station, but I should think twice before agreeing to supply at double that distance, however high the voltage may be fixed.

Mr. Shaw.

Mr. A. H. SHAW (*communicated*): Although much of the information contained in the first three sections of the paper is of interest and value, I do not consider Section IV. helps us to a solution of the problem of cheap power supply for London.

I do not think it is fair to compare the cost of power in the Newcastle and Sunderland District with the cost of power, even from a large bulk station like that proposed, erected in London. The costs of the latter are bound to be higher, and I think I am right in stating that the large majority of 400,000 H.P. in London referred to is made up of a large number of comparatively small consumers instead of a comparatively small number of very large consumers as on Tyneside. I think the costs of the Lot's Road Station have not been altogether

as low as were anticipated, but should be glad to know what these now are, and also the power factor, if Mr. Snell is in possession of these figures. Mr. Shaw.

Turning to Table VII., notwithstanding Mr. Snell's explanation, I do not agree that these figures are correct. If the charges he takes as being "correct" had been charged in the first instance they would still be incorrect, as the load upon which they are based would not have been obtained at this price; and by the same method of calculating it would have been necessary to fix the price at the highest charge possible, in which case no power load at all would have been obtained.

It is also incorrect to compare these charges with the "probable" charge from a power supply company. The former charges include all distribution losses and establishment losses and the average consumer is only a small one, whereas the latter is the estimated average charge of the London County Council for a 20 per cent. load factor for a bulk supply.

I note on page 307 that Mr. Snell recommends that transmission systems should be so arranged that works requiring over 100 k.w. should be supplied from their own static sub-stations. When I raised this point with the officials of the London County Council in regard to their Power Bill, I was informed that they would not be prepared to supply a local authority in bulk at more points than one in any district until such sub-station had a load of between 2,000 and 3,000 k.w.

With regard to the capital cost of London stations, it is only to be expected that the older the station the greater is the cost per kilowatt; but, as the author points out, this is continually being reduced by extensions, and in regard to stations more recently erected is not excessive.

At the station with which I am connected, Ilford, the cost is about £46 per kilowatt, which with further extensions now in hand will shortly be reduced to about £38 per kilowatt, about 50 per cent. of which is expenditure on mains.

I do not agree with the author's conclusion that the only solution of this matter is a large bulk supply company. On the contrary, I believe that the best solution will be found in a scheme of linking up various stations—whether companies or local authorities is immaterial. In such a scheme the larger, more economical stations would be extended as required, supplying current at low rates to the smaller stations, which would only be run on the peak load. It is quite evident that the large amount of capital already sunk in electricity supply in the London district cannot be ignored, and in order to utilise this to the best advantage it is to the interest of all concerned to do their utmost to press the Board of Trade to push forward with their Electricity Supply Bill in the coming session.

Mr. J. H. Bowden (*communicated*): Mr. Snell's paper is particularly attractive to those engineers and managers of electricity undertakings interested in the problem of bulk supply to Greater London, consider- Mr. Bowden.

Mr. Bowden.

ing that it affords an insight into the premises upon which the London County Council Bill of 1907 was compiled, and I trust that the discussion will not close until some light has been thrown upon the methods by which the figures in Table VII. were ascertained. When this table in its complete form was laid before the Parliamentary Committee last year I was surprised to find that nearly every London engineer did not until then realise that he was supplying energy for power purposes at a dead loss to his undertaking; but, fortunately, information was available as to the "correct" price, for which I am sure we were all extremely grateful. Personally this table was a revelation, but I then understood it simply as a means to an end in engineering the London County Council Bill through the Parliamentary Committee, and that it should not be taken seriously. But when we are confronted with the selfsame figures in the debating-room, and one individual stands sponsor for the same, I think it a fitting opportunity to gather information, and I shall be glad if Mr. Snell can by any known method of reasoning assimilate his figures with actual facts as stated below.

With regard to his statement concerning Poplar—actual charge, 1·43d.; correct charge, 1·686d.; probable charge for direct current by power company, 0·83d.

	1906.	1907.	1908.*
Actual charge by Poplar ...	1·389	1·156	1·12
Load factor of system	22·84	24·26	23·5
Power units sold	1,379,620	2,423,538	3,500,000
Net profit...	£600	£635	£3,340

From Mr. Snell's methods of reasoning (see p. 308), "but let there be a big increase in the amount of power supplied at the same rates (and despite its effect in the general reduction of generating costs) it will be found that the total receipts are not commensurate with the total costs, and the result will be an unsuccessful undertaking," we should anticipate that the result of the current year's trading in Poplar would be quite contrary to the assured results, especially in a year remarkable for an enormous rise in the price of coal. But practical business teaches us otherwise, as from the figures given it will be seen that Poplar's power supply in 1908 is over 150 per cent. more than in 1906 (to which year Mr. Snell's statement relates), and the average selling price is 19·$\frac{1}{3}$ per cent. less; meanwhile the price for private and public lighting has remained stationary, whilst the increase in supply is only 13·$\frac{1}{3}$ per cent. more than in 1906, and the surplus has increased by 456·$\frac{2}{3}$ per cent. I

* These approximate figures are based upon actual balance-sheet to December 31, 1907, and the estimated result for quarter ending March 31, 1908.

Mr. Bowden.

do not claim that these returns are entirely satisfactory, as I am convinced that if inducements had been offered to lighting consumers similar to those offered for power the result would have been much better; but they serve to prove that Mr. Snell's theory is entirely wrong.

Still referring to Table VII., in which existing undertakers are shown to possess a miserable conception of business methods, we find that the probable charge by power supply companies for direct current is 0·83d. per unit. I am accredited with charging the sum of 1·43d. per unit for something which ought to be charged at 1·686d., but which might be obtained elsewhere for 0·83d., or, in other words, at half-price; for in comparative statements relative conditions must be considered, and I read that the private consumer would be able to purchase at 0·83d. I am anxious to know the source from which Mr. Snell obtained the latter figure,* as I find on reference that columns 1 and 2 in the table are derived from Tables Nos. XVIII., XIX., and XX. submitted by the London County Council to Parliament last year, and column 3 from Table No. V., but in the first instance the price given is the charge to consumers by authorised distributors, and in the second instance the "estimated" charge to authorised distributors.

Am I to take it that private consumers are to be placed in a position equally favourable with authorised distributors? Possibly Mr. Snell has overlooked London County Council Table No. IV., where the maximum charge under similar conditions is stated at 1·04d. (and at present coal prices this figure would undoubtedly be enforced), and the charge to power users for low-pressure direct current is 1·255d., actually in excess of the average charge by Poplar at the present day.

I am surprised at the statement appearing on page 306, "It is of little use straining to reduce the capital outlay per kilowatt on the power-house and distribution system if one is going to sink an additional £2 or £3 per kilowatt on rotary sub-stations." One might as well assert that it is useless to strain to reduce coal costs if it is found impossible to reduce the cost of water and stores, and I am sure on due reflection Mr. Snell will modify this statement.

On page 308 Mr. Snell states that "the severest handicap to power supply from existing London and provincial stations is, in the majority of cases, their present high capital cost per kilowatt." This is admitted by all concerned, but Mr. Snell assumes that by shutting down the existing plant the debt on account thereof is automatically wiped out.

Bulk supply will not reduce the price to the consumer one iota unless it can be supplied cheaper than can be produced by extending existing undertakings. Who is to bear the original or experimental cost—the present and future consumers, or the ratepayers as a body? Or in the case of companies, the original shareholders who have sunk their capital in a concern that is now found to be obsolete, and all the

* The figure of 0·83 referred to in Table VII., column 4, has since been corrected by the author.

Mr. Bowden.

advantages of experience to be offered to new subscribers? Mr. Snell admits that each extension will reduce the original capital cost per kilowatt, but to prove his case he must offer something cheaper than can be produced by each of these extensions. But does he do so?

Fortunately, I am in a position to disprove by facts the figures that he assumes by hypothesis. During the year ending March, 1904, the original scheme at Poplar cost (1) on account of loan service, £10,165 15s. 8d., and (2) on account of revenue, £12,222 10s. 9d., making a total of £22,388 6s. 5d. (the published figures are adjusted by the inclusion of repayments of loans that had not then come into operation); the effective capacity of the plant was 1,100 k.w., and the output, with a 23 per cent. load factor, 2,216,280 units.

During the year ending March, 1908, the effective capacity of the plant is 2,600 kilowatts, which, with a 23 per cent. load factor, will produce 5,238,480 units. The charges (1) on account of loan service, £13,968 6s. 4d., and (2) on account of revenue, approximately £18,120. By deducting the first set of figures from the second, we have the following result:—

Output	3,022,200 units.				
Loan charge	£3,802	10	8	= 0·302d. per B.T.U.	
Revenue charge ...	£5,897	9	3	= 0·468d.	„
Total	£9,699	19	11	= 0·77d.	„

The total cost per unit in the first case was 2·423d., but by a recent extension, producing current at 0·77d. per unit delivered to consumers, the average cost is brought down to 1·47d. The "estimated" price for an equivalent load factor, as scheduled by the London County Council, is 0·757d. metered—I presume at the feeding-points—to which must be added loss in distribution, distribution costs, management, rates, and loan service charges on account of mains, meters, and services, which would be considerably more than present cost.

If I have to pay 1d. for what I could produce for ¾d., and also compete with the supply company for supply to large consumers, the possibility of wiping out the original and uneconomical plant will become very remote indeed.

But I doubt if any company can yet hope to supply at the price estimated by Mr. Snell, as his hypothesis appears to me to be on an entirely wrong basis. From data given on page 305, I take it that the charges are worked out upon the capital cost per kilowatt installed, whereas they should be based upon the capital cost per kilowatt of maximum demand, which would raise Mr. Snell's figure of £29·73 to something in the nature of £50.

If we allow 4 per cent. interest and 4 per cent. depreciation, we have then alone a charge of £4 per kilowatt of maximum demand on station, which, with a diversity factor of 1·5, is equal to £2 13s. 4d. charge to consumers. The remaining standing charges will quite absorb the balance of £3 per kilowatt for E.H.T. untransformed

current, which is the basis upon which Mr. Snell's price, namely, 0·83d. low tension, is estimated, but only 0·18d. per unit is allowed for energy supplied. Sanguine as all promoters of bulk schemes are, the time has not yet arrived for production at this price. Mr. Bowden.

May I ask for a little more information on this matter, particularly in view of a recent firm quotation received from the largest existing undertaking, in which a stipulation is made for a variation of 0·01d. for every 6d. in the price of coal, equal to at present cost prices 0·3d. for coal alone?

Mr. L. H. HORDERN (*communicated*): Whether or not we agree with Mr. Snell's conclusions, we all owe him thanks for the facts he has given us as to the costs of working gas and steam plants compared with electric driving. The economics of electricity supply have, however, a commercial as well as an engineering side. Not a single one of the big power schemes has yet proved itself a financial success, and I hope this may be the last year in which it will be proposed to introduce into London such a peculiarly unsuitable method of supply. We must look for safe progress on evolutionary, not on revolutionary, lines. Mr. Hordern.

The term "diversity factor" has hitherto denoted the effect on the stations of the different time at which consumers demand their maximum load, but this does not seem to be always its meaning in the paper before us. Now, this difference in time is the essence of the whole question; the load factor of individual consumers is a secondary consideration. Lifts, for instance, which individually have a very bad load factor, give collectively a very good load factor to the supply station. Under any scale of charging which involves a high rate per kilowatt and a low rate per unit, the cost of working lifts electrically would be prohibitive, and I believe that in London a flat rate is the correct system of charging for power, and not the scale suggested by power companies. These scales may suit the scattered districts for which power companies are the only possible system of supply, but they seem to be as unsuited to a closely populated district as are power companies themselves.

Mr. Snell has been at pains to work out a table showing the "correct" charges which should be made for power in each district in London based on two assumptions. One of these we know cannot be correct for every district, while the other he warns us himself "is quite probably wrong," and "if applied universally will certainly lead to trouble." In these circumstances, might not the third place of decimals have been omitted?

In conclusion, we are advised that by hook or by crook a large power station must be erected, but that it is essential it should not prejudice present capital; the omelette must be made without even cracking an eggshell. Even supposing this impossible feat achieved, we are not told why we should go to the new station for our supply. We are responsible for maintaining a supply, each in a certain area; that we were purchasing our supply from another company at a fractionally lower rate than we could make it ourselves would not

Mr. Hordern. divest us of responsibility for failure—even if our statement were true. Who, again, is going to place the success or failure of their undertaking in the hands of another company who, on its own showing, would have to cut everything very fine to make a profit, and whose plant or mains might consequently break down sufficiently frequently to interfere with business?

The generation and supply of electricity must be in the same hands if a company is to control its own destiny, and to achieve the best results the whole of the supply in any district should be given by one undertaking and not be divided between two or more. Small areas where this is the case are in a better position than large areas where generation and supply are separated, or where power and light are supplied by two different concerns. A power company operating in Westminster could not supply the large consumers cheaper than my company, and it could not supply the small consumers at all; while if it were merely supplying current in bulk, we should require to be able to prevent it economising at the expense of reliability and to exercise considerable control.

Mr. Heaviside. Mr. A. W. HEAVISIDE (*communicated*): I do not propose to say anything upon that well-worn subject, the cost of production, distribution, and application of electric energy. That has been well thrashed out at the present meeting—the delusive 1905 scheme for London, the timid 1906 scheme, and the emasculated 1907 scheme, that is, minus the tramway load. Neither do I propose to say anything upon the shibboleth of the electrical engineer, those wonderful factors, the power factor, the load factor, the diversity factor, the intensity factor, or the determinate factor. It has been said abroad that these factors are born of the British savage's desire for a formula for every problem in life. Let them laugh—we can afford to smile. Factors have their uses, though they may be overdone.

Mr. Snell's paper is reminiscent of the electrification schemes for Greater London, therefore it opens the door for a few remarks upon that score.

Let us reflect. The reason for the existence of all electrical undertakings is fundamentally for the advantage of the British public. When Mr. Ferranti suggested that idea before the 1906 House of Commons Committee, counsel spoke of him as the friend of humanity, with tongue in cheek and satire in voice and pose. Perhaps that may be my fate.

Now is it really to the advantage of the British public that London should be under the control of one supplying body? I think not. From early days those who have studied the subject have accepted the principle of concentration of production for distribution within an economical area, which Mr. Snell's paper, as well as many others, show is sometimes within the premises of a private person or persons; at other times, a large city; and sometimes, a whole district, each case having to be taken on its own merits. However, it appears to me that for London and its boundaries it would be a very bad thing for the

British public if London should be assumed to be within the economical distribution limit from one producing centre. It can only be an assumption, for no experience exists in proof. Mr. Heaviside.

Again, electrical energy is not so economically stored as gas or water at the present time. It is upon a different footing, destructive to its storehouse.

Again, can it be said that the means of production or distribution have settled down to finality? Certainly not. The next twenty-five years will be as fruitful as the past both in discovery and invention. Bold would be the man who dared to prophesy before he knows in such a case.

Any monopoly for London, however safeguarded by statute in the public interest, would tend to stereotype all the appliances used by the electrical engineer and to develop the human failing of "Jack in office," the take-or-leave-it style, to the highest degree, its effects being worse to the British public than that on Sinbad the Sailor of the old man of the sea; if once established it might take generations to knock the dead weight off. Therefore, while it is universally agreed that a number of uneconomical systems over small areas is undesirable, I would not, if I were the House of Commons, representing the British public, permit any amalgamation scheme of existing undertakings to go through if the principle of competition was not a *sine quâ non*.

I would group the existing undertakings as would appear to be the most advantageous to secure cheap production and distribution. Each group to be under one management from the coal heap to the lamp, motor, or other appliance, that is under one committee and one engineer.

The engineer should have as free a hand as common sense permits to develop his particular group so as to achieve the most successful result, taking all the circumstances of his group into consideration. Then at stated intervals, five, seven, or ten years, a balance-sheet should be drawn up by an independent body of experts, comparing the results of the five, seven, or ten years' working, both technical and administrative.

Should such an examination show marked advantages in the working and administration of one group as compared with another, it would become the duty of the less successful groups to fall into line and gradually bring their systems up to a higher standard of efficiency. The extreme case would be the desirability of standardising all the groups on one system.

Now take London and its boundaries as it is suggested should be dealt with as a whole. It is expected that in twenty-five years eleven millions of people will have to be provided for in London. Twenty-five years is about the life of many of the licences. To secure competition in supply, in discovery, and in invention, London might well be divided up into, say, six areas, each containing approximately one million persons now, expanding to approximately two millions by the time the licences expire.

Mr. Heaviside.

Surely the wants of such a section of the population for all electrical requirements are big enough for one engineer to look after and make his work a success, stirred by the knowledge that he is competitive with the other five sections, and judgment will go by results. Such a system fairly run would bring about competition in its liveliest and most healthy form, not only amongst the engineers, but amongst the manufacturers who serve them, and amongst inventors who are striving to distinguish themselves. In such a scheme provision should be made for an emergency service to meet the unexpected by means of a ring main all round London, by which one group could assist another group when in need. But if I know the electrical engineer, it would be a rare thing to be asked to assist. How rare is a breakdown now from any cause !

The efficiency of the modern engineer is such, and the plant of the modern manufacturer so good, that the public service is well performed though costly in some cases. The native pride of the engineers would impel them to be self-supporting except where common sense suggested the reasonableness of asking or giving assistance.

Electrical power supply at rates higher than steam or gas pays the consumer in convenience and upstanding charges apart from releasing the brains of the consumer for application to his own speciality. See my 1900 address to the Newcastle Section.*

The view expressed herein was made known to the 1906 House of Commons Committee in response to their invitation to outsiders for suggestions, and was duly acknowledged.

Mr. Woodhouse.

Mr. W. B. WOODHOUSE (*communicated*): The paper, broadly speaking, is a consideration of the commercial possibilities of a public supply of electricity, and I am much interested in comparing Mr. Snell's conclusions with those I myself have arrived at, which were embodied in a paper read before the Leeds Section recently.

As I understand it, Mr. Snell's conclusions are gloomy ones. In Table VII. he compares the prices for power charged by the London undertakings with what he describes as the correct charge, and he concludes that two-thirds of the undertakings are selling at a loss. The provincial stations are in the same sad state, and the whole makes a dark picture. Now I am inclined to think that Mr. Snell has generalised too freely in making his comparisons, but on the broad principle that the smaller and older stations cannot supply large power users at competitive prices I am in full agreement with him.

Mr. Snell estimates in Section I. the competitive prices at which electric power must be supplied, but I think it should be emphasised that a large undertaking's supply may be divided into two classes :—

1. Bulk supply, that is, for redistribution for lighting and small power users.
2. Supply to power users direct.

* *Journal, Institution of Electrical Engineers*, vol. 29, p. 900, 1900.

The first class of customers must have electricity, and the competitive price at which power may be bought is easily determined. In the second class, however, the problem for the power user to decide divides itself into two parts, the first whether electrical energy is preferable to mechanical transmission for his works; the second, which depends on the answer to the first, what is the competitive price? It is obvious that if mechanical transmission is preferable the supply would be utilised in one motor, but that if the reverse is the case then a number of motors would be used—that is to say, the competitive price would be higher. Mr. Woodhouse.

An analysis of the average costs of Table I. shows Mr. Snell's estimate as equivalent to a charge of about £3 6s. per kilowatt per annum and 0·5d. per unit. It should be remembered that by far the greatest part of a power supply will be to consumers having load factors between 10 per cent. and 30 per cent. Between these limits the figures seem fair ones, but they cannot be considered as anything but a first approximation. One power user with a 25 per cent. load factor may require to run his plant day and night continuously; another may only require to run for definite hours daily and will shut down completely the rest of the time; their costs may, therefore, differ considerably.

The cases quoted in Section II. add to our information some very interesting figures. I think, however, that the comparison of the pumping costs on a graving dock and pontoon are made on a false basis, that of ship tonnage. It is obvious that in the dock a large ship will, in proportion to its draught, displace a larger quantity of water from a full dock than will a small one, and therefore leave less to be pumped. The comparison should be made on the basis of pump horse-power-hours.

It may be noted that the price given for the public supply would be an absurdly low one if capital charges were considered. The load factor of the pontoon pumps would in practice not exceed 1 per cent. per annum. With this load factor it is not surprising that clashing with the peak rarely occurred. The average water horse-power in the electrical case was 168, in the gas 96, equivalent to, say, 160 I.H.P. How did two 40-H.P. engines do this? If there is an error here, then the capital charge would be considerably increased.

Paper Mills.—Any calculations based on horse-power-hours of steam plant must be looked on with suspicion. I cannot accept Mr. Snell's ratio of costs per unit and per horse-power-hour. The ratio of kilowatt to electrical horse-power is 1·41 : 1·05, whereas Mr. Snell assumes the ratio as 1·41 : 1, or the generator has an efficiency of 105 per cent.

I cannot agree with Mr. Snell's statement in Section III. that a cable and distributing system sufficient for a large power scheme can be laid down at a cost per kilowatt equal to that of the generating station; but, as I have endeavoured to show in my recent paper, the advantages of centralisation will allow a considerably greater proportion of the total costs to be expended outside the station.

Mr. Woodhouse. Lastly, as to Mr. Snell's cure for the present ills the suppliers of power suffer from. It is, of course, centralisation and co-operation. The power companies' engineers have been preaching this doctrine for some time, despite the open and covert opposition of the municipal authorities. That we now have Mr. Snell on our side is a sign of the times. Mr. Snell's suggestion that the larger boroughs should go into the power supply business outside their own areas overlooks the fact that the principal industrial areas in the country are the fields of operation of already existent power companies. Were this not so Mr. Snell's suggestion is a sound one, but, except for London, the solution comes too late, the problem is already solved; it only remains for the technical advisers of the municipalities to accept Mr. Snell's conclusions for these power companies to have a considerable increase of business.

Mr. Esson. Mr. W. B. ESSON (*communicated*): I have read Mr. Snell's paper with great interest, and am in agreement with many of his observations. It appears to me that he has adopted throughout an unbiassed attitude, dealing with his subject in a singularly fair manner. Much of the ground covered by the paper was covered by my address on "The Industrial Power Problem" to the Civil and Mechanical Engineers' Society in October, 1906, and Mr. Snell arrives at practically the same conclusions as I did. I considered there the cost of electrical power for a factory which could be served by an engine of 500 B.H.P. when Mond gas, suction gas, steam, condensing and non-condensing, and oil were used. It was assumed that the generator would give 350 k.w. maximum and would supply 750,000 units per annum, this corresponding to a load factor of about 25 per cent. The cost of the fuel was put in at rates somewhat different from Mr. Snell's, but the total cost, taking 10 per cent. as the interest on capital outlay and depreciation, worked out and was given as follows:—

	Mond Gas.	Suction Gas.	Steam Cond.	Steam Non-Cond.	Oil.
Total cost per unit	0·516d.	0·564d.	0·513d.	0·536d.	0·592d.

The figures given by Mr. Snell in Table II. for a load-factor of 25 per cent. are:—

	Mond Gas.	Suction Gas.	Steam Cond.	Steam Non-Cond.	Oil.
Total cost per unit	—	0·670d.	0·590d.	—	0·584d.

Mr. Snell's figures are somewhat higher than I gave because he averages the cost of plants between 100 H.P. and 500 H.P., whereas mine were for the higher output. The agreement is sufficiently close, however, to show that, under ordinary circumstances, the total cost will lie somewhere between 0·5 and 0·6 of a penny per unit.

Passing over the interesting examples given, which confirm the figures of the tables, I notice on page 301 something new and somewhat startling in the way of definition. Mr. Snell remarks that in Sunderland at the time of the last census, "there were 587 motors, repre-

senting a total horse-power of 1,634, or an equivalent to 1,220 k.w. The maximum demand on the station plant was only 500 k.w. There was thus a diversity factor of 2·4." Now, whatever meaning should be given to the term diversity factor it should certainly not be this one. There is a diversity factor in lighting as well as in power, but no one would define it as the ratio of the number of lamps fixed to the number simultaneously lighted, though this would be analogous to the definition given by Mr. Snell. Later, on page 306, he uses the term diversity factor in another sense, namely, "as the ratio of the sum of the maxima observed at the several sub-stations to the actual observed maximum demand on the plant supplying them," and here again he misses the proper and scientific definition. Properly the magnitude of the diversity factor expresses the degree to which the load factor is improved by the diversity in the hours of demand in the industries supplied, and it can only be determined by ascertaining the ratio of the sum of the maximum demands of all the factories served to the maximum demand on the power house. If it is found that the former is 50 per cent. greater than the latter, the diversity factor = 1·5 because the load factor on the power house is one-half better than if there were no diversity; if 100 per cent. greater, the diversity factor = 2 because the load factor is twice as good, and so on. The diversity factor cannot be got at by taking the sub-station loads, as Mr. Snell has done, for the simple reason that at each sub-station the demands of all the factories served therefrom are already averaged up. It is quite possible that, though the real diversity factor may be very large, the sub-station to power house ratio may be unity. Mr. Snell, under a misapprehension, again refers in this connection to the horse-power of the motors simultaneously running as compared with the total horse-power installed, but this has nothing to do with either load factor or diversity factor, if to these terms are assigned their proper meanings. Mr. Esson.

On page 307 Mr. Snell says, "In fixing maximum scales it is wise always to base the maximum rates upon a diversity factor of unity," but this simply means that to begin with, some particular load factor must be assumed which it is hoped diversity will improve later on. In any case an assumption must be made, and we are still left to decide whether we shall take as a basis of price, to start with, the load factor of the worst industry in the district, or of the best, or of the mean between the two.

I heartily agree with Mr. Snell's remarks in Section IV. as to prices. His uncertainty as to the meaning to be attached to diversity factor does not vitiate his general argument. There is no doubt whatever that for a big increase in the amount of power supplied by the companies at the same rates it will be found that the total receipts are not commensurate with the total costs.

Mr. C. F. B. MARSHALL (*communicated*): I give below for the author's consideration some figures which I can vouch for, obtained from an installation of three 250-B.H.P. gas engines, single cylinder, made by the Premier Gas Engine Company, of Sandiacre. One is used exclusively Mr. Marshall.

Mr. Marshall.

for electric welding, whilst the other two take fortnightly turns at driving a factory electrically, thus giving a poor load factor:—

YEARS 1905–6 INCLUSIVE.

Wages, repairs, oil and coal	£2,248 4s. 7d.
Units generated	455,175.
Coal consumed	708 tons 7 cwt.
Cost of unit production only	1·185d. per unit.
Cost of unit with depreciation... ...	2·12d. „
Coal consumed per unit	3·485 lbs.
Load factor	6·4 per cent.

The President.

The PRESIDENT (Colonel R. E. Crompton, C.B.): Before calling upon the author to reply to the many interesting points raised during the discussion, I wish to add myself that before I was aware that I was to be your President I had intended to come to this meeting to discuss the questions raised by the author, which were the groundwork of so many of the most interesting discussions which this Institution had before it during the early years of development of electrical engineering. Twenty years ago the controversy raged around electrical design. At that time no one thought for a moment that any one type of plant could be said to be standard or suitable for all kinds of requirements. Therefore, when I read the author's opening words, "The fundamental elements and details of design of large power stations and transmission systems are now so generally accepted that there appears to be but little to add to the available information," I felt that if by these words he means we have approached finality in design, I, for one, am sorry for the Institution of Electrical Engineers, because I feel that the greatest part of the interest of our discussions would disappear. All of us, old and young, who take part in these discussions feel that we have the power of improving matters, and this is a great incentive to us to come here to talk these matters out.

Although I am one of the older members, I am as full a believer in future progress as the youngest of us. I consider that we have no more reached finality in what I may call the water-tube-boiler-cum-turbo-cum-reciprocating-three-phase-static-transformer plant — which now appears to be fashionable, and by some, apparently including the author, to be accepted as finality—than we were fifteen years ago. I do not think that the only notable electrical development which has greatly affected the public—I allude to the exchange of metallic for carbon filaments for illuminating purposes—is going to rest by itself, for I think we have still a very good chance of cutting large slices out of the losses which still exist in thermo-dynamic efficiency, *i.e.*, between our fuel and the energy distributed by our electrical mains. I think this is shown by the results which have followed on the increased study during the past few years of the internal combustion engine and the spur that this has incidentally given to improved steam engine and boiler design. This study will, I think, yield such great advances in thermo-dynamic

The President.

efficiency that the plant which the author, or any of us, could specify now as fully up-to-date will in fifteen years' time be as hopelessly antiquated for producing and distributing energy at low cost as the plant which is now generally in use, and which has been slowly developed in our existing power stations, so that I think it is regrettable that we should drag in this question of the unsuitability of existing plant as an argument for, at this stage, putting down gigantic stations, for, if I am right, those who start now with gigantic schemes will find, when they have got them into working order and, added to that, have gone through the period of canvassing for a power load, that by the time they have got this load they are in no better position than those whom they are seeking to supplant at the present time. I am not a believer in megalomania, or the desirability of gigantic power stations put down merely for their great size. I do not think that Lot's Road, big though it is, is absolute perfection. On the other hand, I can strongly support Mr. Snell's contention as to the great difficulty of persuading users to take power from a power scheme in view of the low cost of producing electricity by providing plant on quite a moderate scale of output. I have myself experience of plant producing on the scale of 1,000,000 units per annum which has produced it for many years at the same, or even lower, prices than those shown in the paper, which have been so criticised by some of the speakers in this discussion. I consider that it is fully proved that many of our modern manufacturing firms can offer plant driven by suction gas or by steam and guarantee that at the 1,000,000 units per annum rate of output 0·6d. per unit will cover all costs, including 12 per cent. for interest and depreciation, and that this low cost of private production is the real difficulty in obtaining a large power load, which the author thinks would be an easy matter if the cost of production and distribution were as low as it could be made from modern stations of very large size. I am not now speaking merely from the engineering point of view, but from long experience as chairman of a London supply company I know that the canvasser for the manufacturer of private plant is the enemy that has prevented us from acquiring power loads of any considerable magnitude. I can mention a case where suction gas plant has been put down within the last few years within a few yards of modern turbo-driven electrical plant in one of the power stations in the north of London. There is ample evidence to show that in London the power load we are likely to obtain, and shall obtain, will be that due to the large and increasing class of small users of power, and that we have no great hopes of obtaining much from large consumers.

As to the small consumers, I should like here to digress to say one thing. As it has been in the past so it will be in the future, we have to educate the public who are the consumers. If we wish to reduce the total cost of electrical supply by increasing the demand for power so as best to fill up the blank spaces on the diagram which was sketched on the blackboard, we must take a leaf out

The President.

of the book of the great drapery houses. You know how much the drapers have reduced their losses by holding remnant sales, and by thus disposing of their remnants they have been enabled to sell the bulk of their goods to the public at lower prices without diminishing their profits. *We* also have remnants for sale, that is, our power and heating load at the times of small lighting load demand. Every one of us who sells energy for power and heating at 1d. does so because, as one speaker has put it very neatly, we wish to make the plant we have already in existence yield the best possible return under existing circumstances. I must demur to the superstition which has arisen that sellers of power must never raise prices once they have lowered them. Why should the price of one commodity—power—differ from all other commodities sold to the public? It is not true of bread, or of coal, or of any other necessity. Why should we not vary our prices according to the conditions of the time? If these conditions are favourable, if our districts become crowded with manufacturers so as to increase our output, increase our diversity factor, and by increasing the assessment value of the district also reduce the rates, why should not we give the public the benefit of the reduced cost of producing the power which will follow thereon? On the other hand, if the price of coal rises, or if the manufacturers migrate from our towns out into the country in order to obtain a supply of cheaper labour, or on account of the heavy rates in the town, all of which would add to the cost of our production, why should we not then raise our prices as the rates themselves require to be raised? It is extraordinary into what a difficult position we electrical engineers have allowed ourselves to be driven by listening to popular clamour, led by the newspapers of London, who tell their readers that they have an inalienable right to electrical supply at less than cost price. The supply at its present price in London has been an unutterable boon to the community, which has been extremely lucky to get it. Shareholders and ratepayers have, as a rule, not had fair returns for their money and risks. This is not the fault of the electrical engineers, who have done good service and have carried out their work fully as well, and in many cases better in this country, and in London in particular, than in any other country in the world. I have studied this question for twenty years; I have seen the large stations in America and on the Continent; I have studied the Americanised power stations that have been established here, and I know that we have as little to learn from them as English shipbuilders have had to learn from the Americans or Germans. We, at the present time, hold the record for low cost of energy production as completely and as certainly as we hold it with the *Mauretania*.

As I have shown, I am heartily with Mr. Snell on many points raised in his paper, although there are blemishes which I should like to criticise, but it is a very valuable contribution to our proceedings as a work of reference which we shall find it convenient to dip into from time to time.

I differ from him chiefly on the point that although I support his

figures as to the small cost of producing from private plant, I do not agree with him that the superiority of the large turbo-driven plant is so overwhelming as to compensate for the heavy cost of distribution over great distances and through crowded streets, in which the distribution system is costly to be laid and costly to maintain. The President.

DUBLIN LOCAL SECTION.

DISCUSSION, *January* 9, 1908.

The CHAIRMAN (Mr. T. Tomlinson): The author has pointed out clearly both the possibilities and limitations of centralised power production and distribution. I regret that he has not given any information as to the basis upon which the tables have been prepared, and hope that he will do so in his reply, and also define his definition of load factor as he uses it. I have here a series of figures which I have worked out in connection with the Dublin and Central Ireland Power Company. I have assumed a generating station of 5,000-k.w. capacity, consisting of five 1,000-k.w. units, one of which is a spare. I will now take two cases of supply—one close by the power station the other 27 miles away. The earning capacity of such a plant is 4,000 k.w. The capital cost for a gas plant is £17·5 per kilowatt installed; therefore the capital cost per kilowatt of earning capacity is £22. To this must be added £4 for transformers and switch gear, and £5 for Parliamentary and engineering charges, a total of £31 per earning kilowatt. The cost of a duplicate overhead line is £6·5 per kilowatt, assuming the pressure to be 30,000 volts. The cost of operation, excluding fuel and distribution, is £6,600, or £1·65 per kilowatt, and distribution £0·26 per kilowatt. Assuming it is required to earn 10 per cent. on the capital outlay, the earning power per kilowatt must be:— The Chairman.

	d.
Interest on capital (10 per cent. on £31)	744
Operating expenses (10 per cent. on £1·65) ...	396
	1,140d.
Or allowing 10 per cent. loss	1,266d.

At a load factor of 100 per cent. the cost per unit would be $\frac{1,266}{8,760} = 0·145$d., to which cost of fuel must be added.

Assuming the diversity factor to be unity, then the cost per unit would be found thus: the 1,266 = hours' demand × price per unit.

Similarly, in the case of the long transmission, the earning power would have to be:—

	d.
Interest on capital (10 per cent. on £37·5) ...	900
Operating expenses (10 per cent. on £1·65) ...	396
Line charges (10 per cent. on £0·16)	62
	1,358d.
Or, allowing 2 per cent. loss in distribution ...	1,698d.

The Chairman.

The cost per unit at 100 per cent. load factor would again be found so : $\frac{1,698}{8,760} = 0\cdot194$d. plus cost of fuel. For any other load factor the cost per unit (less cost of fuel) is then given in the following table :—

Annual Load Factor Per Cent.	Distribution near Generating Station.	Distribution 27 Miles from Generating Station.
	d.	d.
10	1·444	1·940
20	0·731	0·970
30	0·486	0·646
40	0·364	0·484
50	0·291	0·390
60	0·243	0·323
70	0·208	0·226
80	0·182	0·242
90	0·162	0·215
100	0·144	0·194

In the scheme for which these figures are prepared no addition need be made for fuel costs, as the plant is intended to be driven from producer gas obtained from peat fuel, and I estimate that the recovery of the sulphate of ammonia would at least cover the cost of the peat used. The "diversity factor" where operative at the lower load factors reduces the cost per unit.

Mr. Ruddle.

Mr. MARK RUDDLE : It is all very well for Mr. Snell to criticise the heavy capital outlay per kilowatt in some of the older stations, but it is there, and the difficulty has to be faced. He seems to consider that only in a big power company can there be salvation. I, however, think it is easy to work out a very rosy result for a hypothetical power company, but in practice the business has to be got, and the best plan has frequently to be departed from. For instance, primarily one might schedule a certain street or district as good for power requirements, but later, when the undertaking comes into operation, it is often found that the demand is quite in another quarter, which necessitates deviation from the original plans, or perhaps a duplicate expenditure. I do not share the author's pessimistic attitude towards those companies and corporations who are supplying power at low rates. In many cases the power load has resulted in a great reduction in costs, and I consider that there is every hope for the existing systems. In any case it is altogether too early to condemn them. The whole paper is apparently considered from the view of the necessities of London, but personally I am not at all sure that it is not a sounder policy to divide the risks among a number of stations rather than concentrate the whole. I consider that a 5,000-k.w. station can be just as economically worked as a 25,000-k.w. one. Finally, I should like to ask the author how he determines what he terms the "correct charges" London

companies and authorities should make, which he has given in Table VII. Mr. Ruddle.

Mr. L. J. KETTLE: Mr. Snell confines his remarks and conclusions to industrial districts and towns and does not deal with the costs for private plant smaller than 20 H.P. However, in a city like Dublin, where there are few large power consumers, the suppliers are much more interested in the man who wants less than 20 H.P. The cost per horse-power-hour from these small units increases rapidly as the horse-power decreases. I think the following figures would be fair average costs for small gas plant working, as in Dublin, with gas at 3s. per 1,000 cubic feet. Mr. Kettle.

	10-H.P. Plant. Cost per Unit. d.	5-H.P. Plant. Cost per Unit. d.
Gas	1·0	1·1
Labour and stores	0·4	0·6
Capital charges and depreciation	0·3	0·4
Rent and supervision	0·4	0·6
Total costs... ...	2·1	2·7

It is instructive to note from Tables I. and II. of the paper that he has found from actual working experience that gas, both town and suction, is much more expensive than either steam or oil, and that at ordinary load factors suction gas is the most expensive motive power of the lot. I cannot quite agree with the oil-engine figures, as the price for oil on which Mr. Snell bases his costs seems abnormally low. Apart from this the figures given are very much what one would expect, and form an interesting commentary on the extravagant claims of certain suction gas plant advocates.

The cost per unit of electricity for motive power supplied from the Dublin Electricity Works, if calculated after Mr. Snell's method, works out considerably higher than the price obtained, but the Corporation engineers are still heretical enough, or practical enough, to be satisfied to take on plenty more customers on the same terms. A restricted-hour method of power supply would seem very applicable to Dublin. I estimate the motive power load would not seriously overlap the lighting peak for more than about forty hours per annun. However, a general restricted-hour tariff is seldom satisfactory when industries of a varied nature are supplied, although feasible enough with individual customers.

I do not know whether the Sunderland 1906 charges for motive power are attributable to Mr. Snell. Certainly the actual cost, as calculated on Mr. Snell's basis, would appear to exceed the average price obtained. If I am wrong in this, on account of insufficient published details, I trust Mr. Snell will set me right.

Mr. Snell sums up his view of the position by stating that the only hope of successful power supply is to centralise on a large scale. But if all his figures and principles have a general applicability, he has made out a much better case for decentralisation than for centralisation, for

Mr. Kettle. he has shown, to his own satisfaction, that no power station, however large, can compete with private plants. At a load factor of 20 per cent. he gives 0·8d. per unit as the figure (and probably the lowest possible figure) at which his huge power station can supply profitably. As against this he gives the following figures for private steam-driven plants:—

20-100 H.P.	... ...	0·806d. per unit.
100-500 H.P.	... ...	0·673d. per unit.

This does not leave much profit margin in favour of the large power station.

But when Mr. Snell demonstrates how much cheaper it is to supply oneself from a private plant than to take power from a central station on the prevailing terms, the question naturally obtrudes itself, How do central stations get any power load? How did Sunderland, for instance, manage to get 10,000 H.P. connected at the price they were charging? The fact is that, however beautiful, and apparently correct, a theoretical case may be built up regarding the ethics of power supply, there are always important factors which must be neglected because they cannot be put down in figures. There is a safety-of-supply factor, a convenience factor, a mental-and-moral-damage factor, and a dozen other factors which turn the scale against the private plant.

Speaking generally, it is of course a truism that centralisation will in general result in production at a cheaper rate, but we must take things as we now find them, and centralisation, involving the scrapping of existing plants, will not necessarily prove a good investment. The best way to tackle the problem of power supply from the ordinary lighting and power central station seems to be to instal good plant and make a courageous use of the overload capacity.

GLASGOW LOCAL SECTION.

DISCUSSION, *January* 14, 1908.

The CHAIRMAN (Professor F. G. Baily) called upon Mr. Robert Robertson to open the discussion.

Mr. R. Robertson. Mr. R. ROBERTSON: I think the author is to be congratulated upon producing a paper raising so many important points in connection with the cost of power. With the methods adopted by the author, and, generally speaking, the conclusions arrived at, I have very little to take exception to. I think, however, that all through the paper the bases upon which the prices were given are too low, particularly in the case of the Tables I. and II. The author has very properly pointed out that the examples given in the second section of the paper are in most cases higher than these tables, but I think that the difference between them is so enormous that it cannot be passed over merely with a simple remark. In order to make a comparison between these I have had a diagram prepared (Fig. G), made out practically in the same form

Mr. R. Robertson.

as that shown in Fig. 2 of the paper, the ordinates of which represent the cost per unit and the abscissæ the load factor—the two lines marked on the right hand "Table I." and "Table II." respectively represent the figures given in the table which is headed "The Ascertained Cost of Power per Unit Generated for Independent Plants"—in the one case for installations up to 100 H.P., and in the other case for installations from 100 to 500 H.P. The dotted line marked "interest and depreciation" is calculated on the basis that the cost of the station per kilowatt of

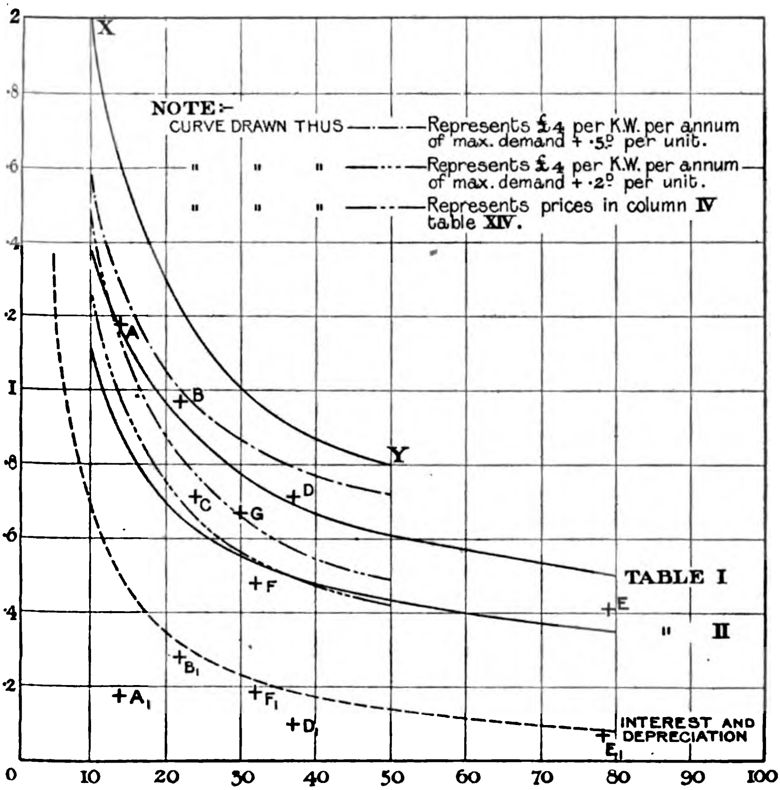


FIG. G.

maximum demand is taken at £25. That figure is approximately the average of the figures stated in the table on page 296. I find on referring to the original paper by Mr. Williamson, from which that table was taken, that these figures are the cost per kilowatt installed, and in taking them as the cost on the maximum demand, I am taking the more favourable view, from the point of view of the independent user. I have verified these figures by looking up examples of recent plants of various sizes. The examples which I have been able to lay hands upon were

Mr. R. Robertson.

for plants for abroad, and I was able to get the figures for plant only, exclusive of erection, foundations and buildings, so that something must be added to the following figures to account for these items. Taking suction gas plants varying from 105 to 175 B.H.P., the average, including the whole generating plant, varies from £13·25 per horse-power to £14. By adding a third on to that it is brought up to nearly £20, and by making an allowance for foundations and buildings, and adding a proportion for spare plant, it will be found to agree very nearly with the figure given on the table referred to. Another engine-driven plant of 270 H.P. comes out about £15 10s. per brake-horse-power. That again brings out a figure of about £20, exclusive of foundations and buildings. A steam plant of 400 H.P. comes out about £13·5, a little lower, owing to the slightly larger size. I wish to compare the curves representing the tables with a few of the examples quoted by the author in the paper. The point A on the diagram (Fig. G) represents the example of a shipyard given on page 294. Although the plant is considerably larger than that represented in Table I., namely, plants up to 100 H.P., the generating cost is the same as in that table, and the proportion of it representing interest and depreciation is considerably less than half what it should be if the cost of the plant is something like the average we have assumed of £25. That arises from the fact that only £60 is taken for interest and depreciation for four months, and taking this as 10 per cent., it shows a cost of £1,800 capital cost for the plant, or only £7·95 per kilowatt. The point marked B represents the example given on page 295—a shipyard and engine works—and although this is 1,000 H.P. the cost is enormously greater than plants of 100 to 500 H.P., as indicated in Tables I. and II. The next example, C, is also for a plant of 1,000 H.P., and the cost of it lies between the cost of the small plants and the cost of the 500 H.P. The next point, D, represents one example taken from the table on page 296, 640 k.w., and it again is very much higher in cost than the table for plants up to 500 H.P. The point E is a better example, with 80 per cent. load factor. That is taken for costs for a steam plant and not for an independent generating station, and as it is difficult to arrive at the average amount consumed by steam engines, I think it is very difficult to accept figures of that sort. I have had a case recently of a plant with a power very much like this, where it was running six days a week, and I was informed that the engine was practically running at full load all the six days a week, which would give a load factor of something like 80 per cent. It was converted to electrical driving, and when the actual figures were taken out, the load factor was only 50 per cent. Point F is a jute mill. It comes slightly under the 500 H.P. cost, although the power is over 900 H.P. From this it is evident that with much smaller plants the cost in reality should be very much greater, and I am doubtful whether in many cases of small installations up to probably 400 to 500 H.P., the actual cost—if everything is taken into account—comes much under the line marked X Y. Commencing at 2d. per unit at about 10 per cent. load factor, it

Mr. R. Robertson.

comes down to 0·8d. at 50 per cent. load factor. I have several figures that closely approximate to that line. The next part of the table to which I should like to refer is the other side of the question—the cost at which power companies or power suppliers can supply. The figures indicated by the author are based upon a total cost of £25 per kilowatt. It would be very interesting if the author would give some particulars of the size the station would require to be and of the size of units before this cost could be reached. I have seen many estimates like this given, but up to now there has been no generating station of any size supplying power in this country the costs of which could nearly approximate to that price, and in discussing the question of supplying current, we must take the costs approximately that are in existence. The only example of power supply given by the author is the Newcastle Power Company. In that case the capital cost is given at £59. The most reasonable method of charging for supply is, I think, to make a charge of so much per kilowatt per annum, with the addition of a small amount per unit. The figure at which companies can supply may be taken at £4 per annum per kilowatt of maximum demand, with a further charge per unit ranging from 0·5d. down to as low as 0·2d., the reason for the difference being that a great many cases depend on the load-factor of the consumers, and the way the power is taken or the time at which it is taken. I have indicated on the diagram two lines, the higher one giving the maximum of these charges and the lower one giving the minimum, between which a rate may be found that is at the same time profitable to the supplier and—in most cases up to 500 H.P. at least—also profitable to the consumer. Between these two lines I have indicated by a dotted line the figure given by the author as the real cost of the Newcastle Company based on the 1907 figures. I merely give that as a comparison. There are a great many other points in the paper which might be discussed in detail, but, so far as I am concerned, I generally concur in most cases with the author. I have examined the costs of sub-stations, and whilst I think the prices are taken rather on the low side, the difference between them and real costs which I have recently incurred is not so great as to warrant taking much exception to them. The question of distance to which a supply company can distribute current is one of the most difficult ones, and I hardly think it is ripe for solution, as there has not been sufficient experience in this country—at least of distribution—to enable any of the companies to come to a definite decision as to the limit of distance. In dealing with the limit of distance we have also to deal with a question of average. If a company extends a distance of 20 miles, it also has probably a much larger proportion of consumers, if the situation of the station is well designed, within a very short radius, and the economic distance depends on the density, taking the supply as a whole. If each individual case is first taken, then, of course, it will be necessary to differentiate in charge between consumers near the generating station and consumers far away. That, I think, is not a reasonable ground for

Mr. R. Robertson.

differentiation in charge, because it is not the fault of the consumer but the fault of the situation of the generating station, and the differentiation between charges among consumers should be based upon the different value of these consumers' operations to the supply. Apart from the slight exception I take to the scale of the author's charges, I quite concur with him in thinking that he has shown that the claim which he makes in the second paragraph of page 318 of the paper is reasonable.

Mr. Lackie.

Mr. W. W. Lackie: I have to admit that when first I read the paper I was rather disappointed with it, but after a second reading I began thoroughly to appreciate all the matter the author has put before us. Not only so, but I gave the paper to my colleagues in the electricity department, and they have already had one or two most interesting little discussions upon it. The object or purpose of a company or municipality in putting down a central electric generating station is undoubtedly to obtain a benefit by the joining up or connecting of the demand for power in the district. For instance, take the supply of electrical energy to hoist motors. The load factor of an ordinary hoist motor in a city is under 9 per cent., but by all the hoist motors in the city getting their supply from one source the supply authority is enabled to give the hoists energy at a rate per unit corresponding to at least a 20 per cent. load factor. The same reasoning must apply to the industrial user, and therefore it appears to me that a maximum price for electrical energy for power or industrial purposes might be fixed at a price corresponding to 20 or 25 per cent. load factor, and the price thereafter should be made to meet a better load factor than 20 or 25 per cent. Those 10 per cent. load-factor users cannot generate power at a rate to compete with a 20 per cent. load factor if all considerations are taken into account. I will not take up time elaborating the points with which I agree, but will rather confine myself to a few notes on matters regarding which I am inclined to differ. I maintain that the cost of supplying from a town installation or power company should be less where a user is taking his supply from one or other of these sources. If we consider that the whole object of a central station is to specialise the generation and supply of electric energy for sale to the outside public, I think we should naturally expect that it would be done cheaper and better than the private user can hope to do with his own plant if all considerations are taken into account. On page 289 Mr. Snell gives certain economical axioms which to my mind are self-evident facts. Taking No. 1, surely the critical load factor and size of plant with which any outside supply can hope to compete against the user's own local plant depend entirely on the size of the outside supply plant. On the same page Mr. Snell indicates that the cost of lighting is not likely to fall below 2d. per unit as an average cost, and that it would always be higher than the cost of the supply for power both because of its lower load factor and also because of the greater cost of low-tension distribution. The average load factor for lighting purposes would be about 12 per cent., but in my illustration of the supply of power to

Mr. Lackie.

hoists I showed that the effect of a diversity factor, which one cannot hope for in a supply for lighting even with a 12 per cent. load factor, is most important in its bearing on the price. On page 290 Mr. Snell states that many electricity undertakings are supplying power at any price, or at least at less than cost price. I must emphatically state that in Glasgow we are not doing that. On pages 291 and 292 ascertained costs are given of power for one unit generated for independent installations. The outstanding fact in the figures given is that the cost of steam is less in every case than the cost of suction or producer gas plant. That is contrary to all the data that have been put before me. The example given on page 294 of a gas plant contains apparently a serious error as to its capital cost. It is stated that interest and depreciation would amount to £60 for four months, or £180 for a year, so that the capital cost of the plant would be £1,800, or £8 2s. per kilowatt. I am perfectly certain that that plant was never put down for £8 2s. per kilowatt, and in support of that I would point out that in every other case stated by the author the cost ran between £15 and £20 per kilowatt. On the same page it is stated that the gas consumption was 36 cub. ft. per kilowatt-hour—a large figure even on so low an annual load factor as 14 per cent. I am of opinion that the annual load factor has nothing whatever to do with the gas consumption, but only the plant load factor; and from the fact that the plant load factor is 48 per cent. (a very good percentage), the only conclusion that one can come to is that 36 cub. ft per kilowatt is about the average figure that one gets with gas-engine plant, instead of the figure of 15 to 20 cub. ft. quoted by gas-engine makers. It really amounts to this, that the cost of gas per unit with gas at 2s. 4d. per 1,000 cub. ft. is about 1d. In all the examples stated, of course, Mr. Snell has not allowed for land and buildings, or rent, rates, and taxes. The colliery load factor may be looked upon as about 36 per cent. In steel works and other similar works the annual load factor is about 20 to 25 per cent. But it must not be forgotten that with a public supply we are able to offer a consumer having an annual load factor of 20 to 25 per cent. a rate corresponding to a load factor of about 40 per cent., owing to the fact that some of the plant which is used during the day to supply the day consumer is also used during the night for supplying some night consumers. On pages 301 and 302 the author discusses the average load factor in Sunderland and puts it down at 10 per cent., but to my mind that is a mistake. I have added up the horse-power installed given on page 290, and the annual units, and have found that, allowing for a diversity factor of 2·4, 2,000 k.w. of plant supplied 5,000,000 units, and thus there is a load factor of something like 30 per cent. On page 303 Mr. Snell states that the modern power station of important magnitude and favourably situated can be now equipped completely for £12 or £13 per kilowatt installed. That does not agree with the modern power station of Carville (referred to later on), where the capital cost was £59·6 per kilowatt; and it is of interest to note that the Newcastle charges, with their capital cost of £59·6 per kilowatt, compare favourably with the

Mr. Lackie. theoretical London County Council figures, with an alleged capital cost of only £25 per kilowatt. The fact is that the horse-power of motors installed in a factory has a diversity factor of about 2, and the result of an individual diversity factor of 2 divided amongst all the consumers gives a diversity factor on the station of about 3. In the tramway supply the diversity factor was about 10, that is, for every 100 k.w. of motors only 10 k.w. of maximum demand is made upon the power house. Hoists have about the same diversity factor as a tramway supply, and each trade has its own diversity factor. Probably the worse the load factor the greater the diversity factor. On pages 305 and 306 Mr. Snell discusses the cost of different transmission systems in third stations, and quotes the figure of 85 per cent. efficiency for rotaries and 95 per cent. for static transformers. He advocates throughout his paper that the supply should be alternating current taken from a static transformer; but he never mentions in connection with that the effect of these transformers on his power factor and the wattless current and the loss in cables, which is a very serious matter. Further, it is advocated that works having 100 k.w. or more should each have their own local static sub-station. It strikes me that that would add tremendously to the cost per kilowatt, and that the cost quoted would be probably doubled. On page 308 Mr. Snell says, after boldly stating that many London companies are selling energy under cost price for power purposes, that "This means that the power user is being supplied by these undertakings at the expense of their lighting consumers to pay an unduly high rate for their supply, or, in the case of certain London municipalities, at too high rates for public lighting." If it was not for these power consumers, in Glasgow at least, the coal consumption would be about 7 lbs. per unit., whereas to-day it is under 4 lbs. If it was not for that sale of current for power purposes, we should require something like ¼d. per unit more lighting to make up for the difference in coal consumption. Mr. Snell does not indicate in any way whatever how he arrives at the correct charge to be made by the London local authorities and larger municipalities, and I must say that I do not at all agree with the figures he has given for Glasgow. On page 311 the average and mean columns simply represent the mean of the five figures given in the five columns. Now, the probability is that in Manchester there are ten times as many units consumed as there are in Nottingham, and consequently to get the true mean one would require to take the number of units sold at 5d. and the number sold at 4·9d. in Nottingham, and similarly with Leeds and Glasgow, and so get the true mean or average. The conclusion of Mr. Snell's paper is a most unfair one. He states that, looking back, it would have been much wiser if the authorities, namely, the Local Government Board and Board of Trade, had prevented some of its smaller local boroughs from adopting separate electricity stations. I do not agree with that statement at all, for if the Local Government Board and Board of Trade had not allowed the smaller local boroughs to do so, a company would have stepped in and done the work, and the companies would be in no better position to-day

than those small local boroughs. Further, those local boroughs have done pioneer work, and given a supply while the demand for electrical energy was growing and the public being educated to its use. Would a power company ever have erected a large works in the vicinity of those boroughs if the boroughs had not first shown that there was a demand and profit to be made from the supply of electrical energy? Some people might think that the Corporation of Glasgow should have started away with a station such as they now have at Port Dundas. If they had, I do not for a moment believe that their revenue would have been more than it was for the first three or four years with their Waterloo Street station, and if that had been the case the revenue would not have paid interest and sinking fund on the lands and buildings and machinery put down. The undertaking would further have been put down as a financial failure, and would have acted as a stumbling-block to other electrical undertakings. I believe that the public have to be gradually educated to understand and realise the economy and convenience of the use of electrical energy for power purposes, and that they will not, immediately a big station is built, throw aside their existing forms of prime movers and adopt electrical driving. The industry has to crawl first before it begins to run as it is running to-day. I am afraid also that the remedy Mr. Snell suggests would not prove a beneficial one to the power companies, although it is quite correct. Those smaller local boroughs would only call upon the power companies to supply their peak load in the winter months, and instead of a load factor of 30 or 40 per cent. it would be more like 2 or 3 per cent. for some years to come. Mr. Lackie.

Mr. J. A. ROBERTSON: With regard to Tables I. and II., I must congratulate Mr. Snell on having been able to obtain so many data regarding the operation of private plants. I have been interested in the question for some years, having seen a number of private plants shut down in favour of public supply, and I have invariably found the greatest difficulty in obtaining reliable data as to the cost of production of these plants. When figures have been supplied, further investigation has shown them to be of very little use for purposes of comparison. In view of the varying conditions in shipyards and similar works, I could hardly see at first the object of introducing the figures showing the rates of horse-power installed to the number of men employed, but out of curiosity I obtained the corresponding figures for several works in Greenock and find that these correspond surprisingly with the figures supplied in the paper. Mr. J. A. Robertson.

Mr. Snell discusses at length the best system of supply, and I think most of us will be inclined to agree with his conclusions. I think that at Sunderland he has done the right thing in adopting a high-tension transmission system with 2-wire low-tension distribution, and although the cost of the low-tension mains may be somewhat greater, the resulting simplicity of a system with no complication of balancers and the troubles that arise from a middle wire at earth potential are easily worth the cost. If we could start again where we were fifteen years

Mr. J. A. Robertson.

ago, I fancy there would be very few engineers who would instal a 3-wire low-tension continuous-current system in any but the smallest towns. An important advantage of multiphase high-tension transmission is that existing installations can be connected to the mains without altering their voltage, and that a fluctuating load can be supplied without any abnormal disturbance in the supply to other customers.

I am of opinion that Mr. Snell is too pessimistic regarding the future of the small or medium size undertaking. I can remember when he looked at this question somewhat differently, and would hardly have told us that the small or medium size stations were to be swallowed up by the power companies. He has, in my opinion, made the mistake of generalising from his experience in London, where the conditions are quite different from those existing in the provinces. Where there are twenty odd stations within a radius of ten miles, the possibilities of economy by putting the generating plant down at one centre are of course enormous, provided that such an undertaking is not burdened with heavy capital expenditure in buying out existing interests, but in the provinces the problem is entirely different, and it will be found that the existing power companies are in no better position—sometimes, indeed, in a much worse position—than the small stations owned by municipalities. The cost of transmission over a considerable distance neutralises the economies of generating on a large scale, and in one case I know where a high-tension supply is given, the proportion of units sold to units generated is only 56 per cent., the remainder going in distribution and transformer losses.

It is hardly fair, either, to take the figure of £70 per kilowatt, the average cost of provincial undertakings, and to compare it with the estimated price per kilowatt for a much larger station. Mr. Snell's average price includes sums which have been paid in towns like Liverpool, Sheffield and Birmingham to buy out the older company. The cost per kilowatt is only of interest in so far as it affects the total cost of production, and many of these provincial undertakings have already repaid a considerable proportion of their capital, so that the standing charges, at present, are not paid on £70 per kilowatt, but on a figure very much less. In Greenock the capital cost per kilowatt has been slightly over £43, but the actual payments to-day are made on £36 per kilowatt.

I do not think that the future success of the smaller stations depends necessarily on their securing the few large power users who may happen to be within their area, and are probably quite satisfied with the results of generating from their own plants. There is still an enormous lighting business to be developed by station engineers, and the introduction of metallic filament lamps is going to help us very much in this direction. The small power user is bound to come to the central station, and with a traction supply, which is often to be found on small stations, there is no reason whatever why these undertakings should not be able to do even better than they have done in the past. The big power companies, on the other hand, have not shown results equal

to the anticipations of their promoters, and, in one or two cases, have been disastrous failures. Mr. J. A. Robertson.

ADJOURNED DISCUSSION AT GLASGOW, *February* 11, 1908.

Mr. F. A. NEWINGTON: I think at the last meeting the figures in the paper were fully discussed, and I do not propose to say very much about them. The paper generally tends to show that most of the existing undertakings are going to the bad, that our plant will all have to be scrapped very soon, and that general ruin stares us in the face. But after examining the first table I personally feel rather more cheerful, because it shows that suction gas plant is not going to be the serious competitor that we expected a few years ago, and that suction gas engines cannot be run any cheaper than engines using town gas. Electric motors displace town gas plant every day of the week, so probably the same will happen to suction gas plant after it has been running a bit longer. Table I. does not allow for supervision, general establishment charges, or for rating. I suppose supervision is meant to cover ordinary running charges and wages. It is interesting to compare the first two instances of actual tests given on pages 292 and 293, where the wages for the motor are put at 0·1d. per unit and for the gas plant at 0·412d. If wages have to be added in Table I. in the same proportion, the figures will be very much altered. Mr. Newington.

I have some figures of a suction gas plant with me. There are two suction gas engines, each of 90 H.P., and each driving a dynamo to supply power for engineering works. One engine runs about ten hours a day and the other four to five hours a day. I do not know anything about the load factor. The works were previously supplied from the Corporation, and the combined cost per unit for lighting and power was 1·2d. The following are the costs for the twelve months ending December 31, 1907:—

	£
Wages	100
Coal...	133
Water	31
Oil waste, etc.	31
Repairs	8
Insurance	24
Total running cost ...	£327

The capital cost for the two gas engines, dynamos, etc., is £3,680. Ten per cent. on that for depreciation, interest, and so on, is £368, giving a total cost of £695. That works out at 1·1d. per unit. The value of the land for the engine house is not taken into account. I think that 10 per cent. for depreciation and interest is on the low side, because in a manufacturing concern of that sort at least 5 per cent. or more should be allowed for interest if the money had been put into the concern. The maintenance, of course, is extremely low, because the plant is

Mr. Newington. quite new. These figures show, in that instance at any rate, that a private gas suction plant is not any cheaper than the electricity supply from the town mains.

On page 303 Mr. Snell states that there is an enormous scope in this country for electrical application, and in Greater London alone there are factories using 400,000 H.P. But is not the greater part of that used in comparatively small works? London is not a very large manufacturing centre, so that the cost per unit, even if it is 1d., is not a very serious proportion to the total cost of the works. Is it reasonable to expect that the whole 400,000 H.P. must be driven by motors immediately? Is not 29,000 H.P. out of the 400,000 H.P. in ten or twelve years a fair proportion? I am afraid I cannot agree with Mr. Snell in saying that Greater London, cleared of factory chimneys, would be a very much better place to live in. I think the ordinary house chimneys are more the cause of fog and smoke than factories. I am not quite certain that the present type of motor-bus is an improvement on the old horse-bus. I do not think that cheap supply of power will prevent the migration of power users, because land, wages, and rates are very much lower in other places, and therefore large works are bound to leave London. Mr. Snell seems to assume that every town in Great Britain is full of manufactories, and that high tension must be used. He also sympathises with those towns having only low tension. But, fortunately, the majority of towns in Great Britain are not manufacturing centres, and a large number of them, at any rate, can do quite well with low-tension current. In Edinburgh we have not found any necessity for high-tension transmission. We have some 9,000 H.P. of motors connected. A good deal has been said about the diversity factor. A high diversity factor will reduce the load factor, and so may not be an advantage. For combined lighting and power undertakings, it seems to me that we want a diversity factor of 1 during the greater number of hours of the day, and as high as possible during the one or two hours of peak load. It is only during the hours of peak load that a high diversity factor is of any use to us. It is not very easy to find out what the diversity factor is at the time of top load.

Section IV. of the paper criticises the prices charged by existing undertakings in London, and shows that they are all gradually drifting into bankruptcy; and then Mr. Snell proposes as a remedy that a large power station must be erected at a minimum of cost, but without prejudicing the present capital involved in the existing undertakings. I should very much like to know how this can be done. The Electric Lighting Act of 1882 provided that the local authorities could take over the supply from companies after a period of twenty-one years. That was found to be too short a time, and in 1888 the time was extended to forty-two years. In London, companies then commenced work, but each district was given over to two companies, so that there was competition between those two companies and also with the existing gas companies. But now there is a fourth competitor coming in, and apparently making matters worse. In 1888 the conditions were very

different from what they are to-day. The plant could only be made in small units; but with the prospect of forty-two years' life in front of them, the pioneers took the risk, and now some of them are reaping the benefit of their pluck and foresight. Is it fair now for that fourth competitor to come in and upset the arrangements of 1888? Mr. Snell seems to think that London is urgently and suddenly in need of 400,000 H.P. of machinery electrically driven, and unless it gets this its trade will vanish. I would ask him not to be too hurried. At the end of the forty-two years the local authority can buy up the existing undertakings and start afresh. Mr. Newington.

Mr. F. C. RAPHAEL: What Mr. Newington has just said makes me think there is one point of view that is not always taken sufficiently into consideration in comparing the electric lighting laws and the practice of 1882 and 1888 with the conditions of the present day. In order to supply in the most efficient manner, the companies must scrap their old plant, and get in very much larger plant, and, in London, all the Provisional Orders expire in 1931, which gives a very short time in which to write the plant off. On the other hand, I must say Mr. Snell's estimates of what the London supply companies ought to charge are based on a different point of view from that which the companies ought to take. Assume that a man is going to manufacture something, and by using a new method of manufacture, the economy of which depends on a large output—say, a million articles a year—he is going to manufacture at a far cheaper rate than hitherto. Naturally he has to introduce his product on the market. He cannot hope the first year of his business to produce and find a sale for a million articles at once. On the other hand, as his business absolutely depends on producing articles at a reduced price, he would never get to his million articles in the year and his profits, unless he started at the cheap price. The consequence is, that if he starts selling at the actual cost of production at first he will never be cheaper than anybody else, and will never get to his big output—he would have to shut down his plant long before he reaches it. It is practically the same with the power supplier, be he power company or local authority. It is no use his trying to sell at the high price at first, because then he is supplying at a price at which it will not pay people to buy. If he is going to supply at a much cheaper price than the consumer can generate at with his own plant, in a very short time he will be able to produce as much as he possibly can and reach his remunerative output. I think from that point of view the present-day undertakers are perfectly right, and in many cases, as Mr. Snell's paper shows, it would pay them to put in larger plant at once and to re-model their stations if it were not for this terrible purchase clause, and they would then be ready to supply at even lower rates than they are doing now. Mr. Raphael.

Mr. J. K. STOTHERT: I had intended to gather some figures together, but have not had the time to do so. I can only give the costs of generating electricity at our own works. The works costs worked out at between 0·25 to 0·3d., varying week by week, and if we add to that Mr. Stothert.

Mr. Stothert.

the interest and depreciation, the total cost works out at 0·46 and 0·45d. per unit. Our load factor is about 60 per cent. There is another plant in the neighbourhood of Johnstone, a paper-mill, Messrs. Watson, of Linwood. Their load factor is, I believe, about 70 per cent., and they have often told me that their works costs, including the 7 per cent. interest on capital, worked out at 0·19d. I do not think it would pay any company to supply cheaper than that, because these particular works are in an isolated position and it would mean a connection of several miles in length with the load at the end.

Mr. Parsons.

Mr. T. C. PARSONS: There is one thing on page 292 I have not heard anything about, and that is the cost of docking these vessels. I think Mr. Snell took a very unfair method of comparison. He should have taken as the basis the foot-ton of water pumped. In the case of the electric drive Mr. Snell has a vessel of 2,600 tons, and he pumps 3,875 tons of water. In the case of the gas engine and with a vessel of 1,760 tons he pumps 10,632 tons of water. If we take the 62,000 foot-tons of water pumped in the case of the electrically driven dock, that works out at 0·0135d. per foot-ton pumped. In the case of the gas engine we have 180,744 foot-tons pumped, which work out at 0·01017d. per foot-ton, so that really the gas-engine pump is about 33 per cent. better than the electrical one. The case for the electrically driven pump might have been made much more favourable on the line of comparison Mr. Snell has taken if he had had a 4,000-ton vessel in the electrical dock and a 1-ton vessel in the gas engine dock. On the last page Mr. Snell says that it might have been better if the Local Government Board and the Board of Trade had prevented some of the smaller local stations from putting down their own plant, and if they had taken bulk supply, but the places that have done that so far have not come out particularly well. I have taken a note of these places from the *Electrical Times*—there are seven corporation stations and two company stations that are supplied in bulk. The first is Acton, which in its first year made a loss of £2,615, which was rather a big loss for the first year's working when they had not got their generating plant put down—they put aside 2·79 per cent. for depreciation; Alloa in their fourth year made a loss of £490. Bridgend the second year made a profit of £122; Rugby in the second year made a profit of £3; Wednesbury in the second year made a loss of £770; Willesden in the third year made a loss of £1,262; and Wishaw in the first year made a loss of £593. In the case of the company stations, the North Metropolitan—the three stations lumped together, one in the sixth year, one in the fifth year, and one running for three months—they made a loss of £171, and put aside for depreciation 0·79 per cent. of their capital. Chislehurst in the seventh year made a profit of £446, and put aside for depreciation 1·15 per cent. So far the examples of places being supplied in bulk are not very favourable.

Mr. J. A. Robertson.

Mr. J. A. ROBERTSON: During the discussion last month, I referred to the difficulty I had experienced in obtaining figures of the generation costs for private plant. In the interval I have obtained some figures from a shipyard plant which may be of interest. The plant installed

consists of two sets of a somewhat old type, and cost approximately £4,000. During last year the current generated amounted to 360,000 units, and the cost comes out at—coal, 0·47d. per unit; wages, 0·22d. per unit; repairs, 0·21d. per unit, making a total of 0·98d. per unit running costs. There is another point to which I would like to call attention. One of Mr. Snell's interesting tables shows the relation between horse-power installed and the number of employees in certain works. If we take No. 7, where the units per capita are as high as 1,012, and where, I understand, the whole of the works is electrically driven, and if we calculate the relative cost of current and labour, we find that the power cost is only a very small percentage of the labour cost. In this particular case the ratio of power cost to labour cost is only 5·6 per cent. In order to check this figure, I have obtained particulars from one of our largest consumers in Greenock, and find that in a combined shipyard and engine works where the machinery is exclusively driven by electricity the annual wages bill is no less than £130,000, while the power cost is slightly under £4,000 per annum. My point is, that having regard to the freedom from breakdown which a public supply ensures, the question of saving in power costs is a much smaller matter than we are led to believe, and the problematical saving to be obtained from any kind of private plant is hardly worth considering when compared with the losses which might accrue through a failure of supply. Most manufacturers will find that under these circumstances their capital can be more usefully employed in other directions than the purchase of costly generating plant. Mr. J. A. Robertson.

Professor F. G. Baily: The paper gives rise to many thoughts which in some cases suggest criticism. For instance, Mr. Snell strongly recommends the use of 3-phase current in the network on the score of the greater economy of the static transformer over the rotary convertor. But he, with some others who consider schemes for supplying power on large lines, does not regard sufficiently the desires of the consumer while he is saving small fractions of a penny from the works cost. One-tenth of a penny is a considerable portion off the works cost, but it is very little off the consumer's bill, and a less efficient or less convenient motor will more than counterbalance the saving. For the small uses, such as small lathes, fans, dentists' drills, lifts, the charging of batteries, and the various household applications of a motor, the direct current is usually more convenient, while for the important item of arc lamps the alternating current is a nuisance. Professor Baily.

The tables of the cost of power in private plants from various sources are very interesting, but such average figures are rather misleading. For example, where steam engines are used, steam is often required for other purposes to a large but very vague amount, so that it is impossible to allocate the cost of coal, stoking, cleaning and repairs, standby losses, etc., with any accuracy. The cost of oil engines in the tables is surprisingly low, and in practice I have found them a good deal more expensive per unit generated, and more costly than anything else except for small powers and intermittent work.

Professor Baily.

Another and more important matter, which Mr. Newington touched on, is the recommendation that the largely increasing power load should be supplied from new bulk supply stations, on the ground that the old lighting stations were so overburdened with old-fashioned plant and capital expenditure that they could not supply power cheaply enough, and he further accuses them of already supplying power at unremunerative prices. There seems to be a fallacy here. The old stations were originally almost purely lighting stations, which supplied current at a price which people were willing to pay on account of their preference for electric light. At the present time many of them have a large power load, for which they have put down modern plant, and the capital charges for the power supply should clearly be the cost of the plant bought for the purpose, and not the average cost of the whole plant in the station. Hence if their power load increases greatly, they will put down more plant at still cheaper prices, and will be able to reduce their charges still further, instead of being ruined at their present rates, as Mr. Snell foretells. If a new company owns the power-supplying plant, Mr. Snell would allow them the benefits of modern equipment, but the fact that the plant is all under one roof can make no difference to the accounting and the profitable working of the new part.

I cannot help thinking that the paper suggests the usual outcry against monopoly, as soon as some enterprising people, who have risked money on an untried scheme, begin to make a profit, after buying at a heavy price the experience that now enables them to gain success. Others want to come in who have paid nothing for experience, and they point out how much more economically they could supply the public, because they are able to appropriate the knowledge and business labours of the pioneers. This gain to the public savours somewhat of the repudiation of a debt, an obvious immediate benefit, but a dangerous policy for the future.

LEEDS LOCAL SECTION.

DISCUSSION AT SHEFFIELD, *February* 17, 1908.

Mr. Yerbury.

Mr. H. E. YERBURY: It appears to me that some of the axioms and tables submitted by the author are open to criticism, and in the first place I should like to ask for a clearer definition of the author's view of diversity factor, say on power load. Is it based on motors simultaneously running or the total horse-power installed? For instance, on the Sheffield tramways we require about 150 k.w. plant for 400 k.w. motors on cars. That is, of course, under ordinary conditions, at times of peak load. This would give a diversity factor of 2·6. But assuming we have, say, a snow-storm, then the diversity factor drops to unity, and I contend that plant is required to cope with this emergency. In respect to Tables I. and II., I am rather surprised to note that the author has found that the generation of power by steam plant is cheaper

than by suction or producer gas plant at all load factors, but I observe that he bases his figures on town's gas at 2s. per 1,000 cubic feet, whereas in Sheffield it is 1s. per 1,000 cubic feet, which would obviously improve the gas plant figures. The probable cost of buildings, rates, and taxes, is not mentioned in the paper. I agree with the author that a large power station could not be equipped completely for less than £12 or £13 per kilowatt installed. Certainly in the majority of municipal stations it is more than double this estimate, and the important question remains as to the annual amount to be set aside for renewals and depreciation. There must still be diversity of opinion when we find that here in Sheffield current is sold as low as 0·6d. per unit, and in Leeds, I believe, 1d. per unit is the lowest price that current can be purchased from the Corporation mains. As in both cities the tramways have their own generating stations, it would be interesting to know whether the diversity factor or the standing charges differ considerably, or whether it is the policy of administration that determines the great difference in price. Personally, I am inclined to think that a power company could not generate at much less than 0·36d. per unit or sell at less than 0·6d. per unit unless the station was run with a very high load factor, which is not usually attained during the first few years. On Table VIII. I observe that Sheffield's low costs are omitted. Perhaps the author will tell us why this is so. Mr. Yerbury.

Reviewing the paper as a whole, it seems to me that the author's scheme for a large power undertaking or centralisation shows up in a more favourable light in the neighbourhood of London, and I am inclined to think that the majority of engineers in the provinces will not favour the author's proposal to stop all extension work. In my opinion there is absolutely no hope of large tramway undertakings or large iron or steel works taking current from a power company's supply.

Mr. WILSON HARTNELL: I am of the opinion, as the result of considerable experience amongst manufacturers, that where existing factories are already supplied with efficient power plant, they can generate power for themselves more cheaply than it could be obtained from outside sources. At the same time some owners would prefer to avail themselves of outside power supply, even at an enhanced price, rather than withdraw a large amount of working capital to sink in extra plant. To avoid this they might be prepared to pay up to 1d. per unit, although they might otherwise generate for themselves at, say, 0·6d. Mr. Hartnell.

In one instance over 1,000 H.P. was installed in 2-phase motors with an average load of from 600 to 700 H.P. At about 1d. per unit the annual charge was nearly half the cost of a private power station. In consequence plans and estimates were prepared for one, but on a considerable reduction being made in the cost for power the idea of a private station was left in abeyance, although the reduced cost of electricity was higher than could have been supplied from their own plant. In another instance over 1,000 H.P. was installed in

Mr. Hartnell.

3-phase motors with a working load of about 500. Here the cost of power was little more than 0·75d. per unit. The idea of a private power station has not been entertained. In each of these examples the pre-existing steam plant was removed.

New factories recently built are in many cases receiving their power from a public electric supply station.

The cost of a private supply of electricity on a smaller scale is often considerably enhanced by the wages of the men required to look after the engines, boilers, etc. This is often overlooked, as also the value of the space to be occupied. Thus there is considerable scope for a power company supplying electricity at less than 1d. per unit, except in large ironworks, where it can be often supplied at less than ½d. per unit.

Mr. Fedden.

Mr. S. E. FEDDEN: I am very much indebted to Mr. Snell for having taken so much trouble in getting all these figures together, as they are extremely interesting. As the last speaker said, there are a great many of them which look all right, but some of them are not exactly to our way of thinking in Sheffield. I do not think the power companies could come here and supply at anything like the price we are supplying current at. On page 389, Mr. Snell says, "There is generally a critical load factor and size of plant within which no outside supply can hope to compete with a user's local plant—that is, at and beyond this critical point a user can produce more cheaply from his own plant." How many works are there that have such a load factor? I will just give some particulars of my customers. One takes 600 H.P. in a steel works, and the load factor is 14 per cent.; another with 1,000 H.P. has a 7 per cent. load factor; a rolling mill with 300 H.P. has an 18 per cent. load factor, and so I can go on down to 13·14, and some as low as 5 per cent. These works, I claim, could not put down their own plant, with spare plant, and supply themselves at even double the price that I am charging them, but it does not necessarily follow that we ought to charge them with a higher price because their load factors are so low. We get from 40 to 45 per cent. all day long. I consider that our figures for power supply should be calculated out on the plant load factor. It is not fair that we should charge customers for power a price sufficient to recoup ourselves on the large and partially experimental expenditure which was paid to the former company for its lighting station. About £200,000 was paid for goodwill, but why should we charge that goodwill amount on the price to new consumers? If we did, I am afraid we should not get one of them on our mains. We charge our customers on the present capital expenditure and on the load factor of the plant with which we supply them, otherwise of course we should not supply at all. In Table I. I notice the author has calculated all the town gas price at 2s. per 1,000 cubic feet, and he gets some very low figures. What would be his figure with gas at 1s. per 1,000 cubic feet, as in Sheffield? All his figures would be about half, and yet I still keep putting on power from 20 to 50 k.w. per week.

In Table II. Mr. Snell gives some figures relating to suction gas

producer plant. I suppose this refers to producer plant complete. If the figures were obtained with suction-producer gas they must have been very perfect producers, perfect engines, and perfect men. I have had a few instances of these users, one with 200 H.P. and another with 400 or 500 H.P. I tried to get them on our mains some four or five years ago, but they would have nothing to do with electricity; gas-producers were the only things. Since then, however, they have taken into consideration the wages they paid for attention, the lost time, the explosions and fires they have had, and the fact that when they wanted a little extra power they invariably could not get it owing to bad gas. The consequence is that I am now supplying the whole of the power for these works. I understand that their cost ought to have been $\frac{1}{10}$d. per B.H.P., but taking into account all the above disadvantages, I believe it actually worked out nearer 2d. Of course, these two cases may have been exceptionally unlucky. Mr. Fedden.

With reference to the steam at 70 per cent. load factor, the price given in the paper is 0·373d. per unit. I have had brought to my notice lately a 1,000-H.P. steam engine that has been running for some years with a 70 per cent. load factor, and the price is very similar to this. It comes out actually at 0·36d. per B.H.P., including everything and 10 per cent. for depreciation. This clearly shows that if a factory has a good steam engine with plenty of condensing water, with the work all round the engine and no long transmission of steam through pipes, no power company or corporation can supply cheaper than the owners can supply themselves, but their conditions must be perfect to do it. On page 295 he says it may be observed that 1s. per ton rise or fall in the price of coal per ton would have raised or lowered the price per unit by 0·039d., or 4 per cent. In getting out my prices for large consumers who are likely to earn 1d. less 40 per cent down to 0·6, I arranged that every 10 per cent. increase in the cost of coal should reduce their discount 5 per cent., which I see is practically the same figure as the one given. I have had a number of readings taken to get the sub-station diversity factor. Mr. Yerbury asked how it was obtained. We have taken the sum of the maxima in the sub-stations and divided by the maximum in the station, and that works out in Sheffield to 1·8. If we took the diversity factor on the installations connected in the city we should get one of 2·77. I think it very doubtful if any power company could bring a long transmission line into Sheffield and supply as cheaply as we are doing, because the power company has got to start with no load and gradually work it up and try and obtain a diversity factor. Most of their load would therefore come on in the peak time, and their diversity factor would be very small, at any rate for some years to come, and by that time they might have obsolete plant and become over-capitalised, as many municipalities are now. If one could put down 10,000 k.w. or 20,000 k.w. of plant and immediately load it up, then it would be a different thing. The average price we obtain for all our motors connected is 0·9, so that we still have a margin of profit.

Mr. Cridge. Mr. A. J. CRIDGE: I should like to ask Mr. Snell for some information on matters arising out of the paper. With regard to the term "diversity factor," I do not know whether I am right in supposing that Mr. Arthur Wright invented it, but he was the first to use it so far as I am aware. He defined it as the sum of the consumers' maximum demands divided by the maximum demand on the station. In one of the technical papers I noticed that some one defined it as a percentage—that is to say, the maximum demand on the station, divided by the sum of the consumers' maximum demands and multiplied by 100. The second method appears to be the reciprocal of the first, and I should like to have some definite guidance from the author on the point. It seems that the second result might be called the "system load factor," though it is not desirable to introduce any more names of factors. On page 289 of the paper Mr. Snell speaks of a diversity factor of nil. I presume he means unity. I should like to know if Mr. Snell has worked out any law for connecting the price of gas per 1,000 cubic feet with the cost of a unit turned out by a gas-driven plant. I think it would be a straight line law, and it seems that when gas is 1s. 6d. per 1,000 cubic feet the cost of a unit from the dynamo is about twice as much as the cost of a unit put into the gas-engine. That is, when gas is 1s. 6d. per 1,000 cubic feet the gas bill represents only one-half of the total cost of power. In the case of electric motors, the electricity bill represents something like nine-tenths of the total cost. The remainder, made up of oil, stores, waste, water, and other items, is obviously much smaller in the case of electric motors than in that of gas engines. On page 293 Mr. Snell refers to some pumping plant in connection with a dock, where the stipulation was made that only one-half of the pumps should be worked during the hours of peak load. Perhaps he will tell us if any automatic device was used to insure that the supply was restricted in this way. Reverting for a moment to the diversity factor, a difficulty has occurred to me. Unless one uses the maximum demand system I do not quite see how one is to get at the sum of the consumers' maximum demands at all. Would it not be some sort of a guide to us to divide the total number of kilowatts connected to the mains by the maximum demand on the station? That would give us some sort of a factor, which would be higher, of course, than the true diversity factor, but possibly higher always in the same proportion. Much has been said in the paper, and in the discussions here and elsewhere, about the price charged for power not being sufficiently high. It seems to me that, as Mr. Fedden says, if the price for power in Sheffield were as high as is recommended here we should never get any business. Surely it is better to offer power at a low price per unit, and to build up a big business, and thereby reduce costs, than to charge such a price as to produce a profit on every unit sold, and consequently to sell very little. That seems an elementary business principle. Table XIII. struck me as showing that it may sometimes be cheaper to cart coal over a railway than to transmit electricity over wires. That follows from the limits of economical transmission given therein.

Mr. Beauchamp.

Mr. J. W. Beauchamp: I notice there is some information in the paper concerning units consumed per capita of employees in electrically driven works.

I should like to ask the author if he has any information as to the cost of power in different kinds of works, taken as a percentage on the turnover. I know it is a difficult thing to obtain, as manufacturers would be reticent about it, but in two or three cases which have come under my observation the cost works out at about 5 per cent. on the turnover in jobbing engineering works of moderate size. In a stamping and piercing works as low as 1 per cent. In another place devoted to bicycle and small repetition work the figure was 2½ per cent. I presume it will be a much higher percentage in flour and spinning mills. Many examples, however, would be needed to allow of any generalisation.

With regard to the price of power, I rather feel that, although such low rates are needed to enable us to oust existing steam and gas plants, when the advantages of electric power are more widely known by experience to manufacturers it may be possible to obtain higher average prices than those which rule to-day.

Mr. Dickinson.

Mr. H. Dickinson: I do not wish to dispute the figures regarding the cost of private plant, because I have always found it very difficult to get any reliable figures from private users, but I should be prepared to dispute that although these figures may be obtained under ideal conditions, yet I feel absolutely certain that they are not ordinarily obtained in practice by power users, but may be got in individual cases. In cotton and woollen mills, where the conditions for cheap production are very favourable, very good results are obtained, and the plant is, generally speaking, well looked after, but my experience is that the power plant is not well looked after, which must lead to inefficiency. I think it would have been an advantage if the author in such a valuable paper had emphasised the fact of the very great convenience to the manufacturers in taking the power from outside. The supply from outside saves them all the trouble and worry involved with their own plant, and enables the staff to devote its whole attention to their particular business, and I am certain there is much more to gain by the technical staff being able to devote the whole of its time to the production of the article of manufacture than there is in its busying itself with the running of the power plant. As Mr. Beauchamp stated, in course of time, when people have fully appreciated the many advantages, they will take a supply from outside, and be prepared to pay a higher price for it than they are paying to-day. I was very much pleased to hear what Mr. Hartnell said on this question. He is very closely in touch with the large manufacturers of the district, and he knows their feelings with regard to the advantages or disadvantages of the outside power supply. I cannot, however, agree with Mr. Hartnell when he says that only where new factories are concerned the power company has a chance. Possibly Mr. Hartnell meant that only in new factories, or factories that have got to be modernised, the power company has a chance. I cannot agree with the author that

Mr. Dickinson. £12 per kilowatt for distribution is sufficient. On page 304 he says that "it is £12 per kilowatt of the kilowatts supplied." I think that must be wrong. Presumably he means £12 per kilowatt installed, but even on this basis I think the figure is too low, because it must be always remembered that the capacity of the mains laid must of necessity be very much in excess of the actual load, because it is necessary in laying cables to provide for some years ahead, and it will be found that in very few instances is the capacity of the cables laid less than twice the maximum load on the feeders, and in some cases it will be higher than this. The author emphasised the necessity for reducing capital. Some of the old undertakings, unfortunately having had to buy generating plant costing £15 per kilowatt when it can now be bought at less than £5 per kilowatt, must be seriously handicapped, especially as much of that plant will have become antiquated, and will soon have to be scrapped. The desire of those responsible for the old undertakings should be to bring down their capital to a modern basis in order to compete with those undertakings put down in recent years, having the advantage of having bought their plant at present day prices. There is one point which the author has not mentioned which should be borne in mind when calculating the cost of power on a mixed load of lighting and power. There is no doubt at all, owing to the steady demand of power load, it is cheaper to produce than a lighting load, and if such a thing could be imagined as a lighting load and power load with the same load factor and the same diversity factor, it would be found that the power load could be generated cheaper than the lighting load, for the simple reason that for the power load the plant is run more regularly, and is not subject to anything like the fluctuations the lighting load is subject to, and in the case of lighting it is necessary to keep a certain amount of spare labour and spare boilers under steam, ready to cater for dark clouds and fogs, the extra cost of which is very appreciable. With regard to the question of diversity factor, taking the sum of the consumers' maxima in relation to the load at the generating station in Leeds, this factor works out at 2·4.

Mr. Marsh. Mr. E. J. Marsh: I am glad to notice that the author mentions the question of charges on page 290, and remarks that "power has been supplied at speculative prices, and in some cases to their sorrow." The various methods of charging have been a very interesting source of discussion in the electrical papers for the last four or five years. Many of these charges appear to be based on the extra expense of coal, etc., involved in obtaining a higher load factor from the plant originally put down for lighting, but if this plant has to be increased to supply the demand for power, then the charges would require revision, for the working of the new plant not only increases the running cost, but also the capital cost, of interest and sinking fund, etc., which form so large a part in the cost of electrical supply. In some towns it is possible for large consumers to obtain a rebate of 85 per cent. for motor loads from the usual amount paid for lighting, while in the same towns, with equally large customers, the gas companies only allow a rebate of 25 per cent.

In my opinion the cheapness of the motor supply has been worked upon the basis indicated, and it has been found impossible to cheapen the charges for lighting, which form the source of profit, owing to the increasing motor load requiring new plant, and the power user has been supplied with cheap power at the expense of the lighting consumer. It is difficult to imagine that so large a rebate can be based on the right method. Some of the large electricity works are supplying the tramways at 1·2d. for 10,000,000 to 25,000,000 units, while manufacturers with a lower load factor and less units are charged ½d., which seems scarcely fair to the tramway user. Mr. Marsh.

Mr. W. T. WARDALE: I think there is one thing which large works will have to consider in respect of supplying their own current regarding the present attitude of the Board of Trade in the event of disasters in power stations. For over twelve months, whenever a disaster has happened in a power plant, the works manager (who is responsible for the plant) has been heavily fined. Managers, as a rule, will not pay a reasonable price for an engineer to look after their plant, consequently they are apt to get incompetent men. As soon as they realise that they will have to pay a reasonable price or run the risk of very heavy fines as a result of these disasters, which are often due to negligence, I think they will gradually come on to the power supply mains. Mr. Wardale.

Mr. F. WARDROBE: I think as a general rule the private plant of about 300 or 400 k.w. and upwards can generate as cheaply as an outside authority can supply, except in special cases, and the whole subject of private plant *versus* public supply comes down to the question of capital cost. If a private company can lay out the £10,000 or £15,000 required for a fair-sized private plant, so as to earn a better percentage than the generating plant can earn (in saving on power charges), well, I think as a rule they will do so. After reading the various costs, etc., in the paper, it struck me that if a supply company want to sell power cheaply, they must do so in the form in which it is generated, namely, alternating current. Now, I should say the larger proportion of works in the country are driven by direct-current motors (particularly the older and larger ones), and if we have to put in rotary converters, with their average 20 to 25 per cent. loss and cost of attendance, etc., why, I am perfectly convinced that no supply company can compete with a well-equipped private plant. Mr. Wardrobe.

As regards spare plant in private stations, I think most manufacturers are willing to take a fair amount of risk in this matter, and in any case one generally has the week-ends for any large repair. Personally I had a plant running for about five years, and in that time there were only about three stoppages, all of fairly short duration.

Mr. DICKINSON: The works manager should not be called upon to be responsible for the additional boilers, etc., necessary for the private plant, but in large works having to use steam for a variety of purposes, and having perhaps from twenty to forty boilers and their own boiler staff, I do not think the addition of a few more boilers for the power station need enter into consideration. Mr. Dickinson.

Mr. Snell.

Mr. J. F. C. SNELL (*in reply*): After such a prolonged discussion it would be quite impossible for me to reply to all the points which have been raised by the various speakers.

I want first of all to make a correction. The Newcastle Power Company has asked me to point out that Table XIII., which states that the figures are for 1907, refers really to the year ending December 31, 1905. I should also like to make one other correction. Some one seemed to think that the reading of my paper was something in the nature of an advertisement. It was not in the least intended to be so. I began to write my paper last year, and when I say I have absolutely no connection with any of the present London Power Bills before Parliament, I hope it will be seen that that part of the paper which refers to London is entirely the free criticism of a free lance.

With regard to Tables I. and II., I should also be very sorry if this paper gave the impression that the figures there given were such as all manufacturers could obtain from their local plants. I have been most careful to show that no supervision, no proportion of assessment, nor of the general establishment charges is included. The tables also do not include any spare plant. Sections I. and II. of the paper have been somewhat misunderstood; no doubt it is entirely my own fault—I think it is. I wanted to show that these figures can be obtained from small plants, provided certain costs are omitted. In Section II. I go on to show from the results of various works—in many cases figures taken by myself or by my assistants—that the figures are higher than in Tables I. and II., because in Section II. *all* the costs are included, that is to say, the actual capital charges of spare plant and some proportion of the general establishment charges, etc. The figure of 0·71d., to which one of the speakers referred in particular, is taken from a very well-known engine works, and I know is correct; but the 0·71d. is higher than the corresponding figure in Table II., because it includes, very properly, those proportionate charges to which I have referred. I should be sorry if the paper had the effect of frightening capitalists—they appear to be quite frightened enough already; but I think it advisable, when considering a question like this, covering so broad a subject, that we should face the truth, whatever the truth may be.

I am glad to find Mr. Rider in close agreement with me. He asks if the diversity factor of 1·25 referred to in my paper (p. 306) refers to sub-stations supplying lighting and power. This 1·25 diversity factor is the ratio of the sum of lighting consumers' demands to the lighting maximum demand at the station. He agrees emphatically with me as to the necessity for centralisation in dealing with the problem of power supply in London, as might be expected, seeing we have worked together on this problem for so long a time.

Mr. Taylor is totally wrong in stating that I ignore consumers of less than 100 H.P. Table I., for instance, deals with installations "up to 100 H.P." In Table VI. prices are given only for sub-stations of 100 k.w. and upwards. But surely Mr. Taylor would not advise sepa-

rate sub-stations of less than 100 k.w. each? If he does, then I disagree with him entirely. Mr. Snell.

In the London County Council figures referred to by him a proportion of the power consumers were to be dealt with through static sub-stations provided for in the estimate, some by direct current through rotary sub-stations (hence the differentiation between columns I. *a*, *b*, and *c* in Table XIV.), and some at low pressure through distributing mains, also included in the estimated capital expenditure. Mr. Taylor's presumption that in the London County Council figures only consumers who could "afford to pay for the costs of bringing high tension to their premises" were to be supplied, is totally incorrect.

I cannot agree that accumulator sub-stations can, as yet, appreciably modify the economical distances to which a supply can be given. My remarks in the paper are in nowise inconsistent with my former paper, and I am quite as much a believer in their usefulness as I was then. I do not follow Mr. Taylor's criticism of Tables VIII. to XII.

In Tables VIII. and X. and the first column of Table XII. the load factors are station load factors. In the other tables and columns they are consumers' average load factors: allowing for an average diversity factor of 1·66, as assumed, the adjustment between station and consumers' average load factors is obviously made.

In the example given by Mr. Taylor of the Poplar figures he himself is guilty of a "fundamental heresy." He defines diversity factor as the ratio between *kilowatts connected* and kilowatt demand at station. This is a very serious mistake; and in my opinion this is a ratio of no value whatever. I have again and again defined diversity factor as the ratio between the sum of consumers' demands and the station demand —a very different thing. Thus, Mr. Taylor's figures for Poplar are entirely wrong. For his correction I venture to give the method of arriving at my correct charge, with which I am sure he—as a thoroughly competent student of such figures—cannot fail to agree.

POPLAR.

Year ended March 31, 1906.

Units sold, 3,041,100. Maximum demand, 1,520 k.w.

Total capital expended, £250,873. 3½ per cent. interest on same, £5·777 per kilowatt demanded.

Standing charges :—	£	Works costs :—	£
Wages	1,827	Coal	5,081
Management, etc. ...	4,442	Oil	382
Depreciation, 2½ per cent....	6,899	Maintenance ...	855
	£13,168		£6,318
or £8·663 per k.w.d.		or 0·4994d. per unit sold.	

Mr. Snell. From which the total standing charges at 20 per cent. load factor and 1·66 diversity factor will be found to be 1·1866d. + works costs 0·4994d., giving a total of 1·686d. as the proper *average* charge for power under the above conditions.

Mr. Seabrook says there is so small a difference between the figures in Table II. and Table XIV. (I. *b*) as to negative the hope of power supply from centralised stations; but he overlooks my explanation (Tables I. and II.) of the fact that these do not contain any allowance for supervision, rating, or proportion of general establishment charges; and, I should have also added, spare plant. I think he may console himself by referring to Section II. of the paper, where recorded costs from certain well-known installations are given. Fortunately for us these figures are higher than those in Tables I. and II., because they include, very properly, the additional charges absent from the two first tables.

He questions my figure of £12 per kilowatt for a large station completely equipped. May I refer him to Mr. Sparks's discussion, where he gives a confirmatory figure for an actual station recently erected and very much smaller than that proposed by the London County Council. Mr. Seabrook takes as an example for his argument my figure for Newcastle, but he forgets that here there are included the cost of the old Pandon Dene Station, now abolished, and also the cost of the pioneer power station of this company—two items which have gone to swell the cost per kilowatt.

He criticises my warning as to the possible temporary falling off in lighting units, and goes on to say that this will not matter so long as his charges are estimated correctly. I agree. He then says, "It is quite obvious that if the power units are incorrectly calculated it will be disastrous." Exactly—that is the whole theme of my paper.

Mr. Seabrook cannot reconcile the differences in Table XIV. between "untransformed" and "transformed" prices; he has allowed for the loss in transformation, but has not allowed for the capital expenditure on transformers, switchgear, etc., which he will find will together account for the difference.

I admit I have not made Section I. of the paper sufficiently explicit; more prominence should have been given to the fact that Tables I. and II. are really incomplete, as is pointed out, but not clearly enough. I have already stated that spare plant is not included, but it must be remembered many factory owners will not admit any increase to their general establishment charges through the addition of their local plant, and are prepared to run the risks attendant upon no spare plant. For such people I am afraid Tables I. and II. are correct, and my experience has shown me that they are impossible to win over to a public supply unless questions of serious breakdown of local plant, space, expenditure on new plant, or nuisance make it worth their while.

I am indebted to Mr. Jeckell for his practical contribution. I must admit at once that I have not given sufficient prominence to the

differentiation of prices between large and small power users, or to the effect of added loads upon an existing station. I do say, however, in the paper that power "charges must be based on an economically designed station . . . together with a sound foresight into the effects of such an added load." Mr. Snell.

Certainly in preparing Table VII. I have taken all existing capital into consideration; and I do not agree with Mr. Jeckell's method of calculation at all.

Suppose he leaves the old undertaking to take care of itself, and to supply lighting consumers only, and bases the power supply on the newer and cheaper station only—in fact keeps the two undertakings separate and distinct in this respect. Very well, the power station pays all right and for the time the lighting station pays its way on a much higher revenue per unit. Then metallic filaments are universally installed and the lighting units fall to one-third or thereabouts of the former output. It will take a considerable time to treble the lamp connections in a provincial town where the electricity undertaking is already many years old. What happens? At once there is a heavy deficit in the lighting side of the undertaking—and, so far as the rate-payers or outside world are concerned, a deficit on the whole undertaking. I cannot agree, therefore, that his method is a sound one; for myself I should prefer to be more cautious. Mr. Jeckell gives his total cost of production as 1·63d., which confirms my figure for Coventry (Table X.), namely, 1·57d., because in my figure 3½ per cent. average interest is taken (so as to compare the various towns on the same basis), and I observe the actual average interest paid at Coventry is more than 3½ per cent. I commend Mr. Jeckell's excellent criticism of the relation of running costs and capital charges as well worthy of study.

There is no mistake in my costs of static sub-stations; they are not for toy transformers in street kiosks, it is true, and, therefore, include heavy switchgear, spare transformers, and adequate buildings, all of which must be taken into an estimate for industrial sub-stations.

Mr. Patchell reminded us of the advice of one of the London companies, namely, to prevent extension of local stations in London, and to co-operate in the provision of one or two large stations, properly situated. So long as this is done at once by some competent Board, which can raise money at a moderate rate of interest and without "watering," I agree that there is no other solution to this London problem.

I do not follow Mr. Patchell when he criticises Tables I. and II. He appears to think "labour" is not included in them. As it is certainly included I do not understand his remark. He criticises my figures for a paper mill—however, I can assure him the figures given in the paper are absolutely reliable.

I agree with Mr. Cottam that it would be iniquitous to permit any authority to obtain powers for a bulk supply in London, if the capital commitments of existing undertakings were to be ignored. The sense of justice in British tribunals would never permit it; but to have

Mr. Snell. facilities for concentrating all future generating plant in one or two suitable places at considerably less cost, and to supplement existing undertakings therefrom, is quite another matter, perfectly fair, and moreover in the best interests of existing undertakers as well as of the public at large.

Mr. Cottam asked whether, if ten or twelve year old stations are now obsolete, promoters would view with equanimity the cost of a larger station which may be also obsolete ten years hence. He forgets that the flagrant difficulty of existing London stations in such cases is their unsuitable position, besides their small units, lower pressures, atmospheric exhausts, and so on. Why perpetuate antiquated methods? Surely it is a simple matter of good engineering (which in this case means good commercial engineering) that an improvement so patent should be adopted.

Mr. Tapper criticises Table VII.—let me say at once that those figures are prepared on the maximum demand basis—the only true basis for electrical charges. He says the diversity factor at Stepney is from 3·5 to 4. I am afraid he has fallen into the error of a previous speaker, and is taking the ratio of consumers' total kilowatts installed to station maximum, which I have shown is quite wrong, and from which no useful deduction can be made. He asks that I should explain the figures in Table VII. I have pleasure in giving below the analysis for Stepney, as requested.

STEPNEY.

Year ended March 31, 1906.

Units sold, 4,046,205. Maximum demand, 2,012 k.w.

Total capital expended, £243,982. 3½ per cent. interest on same, £4·244 per kilowatt demanded.

Standing charges :—	£	Works costs :—	£
Wages	2,014	Coal (and steam) ...	8,788
Management, etc. ...	5,654	Oil, etc.	485
Depreciation, 2¾ per cent.	6,710	Maintenance ...	1,069
	£14,378		£10,342
or £7·146 per k.w.d.		or 0·613d. per unit sold.	

From which the total standing charges at 20 per cent. load factor and 1·66 diversity factor will be found to be 0·936d. + works costs 0·613d., giving a total of 1·549d. as the proper average charge for power under the above conditions.

I am glad to find Alderman Pearson in general agreement with the paper.

I agree with Mr. Shaw that a good deal of the power in London is made up from small installations which would entail a system of

distributing mains; but he might be surprised to learn how many factories there are in the area of Greater London each requiring power from 100 H.P. upwards. They form quite the larger proportion of the total. Mr. Snell.

Mr. Shaw argues that my "correct" charges in Table VII. are really incorrect because power loads would not be obtained at such figures. Lower prices are generally charged, I agree, but it does not prove that my figures are wrong or disprove my assertion. All three columns include costs of transmission and distribution, and all are based on a 20 per cent. load factor. I have modified the last column, however, as will be seen in my reply to Mr. Bowden.

Mr. Shaw advocates a system of linking up as the solution of the London problem. For three years now I have been closely associated in the most minute investigation of this problem. Linking up cannot, and never will, meet the case. Apart from the question of many systems, many pressures, and many frequencies—which cannot be commercially aggregated as he suggests—there is the fact that even with new plant the situations of existing stations (with very few exceptions) are such as to prohibit the cheapest production.

Mr. Bowden thought that Table VII., which figured in the London County Council evidence in the 1907 Bill, and for which I am responsible, was simply a "means to an end." I agree that it was, but certainly not in the sense Mr. Bowden suggests, as he goes on to say he thought "it was not to be taken seriously." This suggestion I repudiate emphatically. Table VII. gives figures which I maintain show the real costs with one assumption only, namely, that the diversity factor is 1·66. Since this figure too is based on my own experience, gained over many years in supplying power for industrial purposes, I believe it will not be found far from the truth.*

Mr. Bowden is right, however, when he corrects me as to the last column of Table VII. I regret that I inadvertently put down 0·83d. for direct current as the correct charge to consumers from the bulk supply *viâ* the authorised distributor. This figure, which has now been corrected in the table, is the calculated charge to the authorised distributor, who has in turn to redistribute it locally. Our estimated resultant total charge to the direct-current consumer on the 20 per cent. load factor I have taken was 0·94d., and for transformed alternating current 0·765d. I hope Mr. Bowden will be able to make as free use of this correction as may have been made of my acknowledged mistake. I have no desire to embarrass my station friends, and certainly less desire to do so by mistakes in figures. I do not think I can say more than this, except that there is no excuse for me, seeing that I was so closely responsible, with my late colleagues, for the preparation of these figures. If Mr. Bowden will refer to the London County Council tables of last year, which must have come into his possession in the Committee-room, he will find the answer to the last question in his contribution.

* See data for Poplar, p. 393.

Mr. Snell. I do agree entirely with a sliding scale of charges to all large power users, based on a sliding scale of costs of coal delivered per ton. It is a caution which was always adopted in late years when I was responsible at Sunderland.

Mr. Hordern is quite wrong when he attributes the comment on diversity factor which I apply to the large and small provincial stations as applicable to Table VII. That is an ingenious misreading of the paper. He and I differ so fundamentally in our ideas of the correct solution of this question that a further amplification of this already ample paper (as it seems to me) would be a redundancy.

I go part of Mr. Heaviside's way when he suggests groupings of existing London undertakings, but in my opinion he does not go far enough, and since reiteration is tedious, I can only refer him to the views expressed in the paper itself. His views of the ring main and the various groups assisting each other would involve such commercial and engineering obligations as to be utterly unworkable.

It is thought by Mr. Sparks that careful examination of Tables I. and II. will show they are inaccurate. He goes on, however, to say "they are the kind of costs one gets with plant in perfect order." I am glad he at least makes that admission, and I make bold to say they are the kind of costs users can always get if they keep their plant in the order in which it should be kept. But to these figures the costs of rating, supervision, and some allowance of general establishment costs must be made. Moreover, I agree that in actual practice users do not keep their plant in anything approaching "perfect order," and thus one finds the real total costs to be as given in Section II. of the paper. The remedy is with the user, but as power represents only 4 to 5 per cent. of his total annual expenditure, and as the difference between, say, 0·6d. and 0·7d. in the power costs only about 0·75 per cent. of his total, he does not bother about it. Mr. Sparks questions the units per H.P. in Table III. I do not think he will find 250 units per H.P.—that is, 340 units per kilowatt *connected*—is wrong; on the contrary, from my own statistics gathered from several parts of the United Kingdom, it is a general figure.

He misunderstands my reference to loss of lighting units. He accepts, of course, the fact that metallic filaments mean loss of units at first—a very considerable loss, I would add—and some time must elapse before the output can be trebled so as to compensate for this. It will come ultimately, I know, and will be to the general profit of electrical suppliers, but in the meantime (and this is what I refer to in the paper) loss of revenue must be faced.

Mr. Hammond is good enough to say that my humble effort will be remembered as having brought prominently before the Institution the very great effect of diversity factor. He and I have had many conversations on this point, and I know how thoroughly he recognises the importance of it. I am obliged to him for bringing forward so forcibly the necessity for some differentiation in the prices charged to consumers who do not affect the peak and those who augment it. I agree

entirely that consumers who aid the diversity factor markedly—or at all—should be, and can be, charged at less rates than those who lessen the value. Mr. Snell.

He thinks I have been too fair to independent plants in Tables I. and II. I believe—with him—firmly in centralisation. Do not I emphatically advocate it later? But there are still certain users with high load factors and small diversity factors and of such magnitude that, *quâ* cost, no central power station can compete.

He questions my Leeds figures (Tables VIII. and IX.). Nevertheless, the total cost of production in 1906 at Leeds was 1·37d. at 25 per cent. load factor. It is indisputable. This figure will be reduced from year to year, no doubt, and the influence of the "watered" capital of that undertaking (I do not use the term offensively, but refer to the purchase price of the original company paid by the City Corporation) will become less and less. But the average price to power users could be 0·99d. (Table IX.) on a 1·66 diversity factor. It will be remembered that Mr. Hammond says the price for power in Leeds is never less than 1d. I was glad to learn from Mr. Dickinson lately that the actual diversity factor was 2·4 per cent., so that my average price is at once reduced to 0·8d. Here we have at once an illustration of the effect of varied industries on the value of diversity factor and of the influence of diversity factor on cost.

Mr. Cowan likens the supply of electricity for power to a by-product. This is a great fallacy (excepting a very few special cases). It is a view against which my paper is intentionally directed—for it is an error which will cost some undertakers very dear. To be fair to him, he says, "This by-product is limited, and its sale at by-product prices must be correspondingly limited." I refuse to look at the sale for power purposes as a by-product. In some districts it may become the dominant part of the total output. The President later on speaks in the same way—a "remnant sale"! Even with so experienced a pioneer as he is, I have the temerity to differ with him. If he is looking at the problem as one restricted to that of the company of which he is chairman (Kensington and Knightsbridge), which supplies a residential area essentially, then he may be justified in looking on a supply to occasional power users in his area as a "remnant," or as Mr. Cowan calls it, "a by-product." But if these gentlemen apply this view to industrial centres such as Leeds and Sheffield, Manchester and Nottingham, they will, in my opinion, be committing a grave error.

I think my remarks on the contributions of previous speakers will be found to answer Mr. Addenbrooke.

Mr. Wordingham thinks too much is being made of diversity factor and that the important thing is the load factor. But the station load factor is dependent on the diversity factor, and it may be expressed as the consumers' load factors multiplied by their diversity factor. Low costs of production at the power station are directly related to station load factor, but how is an equitable charge to be made to this or to that class of consumer unless one knows two things, namely, his load

Mr. Snell. factor and his approximate diversity factor? Only by knowing these can one apportion the station costs of production.

I cordially agree with him when he says, "I think essentially a power company depends on the kind of town." He then goes on to explain, in words I need not repeat, how it is a diversity factor makes itself felt in large districts—he does not call it by this name—but, nevertheless, he describes it excellently.

Mr. W. H. Booth asks for more information as to the percentage of auxiliaries to main engines in textile mills, and thinks I put too high a value on this figure. I can only say that the figures given in the paper for paper and for jute mills were given me by the principals, and were, moreover, checked approximately by myself. I have lately been able to add the figures for another textile mill, which confirm the former example.

Mr. G. W. Partridge thinks my estimate of 1·66 diversity factor altogether too high for London. On the other hand, in the provinces and in Scotland I have been assailed for daring to assume so low a value. The 1·25 value in Sunderland is due essentially to there being practically but one class of power user, but in London the industries are very varied—even in Leeds the value is 2·4, and in Glasgow it is still higher. And in this latter city the value recorded is an actual one, since the maximum demand of every consumer is known from quarter to quarter. I differ from Mr. Partridge, and believe in the future it will be found that 1·66 was too low.

He questions the estimate for transmission in the London power scheme, and quotes £23 as the cost on Tyneside. But really we were both taught, I am sure, to compare comparable things! In Newcastle the voltage of the greater part of the system which cost £23 is 5,500 volts, the London scheme was to be at 15,000 volts. Newcastle includes an antique network belonging to the old company, of which the £12 estimate had no equivalent. Newcastle was a pioneer, and no doubt the same system laid *de novo* to-day would cost a good deal less. Finally, the magnitude of the London scheme and the grouping of duct lines, etc., and consequent reduced cost per kilowatt supplied, effects a great saving. The £12 figure can stand unassailed.

Mr. G. H. Corringham is in error in thinking the item "wages" to be excluded from Table I. Wages are included.

The paper mill figures quoted are reliable.

He gives us the interesting information that the diversity factor at Bermondsey is 2·7. What does Mr. Partridge say to this; does he still think my 1·66 too high for London?

Mr. Leonard Andrews' diagram and reference to Mr. Stott's recommendation are very interesting. I quite agree that when the load curve on any station is known to be adorned with a high peak, low running costs can be sacrificed to low capital for that section which is required only for so few hours during the year. I have adopted this plan myself with respect to condensing plant—having a due regard, of course, to the extra steam raising plant required where there is an atmospheric exhaust. But in a large station dealing with a power

supply—as considered in the paper under discussion—a much fuller load curve will be obtained with a much higher station load factor than is contemplated in either Mr. Stott's or Mr. Andrews' calculations. Thus the necessity for any differentiation between peak and non-peak plants disappears because there is practically no peak—or, at worst, it is but a small proportion of the whole. Mr. Andrews' translation of my running costs for a large station, which he—by a process of elimination—assumes to be 0·4d., is much too high. My figure for a large power station is nearer 0·2d., and I am satisfied this latter figure is quite within the range of practice. I could point to two stations at least with load factors of 42 per cent. and 54 per cent. respectively, where figures of 0·237d. and 0·198d. are obtained. Thus Mr. Andrews' diagram would require considerable modification. Mr. Snell.

The President has overlooked an important adjective in the opening remarks of the paper. I say "the fundamental elements and details of design are now so generally accepted that there appears to be but little to add to the *available* information." I do not mean by this that we have approached "finality of design." I do think, however, that—having regard to the now *available* knowledge—there is at present nothing more to add to what has been published and discussed as to power-house design. He does me an injustice (unintentional, I am sure) when he says that the water-tube-boiler-turbo-generator series is to be accepted as finality. The distinct evolution of, and progress made in, internal combustion engines alone would prove the foolishness of accepting any present practice as final. I venture to dissociate myself entirely from the President's pessimistic dictum that those who start now with gigantic schemes will, when they have got them into working order, find they are in no better position than those whom they are seeking to supplant at the present day. It is the words "in no better position" with which I disagree. Such a scheme—given proper design, a good output, and no waste capital—would be in a very much better position even then than the smaller plants of to-day. To say, however, that by then some still better systems may not become possible would be absurd and unworthy of any one believing in progressive evolution.

The President supports my figures as to the small cost of production from private plant, but he does not agree as to the superiority of "large turbo-driven plants" having regard to the "cost of distribution over great distances," etc. But surely one of the chief points of the paper is to emphasise the importance of not attempting (in this country) distribution over too great a distance so that the economy gained by concentration of plant is lost by the cost of transmission.

My previous reply seems to cover the points raised in the discussion at Dublin.

The Chairman (Mr. T. Tomlinson) has given some interesting figures of a proposed power scheme for Dublin and Central Ireland. The figures given in his table are presumably for high-tension energy untransformed, and even then they are, in my opinion, too low for practice.

Mr. Snell. Mr. Ruddle suggests I am condemning existing public installations. Surely this is a sweeping statement and a misunderstanding of my paper. I have pointed out how dangerous a *large* sale of small-priced units may be to most existing undertakings, having regard to their heavy capital charges; but I do not say that a power load does not improve their position. On the contrary, no one can deny that it does improve the load factor, costs of production, and, if in moderation, the financial results of a "lighting" station. But great care will have to be exercised.

Mr. Ruddle considers a 5,000-k.w. station can be worked just as economically as a 12,000-k.w. I do not agree with him.

Mr. R. Robertson is good enough to say that generally speaking he has very little exception to take to my conclusions. He thinks my figures in Tables I. and II. are too low even after allowance for some of the figures, which I have carefully pointed out in the paper, are omitted, and which are far from being *quantités négligeables.* He has plotted the results of Tables I. and II. and also the results quoted in Section II., in an instructive curve. Mr. Robertson goes on to say the figures at which companies can supply may be taken at £4 per kilowatt per annum, with a further charge per unit ranging from 0·5d. to 0·2d. I have tabulated these figures of Mr. Robertson's as follows, in order to compare them with Table XIV.:—

Annual Load Factor.	£4 per Kilowatt per Annum and	
	0·5d.	0·2d.
Per cent.	d.	d.
10	1·595	1·295
20	1·048	0·748
30	0·865	0·565
40	0·774	0·474
50	0·719	0·419
60	0·683	0·383
70	0·656	0·356
80	0·637	0·337

He does not say whether his prices are to be for extra high tension untransformed, transformed alternating, or converted to direct current. I agree that a differentiation must be made between certain classes of

power consumer—dependent on the nature of their loads and their extent. But he makes a mistake in not further differentiating between the form in which energy is to be metered to the consumer. I have dealt with the remainder of Mr. Robertson's remarks in my previous reply. Mr. Snell.

I am glad that Mr. Lackie's second thoughts proved better to him! The example he quotes of ordinary lift motors is surely an extreme. One knows that the single lift usually gives but a poor load factor and a small revenue. I agree that a large number will show a large diversity factor, and thus together give the central station a good return. One must not forget, however, the comparatively high capital outlay per lift for service lines and apparatus. Mr. Lackie concurs that my "economical axioms" are "self-evident facts." So much the better. He questions the annual load factor of small miscellaneous plants in Sunderland which I have quoted. My figures, I can assure him, are correctly given. It is most interesting to find that the power diversity factor in Glasgow is 2·4. This is a high figure, but I have no doubt it is correct, for in Glasgow the maximum demand of every consumer is known, and a differentiation between lighting and power classes can be observed with a fair degree of accuracy.

Mr. Lackie says I make no mention of the effect of power factor when advocating a wholesale use of static transformers; but, as he well knows, a low power factor can be corrected when necessary by the adoption of synchronous motor-generators installed at certain points of the transmission system. My advocacy of dealing with consumers of 100-k.w. demand or over with high-tension services and local transformers he thinks would add enormously to the initial outlay; but I have shown that this cost is not great. Certainly an addition must be made to the consumer's charge to cover this, but the consumer's cost will not be "doubled," as Mr. Lackie thinks, but, on the contrary, only increased by about 2 to 3 per cent. at most.

I agree with him as to the beneficial effect of a power load on a lighting station; it is, of course, well known. It is to be desired, and must be fostered if the most successful results are to be obtained; but what I say is, great care must be exercised in assessing the charges at which such a power supply may be given.

Mr. Lackie is quite wrong in thinking that if existing smaller stations are supplemented by an outside supply the load factor on the latter will be only "2 or 3 per cent." This is absurd, and if Mr. Lackie will only work out several cases as I have had to do in several scores, he will find it pays the smaller station to give all it can to the outside supply and use its own plant only for four months or so each year on peak loads.

It is so easily proved that I will not lengthen my already alarmingly long reply, but will ask Mr. Lackie to work out some examples and agree with me from his results!

I am amused at Mr. F. A. Newington's generalisation of the paper, namely, that "most existing undertakings are going to the bad, our

Mr. Snell. plants will all have to be scrapped very soon, and general ruin stares us in the face." Surely this is an alarming translation of my views. I do not say, or even hint, at either of these three calamities. But Mr. Newington begins to recover some cheerfulness as early as in Table I., so after all his gloom is soon in the process of being dispersed. Sorry as I am to disappoint him further, I must point out that in Table I. his supposition is wrong, and "supervision" (which is excluded) does not include ordinary running charges and wages, which are certainly included in the figures of the table.

At Edinburgh there are two stations situated at the foci of the area! In such a case it is quite possible that high tension can be dispensed with. I assert, however, that the excellent figures obtained at Edinburgh could have been further improved had there been one station and one system (not two as now, for Mr. Newington omits any mention of his single-phase high-tension system), and then high-tension transmission would have been necessary, but the resultant capital and revenue costs would have been less.

He is quite wrong, too, in saying that I advocate a further competitor in London. I do no such thing. I expressly say that existing capital spent by London companies and municipalities must be respected, and that the supplementary bulk station must be arranged with such an organisation that it and the existing undertakers co-operate for the common good; and this is feasible if only the local jealousies both of men and corporate bodies can be overcome.

I have nothing to reply to Mr. Raphael's sensible remarks, with which I am in general agreement.

Mr. Stothert's figures out-Herod Herod, and are even lower for individual users than those I have given! Since he quotes them from actual practice under his own control, we must accept them, but they are nevertheless abnormally low.

Mr. Parsons' figures as to the results from stations taking a bulk supply are interesting. I know only two of these from personal observation—namely, Acton and Willesden. Both are undeveloped, though, I believe, quickly growing districts, and most ably administered by their respective engineers. Had local steam plant been put down with heavier capital charges then the losses quoted would have been still greater. It may be that the power companies supplying these two districts have made very good bargains from their points of view; personally that is my opinion. A few years hence, however, a marked difference and improvement will be recorded by both.

Mr. J. A. Robertson's further remarks include an important item. He points out the very small proportion that the total cost of power bears in a factory to the total annual wages bill.

I know this is from 3 to 5 per cent. usually, and as a percentage is not of much account. Factory owners can be got to look at it in this way sometimes.

Professor Baily forgets that I do not now supply current, but often find myself advising both user and supplier. I agree it does not do to

spend all one's time in reducing costs by fractions of a penny without regard to the consumers' convenience. On the contrary, I should say that would be a grave offence against good business methods. I agree that, generally speaking, direct-current distribution is desirable—that does not prevent all power users (I say above 100 k.w. or thereabout in the paper) being supplied by 3-phase, with economy to both parties. But Professor Baily must not forget that once a low-tension network is introduced and conversion becomes necessary, a considerable increase in the cost of production results. Professor Baily—as I pointed out at Glasgow—misreads the paper when he attributes to me any desire to repudiate the existing debts on undertakings. Really I do not say anything so grossly foolish. Sixteen years of station supply work in all its details of finance, administration, and engineering in London and the provinces have equipped me, I hope, with a little more wisdom than Professor Baily suggests. And yet he says that it is obviously not my meaning—to respect existing interests. He also attributes other views to me which I am sure he will not find in the paper, for I do not hold them. Mr. Snell.

I should like to express my warm thanks to the Glasgow Committee and to the Members of the Scottish Sections for their kind reception of and hospitality extended to me.

I will endeavour in the short time at my disposal to run through the criticisms made by the various speakers at Sheffield. Mr. Yerbury asked for my definition of diversity factor. It is the consumers' maximum demands divided by the maximum demand at the station—that is, Mr. Wright's original definition. The same speaker also asked for the cost of buildings included in the £12 per kilowatt. The estimated cost was £2·87. The question of the Sheffield figures being omitted from Table VIII. has been raised. I can assure Mr. Yerbury that this was not done intentionally, and I simply selected a few towns at random. It was no intentional slight to Sheffield. He seems to be of the opinion that there is no hope that tramways or large steel works will ever take outside supply. Of course, so far as tramways are concerned, my point is that it depends entirely on the tramway itself. At Sheffield I do not think it would be possible, as they will probably be able to produce energy as cheaply as if it were combined with the electricity undertaking, but here the question of policy comes in. There are occasions probably, and there are exceptions to these occasions, when it is to the benefit of the general public for the tramway supply, for lighting and for general power to be combined under one station. I think it must be obvious that, as far as capital costs are concerned, the one station must result in economy. There is, on the other side, the point that a tramway station is able to produce energy more cheaply than the so-called lighting station, but we must look ahead, and in a few years' time there may be a material change; though we now have stations giving an average load factor of perhaps 12 to 15 per cent. (because the bulk of the load is for lighting), in future this load is going to be increased because of the general application of electricity to all

Mr. Snell. purposes, and I feel that as time goes on we shall yet find that the present position of the tramway stations in this country will be somewhat reversed, and instead of having a tramway station with the higher load factor, the general power station will result in better economy. It is not true to-day, but I think it will be so some years hence.

I was glad to hear Mr. Hartnell take a part in the discussion, as anything that he says is always worth listening to. I was very glad to hear his remarks on the cost of plant, particularly so as he agrees very largely with my figures. He went on to say that local factories (not now electrified) would take an outside supply when they adopted electricity, and expressed the opinion, I believe, that existing works supplied from their own electrical plant were not likely to take a supply from outside. In my paper I have tried to be fair throughout. If I found a disagreeable fact, which apparently was against the electrical industry, I have tried to face it squarely. But there are other points which we have to take into consideration when dealing with local plants, whether they are steam-driven or whether they are driven from an existing electrical system of their own. I have found in actual practice that space becomes extremely valuable to the manufacturer, and he is often glad to sell his plant and take the power from outside, and thus save the valuable space taken up by the plant, and there are occasional cases of local nuisance. Directly works begin to work overtime, then manufacturers like to take an outside supply, as the cost of production goes up very fast. Night lighting is also extremely useful in case of fire. Another point, and certainly not the least, is, when it becomes necessary to put in more economical plant, I think the manufacturer always realises that he is expending a large amount of capital which he would prefer to use in his own business. So there are many facts, I am happy to say, to prove the advantage of outside supply. Some people seem to think that the power people have a hard fight to get factories connected to their mains, yet there are many facts which make it worth their while.

With respect to the works which Mr. Hartnell mentioned, a full record of cost was actually published in the *Engineer*, and my recollection is that it was much nearer 0·7d. than 0·5d., but it could be easily looked up.

The tables I have given, and especially the costs in Section II., are actually taken from my own observations. Members may take it from me that the figures are not only taken by myself, but have been separately checked, and I think they are as correct as they possibly could be.

Mr. Fedden throws down the gauntlet at once and says he is not afraid of the power companies. I think he is quite capable of taking care of himself. He criticised one of the "axioms," namely, that beyond a critical load factor and size of plant no outside supply can hope to compete with a user's local plant. In spite of his criticism, I still hold to that. I am quite certain that there is no power company

in existence in this country that can afford to supply a paper mill at a cost of something like 0·33d. It cannot be done. One cannot afford to pay for the transmission system to the paper mill and at the same time make a profit. The sooner we recognise these facts the better for the industry. I was interested to hear Mr. Fedden's ascertained load factors for various large consumers. I was rather surprised at the figure of 16 per cent. in a steel works, but my experience, I agree, is limited so far as steel works are concerned. In two cases, however, within my experience the load factor was over 20 per cent. Although he has a number of these works which give load factors of from 7 to 18 per cent., owing to the diversity factor his power-house load factor is from 40 to 45 per cent., and that is excellent and seems to bear out the figures I have taken. He says all the charges should be based on the plant load factor. I presume he means the station load factor. I quite agree with that, and it is exactly what I have done in Tables IX. and XI. I have shown the actual cost of production with a diversity factor of 1·66. Mr. Fedden went on to say that it was unfair that the quarter of a million pounds paid to the old company in Sheffield should be charged to new power consumers. Of course, that is a matter of policy which I think it would be indiscreet of me to question in his own town. He referred to the chances they have in Sheffield, where the price of gas is only 1s. per 1,000 cub. ft. Of course, the cost of gas is not the only item. As a matter of fact, it represents about three-fifths of the total. I quite agree he must have an exceedingly uphill task where gas is so low. I have found it difficult enough at 1s. 10d., and I think it speaks extremely well for Mr. Fedden and his assistants that so much power is supplied at Sheffield. He gave some figures for suction gas engines, but they did not agree with my figures, although those given in Tables I. and II. are from actual installations. I am very glad to see that he confirms the figure for steam engines, namely, 0·36d. per B.H.P., as against my 0·373d. per unit. I have suggested that every power user ought to be supplied on a sliding scale. He puts the diversity factor of his sub-stations in Sheffield at 1·8. That is to say, he takes the sum of the maxima of the sub-stations divided by the maximum of the station. Then he gives this ratio based on the kilowatts installed, which is 2·77. My figure was 2·4, so there is not much diversity between us. With Mr. Fedden's diversity factor of 1·8 the actual cost per unit is 0·65d. It is an excellent figure, and I have no doubt he is doing it. I should like to know at what load factor. Mr. Cridge asked whether the cost of the consumption of gas engines practically followed a straight line. As far as I know, the total consumption of gas does follow a straight line law. Regarding the plant mentioned on page 293, there is an automatic device for cutting off the power. Mr. Snell.

I doubt whether the kilowatts installed is any guide in determining diversity factor. In the cases I investigated I have found that there is no definite ratio between the consumers' maximum demand and the actual kilowatts installed at the works, as they vary in every

Mr. Snell. case. I do not think, therefore, that Mr. Cridge's suggestion of taking the kilowatts installed would give any reliable data.

Mr. Beauchamp asked whether the cost of power could not be expressed in a percentage of the total factory costs. I cannot answer the question, as I find that manufacturers are very reticent in giving information. I got some figures from one manufacturer confidentially, and I cannot give them. In some of the cases in my paper the cost of power is from 4 to 5 per cent. of the total *wages* paid.

Mr. Dickinson is prepared to dispute the figures given in Tables I. and II. He does not doubt the accuracy of the figures, but he does not think they could be obtained. On page 294 I refer to a large works in which the plant is gas-driven. Members would be surprised if I gave the name of that particular firm, yet that plant was taking about 38 cub. ft. of gas per kilowatt-hour instead of 20, assuming 15 per B.H.P.-hour.

I have already emphasised the great convenience of an outside supply to factories. As one speaker remarked, the plant is more easily replaced by motors, but, of course, that applied to local plant as well as to electricity supplied from outside. Mr. Dickinson remarked that £12 per kilowatt for transmission was too low. I quite agree that the ordinary price is very rarely less than £30, but in taking the £12 the figure was worked out, checked and cross-checked, and tendered for. The total cost of high-tension transmission at 15,000 volts, including low-tension supply to a large number of consumers (roughly, about one-third), only came to £12 per kilowatt. I agree it was for a large system of about 120,000 k.w. Mr. Dickinson also drew attention to the fact that the plant in some of the old stations would have to be replaced. I quite agree that they should get rid of it, but how is the deficiency going to be met? I cannot see how it is going to be met, except by an unfair charge to the present generation. I quite agree that the present undertakings are suffering. The fact is, the original plant cost a great deal more than what it could be bought at to-day. This only leaves one way in which it can be met, and that is to push forward business as much as possible until the original capital is merged in the bigger concern. He says I do not mention any method for differentiating between lighting load and power load. I think he is wrong, as I do mention it several times; and I would point out that the diversity factor of the lighting load is generally 1·25, and for power, 1·66.

Mr. Dickinson says the diversity factor in Leeds is 2·4. I agree there are all kinds of methods of charging, and I can say that all the methods must be based upon the maximum demand system, as it is the only system, in my opinion, which is a correct one. Mr. Marsh asked whether I have charged for interest, sinking fund, etc. I have taken 3½ per cent. for the interest on capital and 2½ per cent. for depreciation. It does not matter whether one pays into a sinking fund or a depreciation fund, so long as it is paid. If one puts it into a sinking fund, which is a sum capable of covering the depreciation, one does not want to have a depreciation fund as well. But if the loan period is too long,

then the sinking fund must be supplemented by a depreciation fund. Some people think that one ought to pay into a sinking fund sufficient to repay the total capital, and a depreciation fûnd as well; but that is entirely wrong, as it puts a most unfair charge on the undertaking. Mr. Snell.

In reply to another speaker, it may interest members to know that in the case of the proposed supply from St. Neots to London it was proposed to build a station at a distance of 46 miles, and transmit energy into London at a pressure of 30,000 volts. When it was worked out, it was found to be absolutely cheaper to generate from a large station on the confines of London than it was to supply from a point nearer the coalfields. The cost of supplying power to London from a station at the coalfields would be utterly prohibitive. It may be cheaper to supply power from Niagara, where there is water power, but to supply from coalfields is another matter. As a matter of fact, it is cheaper to carry coal over railways than it is to transmit power this distance over transmission lines in this country.

Proceedings of the Four Hundred and Sixty-seventh Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Mechanical Engineers, Storey's Gate, St. James's Park, Westminster, S.W., on Thursday evening, January 16, 1908—Colonel R. E. B. CROMPTON, C.B., President, in the chair.

The minutes of the Ordinary General Meeting held on January 9, 1908, were taken as read, and confirmed.

Messrs. W. H. Molesworth and J. H. M. Wakefield were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

As Members.

William Baker Brown, Major R.E.
Harry Parker Gibbs.

As Associate Members.

Hugh Wight Arbuckle.
Richard Henry Bradbury.
Louis Burn.
Frederick Arthur Cole.
Edward Comerford.
Haldane Gwilt Cotsworth.
Robert Wm. Johnston Fletcher.
James Kerr Forrest.
Percy Vivian Gray.
Cecil John Grimes.
John Herbert Harpin.
Henry Wilson Hartnell.
William Lewis James.
John Lloyd.
Charles Hawthorne Lydall.
John Joseph McMahon.
Andrew McPherson.
Arthur William Martin.
Walter Lockhart Maxwell.
William Richard May.
William Fernie Mitchell.
Thomas Bradley Naylor.
George Kinnaird Paton.
Ernest George Phillips.
Robert Jones Roberts.
Ernest Rowarth.
Charles William Smith.
Professor William Sandilands Templeton, M.A., B.Sc.
Thomas Sydney Warren.
Bernhard Wiesengrund, Ph.D.
Poul A. von Wildenrath.
Arthur Roland Woodhall.

As Associates.

Julian Bruce-Kingsmill, Major R.A. (Ret.).
Howard Foulds.
Lightly Stapleton Simpson.
Wilfred Broughton Trafford.

As Students.

Frederick Alexander.
Albert John Anido.
Montagu Barrington Baker.
Archibald Nettleton Balme.
Peter Edward Bamford.
John Theodore Baring.
Lewis Barney.
Robert Henry Batson.
Harold Godfrey Baxter.
Stewart Baxter.
Claude William Boak.
Arthur Gerald Bower.
Robert Alexander S. Boyton.
Henry Michael Bruton.
Leonard Gilbert Bulmer.
John Hilary Pyne Burchett.
Francis Edgar Burnett.
Matthew Nimmo Caird.
Robert W. Canning.
Eric George D. Carr.
Reginald Avonal Cartland.
Algernon Gerald Basil Chaldecott.
T. W. Chalk.
Albert Collins.
John Conway.
Norman Richard Corke.
Edmund Crawshaw.
George Gault Cree.
Arthur Wesley Crompton.
Ernest Victor Edward Crosse.
S. P. Dass.
Jeronimo José de De Mesquita.
Alan Elliott Dent.
Thomas Archibald F. Dixon.
Alexander McLaren Doig.
George Oliver Earle.
Eric William Eller.
George Joseph Farrelly.
Arthur H. Finnis.
Frank Gordon Foote.
Roger Ford-Hutchinson.
John Francis Forrest.
Khurshed P. Framjee.
Gordon Franklin.
Frederick William Geoghegan.
John Alexander Gibson.
Leonard Burgess Gilbert.
Charles Malcolm Gillies.
William Thomas Golden.
John Melville Goodall.
Alexander George Gow.
Maurice Frank Gower.
Henry Percival Guy.
John Hacking.
Leonard Harding.
Ernest Arthur Hilton.
John Hamilton Noel Hingston.
Stanley Maskell Hitchcock.
William Richard C. Hockin.
John Hollingworth.
Bernard Whelpton Holman.
Philip Edgar Hosegood.
Ernest Frederick James.
Emanuel Josephs.
Leslie James Jowit.
Sydney Kay.
Robert Eric Keelan.
Cedric Kelley.
Archibald Frederick Kelly.
Thomas Maurice Klein.
Charles Thomas Kreiser.
Ralph Larkworthy.
William Reginald Lewis.
Reginald Frederick Long.
Duncan Whyte Low.
William Currer McCallum.
Edward Stanley McClintock.
Gervais Bushe Manson.
Henry Colvin Rowland Martin.
Leonard Cadoux Martin.
Leonard John Matthews.
Clement Melbourne.

Ernest Arthur Mills.
Robert Malcolm Murché.
Reginald Edgar Neale.
Vernon William Newman.
John Barnabas Gordon Northcott.
Vasudeo Shioram Padhye.
Reginald George Parrott.
Thomas Henry L. Paull.
Middleton Leaviss Peel.
Chas. Bryan Penrose-Fitzgerald.
Thomas C. Pettifor-Catchpool.
Martin Pitt.
Henry Edward Poole.
William Arnold Prescott.
Alan Priestley.
George Reginald Dudley Prince.
George Henry Noel Reay.
Arthur Owen Roberts.
William Langworthy A. Rogers.
Alberto Moreira Rosa.
Clement Saxton.
Gregory Oliver Scampton.
Arthur Rupert Sharpe.
Reginald Arthur Shiell.
Cuthbert William Short.
Edwin Simkiss.
Harold James Stenning.
George Eugene Stevenson.
Arthur Albert Stone.
Arthur Stubbs.
George Skelton Terry.
Henry Montgomery Thompson.
Cecil Parr Tufnell.
Enrique Julio Uhink.
Henry Edmund T. Vale.
Hugh Roger Viall.
Charles Croswaithe Villa.
Leslie Newton Vine.
John Michael Walsh.
Frederick Godfrey Watermeyer.
William Aston Watkins.
Herbert George Weaver.
Jas. Matthew McGregor Whellens.
Percy Francis Ramsey White.
Sydney Norman C. Whitehead.
William Henry Whitehouse.
Humphrey Williams.
Leonard Furniss Willing.
Godfrey Pountney Willoughby.
Arthur Cyril Yeates.
George Ernest Yonge.

Donations to the *Building Fund* were announced as having been received since the last meeting from R. A. Dawbarn and H. M. Stich; and to the *Benevolent Fund* from H. Alabaster, B. Davies, *Electricity*, Major-Gen. E. R. Festing, C.B., E. Garcke, F. A. Greene, The Halifax and Bermudas Cable Company, H. A. Irvine, Sir H. C. Mance, C.I.E., W. Mead, C. Richardson, R. Robertson, F. C. W. Rogers, H. M. Stich, and W. R. Wynne, to whom the thanks of the meeting were duly accorded.

The discussion on Mr. Snell's paper was concluded (see page 332), and the meeting adjourned at 9.30 p.m.

MANCHESTER LOCAL SECTION.

MAGNETIC OSCILLATIONS IN ALTERNATORS.

By G. W. WORRALL, M.Sc., M.Eng., Associate Member.

(*Paper received from the* MANCHESTER LOCAL SECTION, *December* 5, 1907; *and read at Manchester, January* 14, 1908.)

"Magnetic Oscillations in Alternators" * was the subject of a previous paper by the author. In that paper it was pointed out that such magnetic oscillations occurring in alternators as are due to the shape and proportions of the machine parts may be broadly divided into two classes:—

1. The oscillations taking place in the main magnetic circuit.
2. The oscillations taking place locally in the pole-face and air-gap.

The first were shown to be practically independent of the shape of the armature slots and the armature current, while the second were shown to be greatly influenced thereby, both in their magnitude and nature.

The present paper contains the results of further investigations into both these classes of oscillations.

OSCILLATIONS OF THE MAIN MAGNETIC FLUX.

Although the existence of this class of oscillation has been fully recognised, diverse views have been expressed regarding the conditions determining its nature and magnitude.

Guery † remarks, in a mathematical paper dealing with the production of harmonics in the E.M.F. wave of an alternator, that there is always a periodical change in the main magnetic flux, because the reluctance of the magnetic circuit is increased when a slot enters under a pole-face and it is diminished when a tooth enters.

Arnold and La Cour,‡ dealing with the same subject, consider that there are two cases, one when the polar arc is a multiple of the tooth-pitch, and the other when it is a multiple plus one-half. In the former case there are either two teeth or two slots under the tips of a pole-shoe, giving rise to a periodic variation in magnitude of the main flux and in the latter case when a tooth is under one tip a slot is under the other, giving rise to a to-and-fro movement of the flux.

K. Simons § distinguishes between the two cases above mentioned,

* *Journal, Institution of Electrical Engineers*, vol. 39, p. 206, 1907.
† *L'Eclairage Electrique*, vol. 36, p. 51, 1903.
‡ *Sammlung elektrotechnischer Vorträge*, vol. 3, p. 58, 1901.
§ *Elektrotechnische Zeitschrift*, vol. 27, p. 631, 1906.

but considers that when the polar arc is a multiple of the tooth-pitch the reluctance of the magnetic circuit is constant, and the main flux is subject to no variation in magnitude, but only to a to-and-fro movement, and that when the polar arc is a multiple plus one-half, the reluctance of the magnetic circuit undergoes a periodic variation, and the main flux to a corresponding variation in magnitude. Thus the deductions made by K. Simons are in direct opposition to those made by Arnold and La Cour.

There does not appear to be any record of experimental observations of this phenomenon except Fischer-Hinnen's * experiments on the humming of dynamos and motors. The experiments of this author show that the pitch of the note emitted corresponds to the frequency with which the teeth pass the pole-shoe, and he concludes that

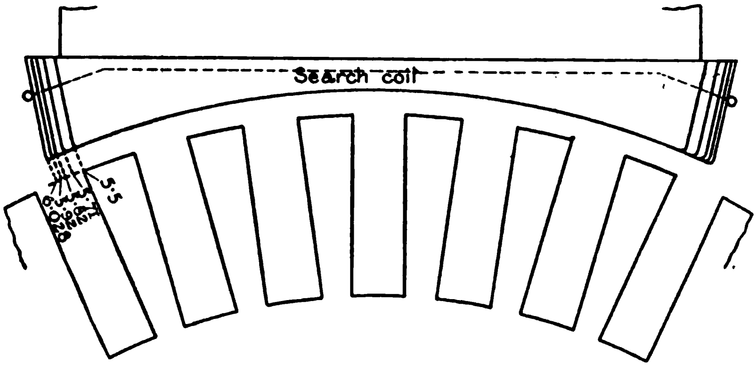


Fig. 1.

humming is due to the oscillations in the main magnetic circuit. As a result of his experimental observations he gives the formula—

$$\frac{b - 0{\cdot}7\, r}{(D + 2\,\delta)\,\pi}\,N = (\text{whole number}) + 0{\cdot}5\,;$$

where b = breadth of pole-face,
r = radius of pole-tip,
D = diameter of armature,
N = number of teeth in armature.

This author considers that theoretically the expression should be equal to a whole number, because in that case the reluctance of the magnetic circuit is constant, and he explains his experimental results by assuming that on account of fringing of the flux at the pole-tips the effective is greater than the actual pole width.

The machine employed in the present investigation was the same

* *Zeitschrift für Electrotechnik*, vol. 22, p. 339, 1904. *Electrician*, vol. 53, p. 300, 1904.

as that described in the author's previous paper under the same title and already referred to, *i.e.*, 3-phase, lap-wound tapped armature, 4 poles, 44 teeth, speed 1,200 r.p.m., P.D. on open circuit 173 volts. To this machine and for the purpose of the present investigation new

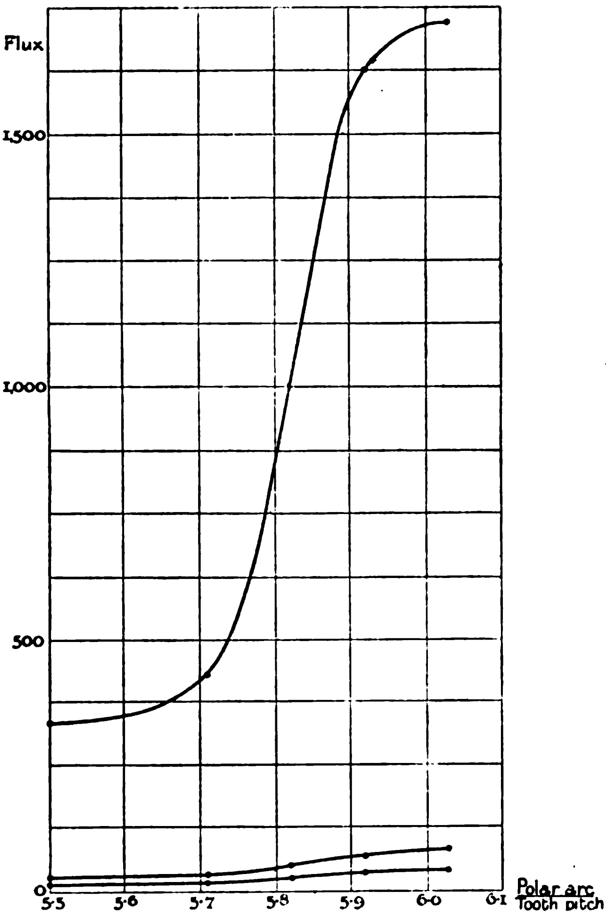


FIG. 2.

pole-pieces were fitted having an arc approximately equal to six times the tooth-pitch. The pole-shoe and limb were laminated and in one stamping. The armature slots were open, $\frac{9}{32}$ in. wide and $1\frac{5}{8}$ in. deep; the width of the tooth at the armature surface was the same as the width of slot; the air-gap was 0·118 in.

The magnetic oscillations were observed by means of the E.M.F.'s

induced in search coils linking the magnetic circuit. One search coil of 10 turns, wound on a wood frame, enclosed the pole-shoe; another of 60 turns was wound round the limb, midway between the pole-shoe and the yoke, and a third of 60 turns round the yoke. The position of the search coil on the pole-shoe is shown in Fig. 1.

As in the author's previous investigation, the E.M.F.'s were photographically recorded by means of a high-frequency Duddell oscillograph and revolving film camera. Since the armature current had been shown to have practically no influence on the magnitude of the oscillations, the machine was run only on open circuit. The polar arc was varied by planing down the tips of the pole-shoes. The planing was done in a direction parallel to the laminations, and with a very sharp tool and fine feed, so as to prevent as far as possible a burr forming over the edges of the laminations. The exciting current was adjusted so as to produce the same magnetic flux in the magnetic limb in the case of each experiment. The flux in the other parts of the circuit also remained nearly constant throughout. Fig. 1 shows the pole-shoe in its various sizes.

The area of the E.M.F. wave induced in a coil represents the periodic increase and decrease of the flux occurring at that point of the magnetic circuit. Table I. shows the actual values of this change of flux for the different sizes of pole-shoe. In Table II. these values are expressed as a percentage of the total flux threading a single turn of the coil. In Fig. 2 the results given in Table I. are graphically represented. It will be noted that the magnitudes of the oscillations occurring in the yoke are approximately one-half those occurring in the limb of the pole; this is, of course, due to the yoke flux being one-half the limb flux. The percentage oscillations are the same in both parts of the magnetic circuit. From these results it will be seen that the magnitude of the oscillations varies through a great range, and that it is a maximum when the polar arc is six times the tooth-pitch and a minimum when the polar arc is five and one half times the tooth-pitch.

TABLE I.

$\frac{\text{Polar Arc}}{\text{Tooth-pitch}}$	Pole-shoe.	Limb.	Yoke.
6·028	1,720	82	40
5·920	1,620	63	30
5·820	1,000	52	25
5·710	425	32	15
5·500	330	28	13

TABLE II.

$\frac{\text{Polar Arc}}{\text{Tooth-pitch}}$.	Pole-shoe.	Limb.	Yoke.
6·028	0·206	0·0094	0·0097
5·920	0·194	0·0072	0·0073
5·820	0·119	0·0060	0·0061
5·710	0·051	0·0036	0·0036
5·500	0·039	0·0032	0·0032

The P.D. wave was oscillographed for each size of pole-shoe, and Fig. 3 shows the half-waves recorded. It will be noted that the ripples

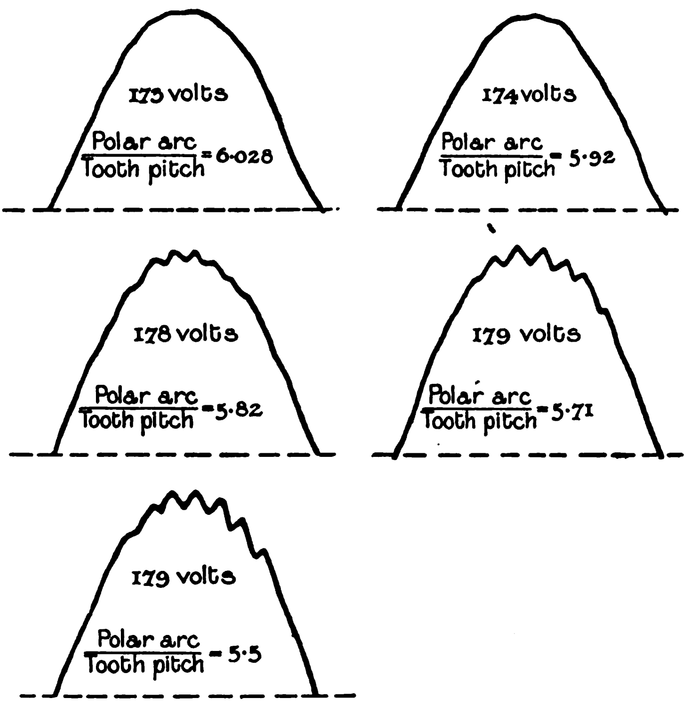


FIG. 3.

increase very considerably in magnitude as the polar arc is reduced, and are a minimum when the polar arc is such that the magnetic

oscillations recorded are a maximum and *vice versâ*. The P.D. was found to rise as the ripples increased, and the values corresponding to the various waves are indicated thereon.

The remarkable result above noted necessitates the further examination of the cause of the magnetic oscillations and their effect in generating E.M.F.'s in the winding of the armature.

The general distribution of the field in the case of the smallest size of pole-shoe was determined by means of the E.M.F. wave induced in a coil attached to the armature surface, and of a width equal to the pole-pitch. Each side of the coil was laid over the centre of a slot, and the E.M.F. wave obtained is shown in Fig. 4. It will be seen that the field immediately beyond the pole-tip is very small compared with that under the pole, and hence the stray flux increases the effective polar arc by but a very small amount. The variations to which the main field is subject may be examined by means of the diagrams in Figs. 5 and 6. In Fig. 5 the polar arc equals six times the tooth-pitch, and in Fig. 6 five and one-half times. In the case shown in Fig. 5 the two extreme positions of the armature relative to the pole are when (*a*) two slots, (*b*) two teeth, are under the pole-tips; the former position is shown at A and the latter position at B.

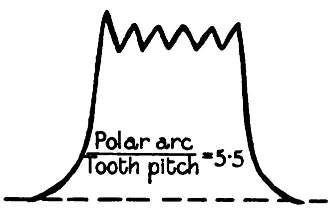


FIG. 4.

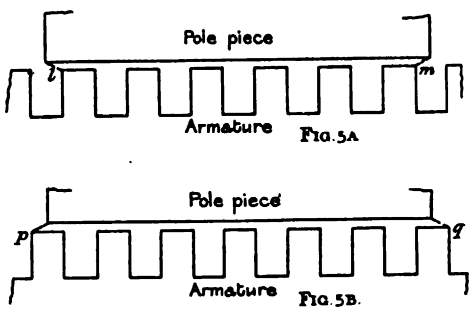


FIG. 5.

In position A, since there is very little fringing, the major portion of the flux will only enter the teeth actually under the pole, and hence the boundary lines of the flux will be approximately as shown. In position B the flux will spread out towards the outer edges of the teeth, and the boundary of the flux will be somewhat as shown. Thus the surface through which the flux enters the armature increases from *l m* to *p q* as the latter moves from position A into position B. Hence the arc of the armature under the influence of a pole is subject to a periodic variation

in magnitude, and the reluctance of the magnetic circuit, and hence the magnitude of the magnetic flux, is subject to a periodic variation also. This magnetic oscillation may therefore be termed "flux pulsation." In the case shown in Fig. 6, when the polar arc equals five and one-half times the tooth-pitch, the two extreme positions of the armature relative to the pole are when (*a*) a slot is under the trailing tip and a tooth under the leading tip, and (*b*) a tooth under the trailing tip and a slot under the leading tip; the former position is shown at A and the latter at B. The boundary lines of the flux in the two cases will be approximately as shown. From these latter diagrams it is clear that the surface through which the flux enters the armature remains constant in magnitude, but is subject to a periodic variation in position relative to the pole-piece, giving rise to a to-and-fro swinging of the main flux, which therefore may be termed the "flux swing." Notwithstanding that the principal variation to which the main flux is subject n the two cases is as described, yet it is evident that it is also subject

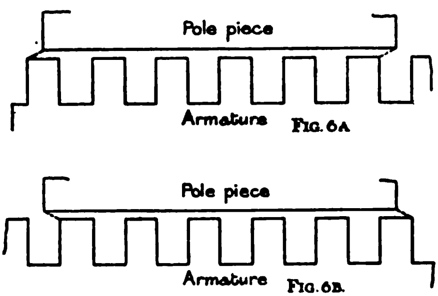


FIG. 6.

to a slight "swing" in the case shown in Fig. 5 and to a slight "pulsation" in the case shown in Fig. 6. When the proportion of polar arc to tooth-pitch lies between the values given above, the oscillations which occur will include both of the types described, but their magnitudes will not be so great.

The "flux pulsation" would affect the whole of the magnetic circuit, but the "flux swing" would affect the air-gap only.

Thus it is that the E.M.F.'s generated in the experimental search coils linking the magnetic circuit were a maximum for the case shown in Fig. 5, and a minimum for that shown in Fig. 6.

The two kinds of magnetic oscillation of the main flux have now been dealt with, and their effect in generating E.M.F. ripples in the armature coils may next be considered. The frequency of the ripples is the same as that of the magnetic oscillations, *i.e.*, equal to the number of teeth passing a pole per second; in the case of these experiments the frequency was 880. When the flux is subject to a "pulsation," the E.M.F. ripple generated in an armature coil will be in proportion to the flux linked by the coil. The flux linked is a minimum when the

centre of the coil coincides with the neutral line, and is a maximum when it coincides with the centre of the pole-face. Hence, the E.M.F. ripples will be a minimum in the former position, and a maximum in the latter. On the other hand, the main E.M.F. generated in the coil due to the rotation of the armature will be a maximum in the former position and a minimum in the latter. Hence, in the case shown in Fig. 5, the ripples in the E.M.F. wave will be a maximum as the wave crosses the zero line, and a minimum in the crest of the wave.

When the flux is subject to a "swing," the E.M.F. ripple will be generated in an armature coil only as the sides of the latter are actually cutting the flux, that is, when they are under the pole-face. Hence, in the case shown in Fig. 6, the ripples in the E.M.F. wave will be a maximum in the crest and a minimum as the wave crosses the zero line. This case is very clearly illustrated in the E.M.F. wave shown in Fig. 4, already referred to.

In considering the summing-up effect of the armature coils, distinction must be made between the two principal types of winding, *i.e.*, the open circuit and the tapped continuous current winding. In the former the E.M.F. ripples in successive coils will add up in the same way as the main E.M.F.'s, and therefore in this case the remarks made on a single coil apply to the whole armature. In the latter the main E.M.F. is a maximum when the tapping points are in the neutral plane, and a minimum when they are opposite the middle of the pole-face. In the former position of the tapping points the "pulsation" of the main flux will induce E.M.F. ripples in the conductors of any armature circuit which will cancel each other and so give no resultant P.D. between the tapping points, while the "swing" of the main flux in the air-gap will induce E.M.F. ripples which will add up and give a resultant P.D. between the tapping points. In the latter position of the tapping points, and when the main E.M.F. is zero, the "pulsation" of the main flux will induce E.M.F. ripples in the conductors of any armature circuit which will add up and give a resultant P.D. between the tapping points, while the "swing" of the flux will induce E.M.F. ripples, which will cancel each other and so give no resultant P.D. between the tapping points. Thus it appears that in all types of armature winding the ripples in the crest of the wave are due to the "flux swing," and those in the zero portion of the wave are due to the "flux pulsation."

This effect may be made more apparent by intensifying the ripples in the P.D. wave by means of resonance. Fig. 7 shows the resonance P.D. obtained in the case of the smallest polar arc, from which it will be seen that the zero portion of the wave is practically smooth. In order to ascertain if this effect occurred in the case of other machines, several generators were oscillographed. Fig. 8 shows two of the half-waves recorded. A is the P.D. wave of a single-phase generator with rotating field; the ratio of polar arc to tooth-pitch was six, the air-gap was slightly greater at the tips than at the centre of the pole-face. It will be seen that the crest of the wave is practically smooth, due to the

absence of "flux swing." B is the P.D. wave between neutral and line of a 3-phase generator with rotating field; ratio of polar arc to tooth-pitch was very slightly greater than five and one-half, the air-gap was constant over the whole pole-face, but the tips of the pole-shoes were very thin. It will be noted that the ripples are very strong in the crest of the wave, due to the "flux swing" effect, but diminish towards the zero portion. The ripples cover a somewhat larger arc of the wave than in the case of the author's experimental machine, on account of the special nature of the winding.

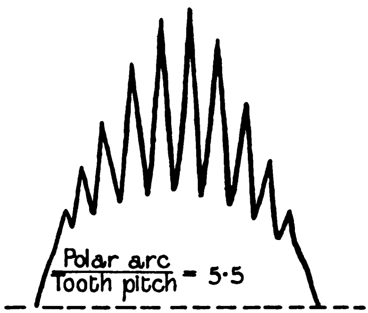


FIG. 7.

Thus similar results have been obtained from three machines widely differing in construction, and it may therefore be concluded that the experimental results given in this paper are of general application.

LOCAL OSCILLATIONS OF THE MAGNETIC FLUX IN THE POLE-FACE AND AIR-GAP.

In the experiments described in the author's previous paper, already referred to, the phenomenon of "phase displacement" was observed. This phenomenon has now been more fully investigated and the displacements measured, and it may be well to introduce this part of the

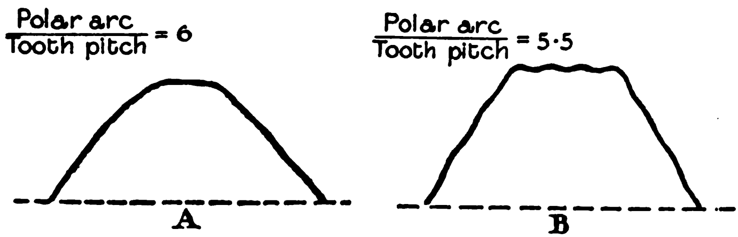


FIG. 8.

subject in hand by repeating briefly here the description of the phenomenon given in that paper. When the conductors in a slot under the pole-face carry current, the local magnetic flux generated by the current has two paths open to it: the one lies across the slot and the other across the air-gap from one tooth-top to the pole-face and back to the next tooth-top. In the case of an open slot armature the reluctance of the latter path, in proportion to that of the former, is usually such that the major portion of the flux proceeds that way. The local flux in that case strengthens the main flux entering one tip of the

tooth, and weakens that entering the other tip. Thus the line of symmetry of the flux is slightly displaced. This effect was called by the author "flux displacement." In the investigation forming the subject of the previous paper search coils were attached to the pole-face for the purpose of observing the local magnetic oscillations due to the passing of the armature teeth; one side of each coil was attached to a N. pole and the other in a corresponding position to an adjacent S. pole; the sides of the coils were parallel to the axis of the armature. When "flux displacement" occurred the phase of the E.M.F. wave generated in a search coil was slightly changed in its relation to the armature teeth. This effect was referred to as "phase displacement."

In the present further investigation of this phenomenon the machine employed was that already described in the opening remarks of the present paper. Three search coils were attached to the faces of

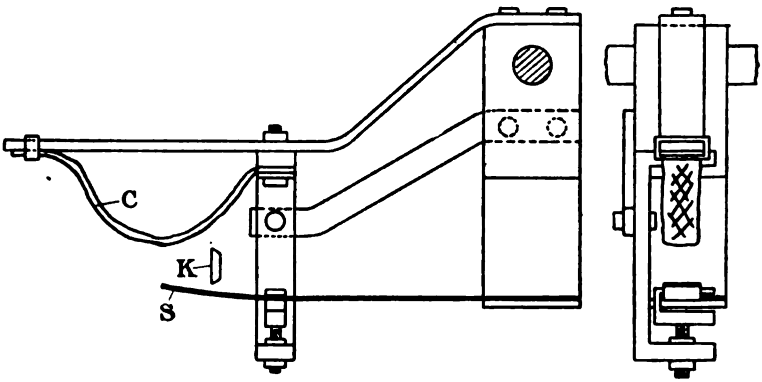


FIG. 9.

adjacent poles, *i.e.*, one of a single turn in the centre and one of three turns at each of the pole-tips. The E.M.F.'s generated were photographically recorded by means of the oscillograph, and a definite position of the armature was instantaneously marked on the record by means of a contact-maker of special design. The construction of this is shown at Fig. 9. It consists essentially of three parts, the steel spring S, the copper gauze C, and the moving steel contactor K. This last is attached to an arm on the machine shaft (not shown) and rotates with it, while the spring S and the gauze C are stationary. The three parts are so adjusted that the contactor K at the same instant flicks the tip of the spring S and rubs the gauze C. Fig. 10 shows the diagram of connections. R is the contact-maker, P is a pair of strips of the oscillograph, T is a search coil, and L is a small accumulator. It will be seen that at the instant of contact the accumulator L is connected in parallel with the search coil T, and hence the current flowing through the oscillograph strips will be momentarily altered, producing

a sharp break in the continuity of the E.M.F. wave being recorded. Fig. 11 shows a record obtained; the break in continuity occurs at P, and this point corresponds to a definite position of the armature relative to the search coil. Only one contact was made in each revolution, and each oscillograph record represented a complete revolution of the machine. The accuracy of the contact-maker was tested from time to time, and proved by the exact repetition of previous records.

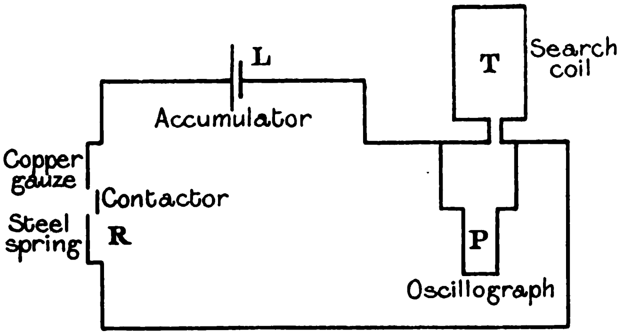


FIG. 10.

When the machine was on load and the armature conductors passing the sides of the search coil carried current, the E.M.F. wave generated was found to be slightly displaced from its position on open circuit relative to the contact mark on the record. This change of position, as measured on the oscillograph record, gives the displacement of the line of symmetry of the flux, or "flux displacement."

Table III. gives the displacements observed expressed as a percentage of the tooth-pitch. In this table the positive sign indicates a forward displacement of the E.M.F. wave on the oscillograph record and hence a backward displacement of the flux relative to the armature. It will be noted that in the case of non-inductive load the displacements are all positive, while in the case of inductive load the displacements change from positive through zero to negative in passing from the trailing tip to the leading tip of the pole. This is due to the distribution of current under the pole-face. When the load is non-inductive the current is in the same direction as the main E.M.F. generated, and hence strengthens the flux in the rear tip of a tooth and weakens that in the forward tip. When, however, the load is inductive the current under the centre of the pole-face is zero, and that under the leading tip is in the opposite direction to the main E.M.F.

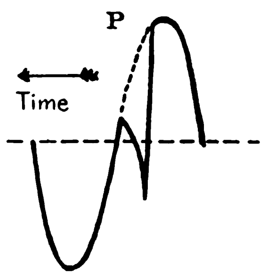


FIG. 11.

It will be further noted that in the case of non-inductive load the

displacements are greatest under the leading tip and diminish towards the trailing tip. This is probably due to the main armature reaction flux weakening the leading and strengthening the trailing tip of the pole.

As successive slots pass a given point in the pole-face the armature current opposite that point would not be quite constant, but the flux displacement was not found to vary within the possibilities of measurement. In a single-phase generator, however, the variation of current and hence the variation of "flux displacement" would be very great.

"Flux displacement" would cause the distribution of eddy currents over the pole-face to vary between no load and full load and with various power factors, and hence the eddy current loss would be also subject to a variation. The periodic variation of the "flux displacement" as successive slots pass a given point in the pole-face would cause the eddy current distribution and hence the eddy current loss to vary periodically also. The magnitude and frequency of this latter would depend upon the type of winding and the number of phases possessed by the machine.

TABLE III.

Load.	Leading Pole-tip.	Centre of Pole-face.	Trailing Pole-tip.
Open circuit	0	0	0
Non-inductive half-load ...	+ 8·0	+ 6·5	+ 3·0
Non-inductive full load ...	+ 15·0	+ 10·7	+ 4·1
Inductive half-load ...	− 2·3	0	+ 2·8
Inductive full load... ...	− 4·5	0	+ 4·1

The results of the investigations described in this paper may be briefly summarised as follows:—

1. There are two kinds of magnetic oscillation in the main magnetic circuit—
   (*a*) A variation in magnitude or "flux pulsation."
   (*b*) A to-and-fro movement of the flux or "flux swing."
2. "Flux pulsation" is a maximum when the ratio $\frac{\text{polar arc}}{\text{tooth-pitch}}$ equals a whole number, and is a minimum when the ratio equals a whole number plus one-half.
3. "Flux swing" is a maximum when the ratio $\frac{\text{polar arc}}{\text{tooth-pitch}}$ equals a whole number plus one-half, and is a minimum when the ratio equals a whole number.

4. The ripples in the crest of the P.D. wave are due to the "flux swing" and those in the zero portion of the wave to the "flux pulsation."
5. When the armature carries current, the flux entering the teeth is displaced (*a*) backwards, when the current is in the same direction as the main E.M.F. generated; (*b*) forwards, when the current is in the opposite direction to the main E.M.F. generated.

The thanks of the author are due to Messrs. Thomas Parker, Ltd., Electrical Engineers, of Wolverhampton, for their loan of the machine and special pole-pieces, and to Professor Marchant for his interest in the work and valuable advice.

The investigations described in this paper were carried out while holding the Research Fellowship established in the Victoria University of Manchester by the Vulcan Boiler and General Insurance Company, Limited.

DISCUSSION.

The CHAIRMAN (Mr. S. L. Pearce) proposed that a very hearty vote of thanks be accorded to Mr. Worrall for his paper, and the vote having been carried by acclamation, the Chairman called upon Professor E. W. Marchant to open the discussion. The Chairman.

Dr. E. W. MARCHANT: In the first place I think Mr. Worrall is to be congratulated on keeping to his title. Some people work up the same material into a number of different papers, and give them different titles in order to distinguish between the papers. Mr. Worrall has given us three quite distinct papers on the same subject. His paper to-night, I think I may say without unduly depreciating the value of the other two, is decidedly the most interesting of the three which we have so far had. I think his observation of the effect of the pole width on the ripples in the E.M.F. wave produced by an alternator is a very interesting one. The method of getting rid of ripples by altering the size of the air-gap at the edges of the pole-shoe is, of course, very well known, but the fact that shaving off the edges of the pole affects these ripples so much has, I think, not hitherto been noticed. I should like to ask Mr. Worrall one question. He says that the experiments of Fischer-Hinnen showed that the noise produced by a dynamo was greatest when the pulsation in the magnetic flux was a maximum. In Fig. 2 there is a maximum for this pulsation when the ratio between the polar arc and the tooth-pitch is 6, a whole number. Turning now to Fig. 3 it will be seen that the best wave is obtained in the case of the same ratio, for with a maximum magnetic pulsation a minimum magnetic swing occurs. Again, with a minimum magnetic pulsation there is a maximum magnetic swing—that is, if we want a machine that is not noisy we must have one with a bad wave, and, *vice versâ*, if we have a machine with a good wave that machine is bound to be noisy. I think if Mr. Worrall could give us some information as to Dr. Marchant.

Dr. Marchant. the actual effect he observed of the noise given out by the machine as dependent on the magnetic pulsation, it would be interesting. The only other point is the ratio between the pole-limb pulsation and the pole-yoke pulsation. Here it will be seen that the ratio is almost exactly 2 to 1; that, I think, is a very interesting result, because one would have expected with a magnetic oscillation of this sort that the further away from the source of the oscillation round the magnetic circuit, the smaller the oscillation would become. This seems to show that the magnetic pulsation is practically the same all through the magnetic circuit beyond the pole-shoe, but within the pole-core there is a certain pulsation, and in the pole-yoke the pulsation is exactly the same as in the rest of the magnetic circuit. That seems to me a very striking and very interesting result.

Mr. Peck. Mr. J. S. PECK: I think the author is to be congratulated on the way in which, after obtaining certain results, he has been able to account for them. It is very desirable to eliminate the ripples from the waves of alternating-current generators, because with such extremely high frequencies as may be obtained, where the ripples are produced by the teeth, there may possibly arise conditions similar to that shown in Fig. 7, where these high-frequency ripples resonate with the circuit, in which event very high voltages will be produced. On alternate-current generators the semi-closed slot would help matters very much, as would also increasing the number of slots, because the smaller the number of slots the wider become the teeth and slots and the greater will be the flux swing. Regarding the latter part of the paper, I am rather surprised at the statement on page 424: "As successive slots pass a given point in the pole-face the armature current opposite that point would not be quite constant, but the flux displacement was not found to vary within the possibilities of measurement." I should have thought that even with a 3-phase alternator there would be sufficient difference between the currents in the slots at different times to make quite a decided difference in the measurements. Mr. Worrall states that with a single-phase armature the effect would be marked, and I should like to ask him whether he has ever made any tests on a single-phase alternator to find out what this flux displacement amounts to.

Mr. Cramp. Mr. W. CRAMP: It seems to me that there are three types of papers read at meetings of the Institution, namely, those which deal with experimental facts, those which deal with theoretical possibilities, and those which deal with practical applications. The paper to-night belongs to the first class, and we are told on page 421 that the "results are of general application." One is left to draw one's own conclusions as to the practical applications which may be made of the work in this paper, and if I suggest a few of these it is only with the object of asking Mr. Worrall whether I am justified in the inferences which I draw. In the first place it seems to me that there are three types of machines concerned with this question of magnetic oscillation. First there is the direct-current machine. There is no doubt that a

Mr. Cramp.

great many direct-current machines have given trouble on account of the noise which they give out. There is also very little doubt that the noise is due to the flux changes, either pulsations or swings, and I take it that Fischer-Hinnen's formula given in the paper refers particularly to such noises in direct-current machines. In any case there is a vibration set up by the armature teeth, which seems, at some particular speed, to correspond with the natural period of vibration of some part of the machine, usually, I think, the pole-tip. But I have come across one case in which the noise is very loud at a particular speed, and that machine has no pole-tips, and, moreover, it has a cast steel pole, which makes the case the more extraordinary. Now the question arises as to whether such noise is due to what is called "swing." I take it that Fischer-Hinnen thinks it is due to pulsation. If it is due to pulsation, then we have evidently to conclude, from this paper, that we must use a ratio of polar arc to tooth-pitch corresponding to the 5·5 case; but if it is due to the swing, we shall have to use as ratio a whole number.

Next there is the case of the alternating-current motor. Every one who has dealt with alternating-current motors knows that there is a good deal of difficulty in avoiding what is known as "cogging," and that such cogging is due to variations of reluctance in the magnetic circuit, due to the effect of the teeth passing the slots. To get rid of cogging, it is usual to choose a prime number of slots for the rotor. If we cannot choose a prime number of slots for the rotor, as sometimes happens, then we must, according to this paper, choose a number of slots so as to get the minimum main magnetic change, that is, corresponding to that which Mr. Worrall gives as 5·5.

Then there is the case of the alternating-current generator, in which we have with one ratio a big change of flux, and with the other the ripples, both of which should be avoided. Mr. Worrall says in order to obtain the minimum change of flux we should use the ratio polar arc to tooth-pitch $5\frac{1}{2}$, and in order to get rid of the swing, we should use a whole number for the ratio. Now it seems to me to follow from Fig. 4 that the swing entails a complete moving of flux right over the pole-face. For the production of those little ripples on the top of the no-load E.M.F. wave means probably that the whole flux moves across the pole-face, which I do not think would have been expected. If the whole flux does move across the pole-face, it is evidently possible to get rid of the ripples on the top of the wave by putting slots in the pole-piece itself, so that the reluctance of the part under the pole shall change in the opposite direction to that in which it changes when the slots and teeth leave the pole. Thus by adopting the proportions of pole arc to tooth-pitch suggested by Mr. Worrall for reducing the pulsation to a minimum, and by making use of my present suggestion for then getting rid of the ripples, it ought to be possible to design an alternator with practically no magnetic oscillations whatever. So in all three cases (if my deductions are correct) this paper leads to a marked improvement in design. There are one or two further ques-

Mr. Cramp. tions I should like to ask Mr. Worrall. In the curve shown in Fig. 2, the change in the flux seems to me very abnormal, and one wonders whether there is anything curious in the construction of the machine. I notice that the air-gap is only about 0·1 in., which seems very small for the size of the machine, so that one questions whether the conditions are quite those met with in ordinary practice. The width of the slot is not very extraordinary, and the ratio of the air-gap to slot is not very extraordinary, but the ratio of the air-gap to slot depth is rather less than one would meet with in ordinary practice. Are we justified in concluding that these figures, 5¼ and 6, would apply to machines with ordinary air-gaps? In Fig. 3 Mr. Worrall shows the small oscillation at the top of the wave due to "swing." Why is it, then, that we do not see small oscillations down the side of the wave and that there is no sign of the pulsation ripples? Referring to the remark on page 420, "When the flux is subject to a 'swing' the E.M.F. ripple will be generated in an armature coil only as the sides of the latter are actually cutting the flux, that is, when they are under the pole-face"—on referring to Fig. 7 I find the chief ripples occur right in the middle of the top wave. I should have expected them to occur much nearer the pole-tip, that is, nearer the edge of the wave. I should like to ask Mr. Worrall whether the air-gap length does not come in again there.

Lastly, I should like to ask Mr. Worrall whether he has anything to offer us in the way of conclusions to be drawn as to the effect on this particular alternator of using closed slots, a most important question both to the designer of induction motors and to the designer of alternators.

Mr. Frith. Mr. J. FRITH: With regard to the noise of machines, my experience has been much the same as Mr. Cramp's. The machine I am thinking of also had no pole-tips. I do not believe at all that the noise was due to a pulsation in the main flux; I believe it was entirely a mechanical effect on the pole-tip due to the intermittent magnetic pull of the teeth. I have noticed it quite as much with machines with laminated pole-shoes, and it can nearly always be cured, on certain classes of machines at any rate, by using the same average width of pole, but staggering it across the slots as many foreign makers do. I think the way out of the other difficulty, as Mr. Peck suggests, is to put many more slots. This machine (if it were a polyphase machine) would have three slots per pole per phase, which is rather few; and obviously on larger machines we shall use a much larger number of slots, so that one or half a one less or more under the pole does not make very much difference to the total number.

With regard to the tooth ripples being very much more marked on the top of the curve, is not that partly due to the fact that they do not show so much when they are on the sloping sides of the curve?

Dr. Rosenberg. Dr. E. ROSENBERG: We are very much indebted to the author for having investigated this matter by experiment. The theory in itself cannot do very much in clearing up questions like these, and we see, for instance, that Simons as well as Arnold and La Cour, by a theoretical

consideration, came to exactly the opposite result. Now we see facts. What we must always bear in mind is that these facts have been attained only on one particular machine, and that the results are valid only for this machine, and we must not generalise. For this particular machine, with this particular air-gap and particular size and number of slots, the magnetic pulsations are the smallest for a pole-face equal to 5½ slots, and the biggest for a pole-face equal to 6 slots. If we alter the air-gap, and alter the number of slots per pole, the results may be different. It is interesting that the author has come to the same conclusion as Fischer-Hinnen in his researches on humming; but I may also say that the results obtained by Professor Hinnen are not applicable to all kinds of machines, and there are many motors which could not be made noiseless by applying Fischer-Hinnen's rule. Figs. 5 and 6 apparently give a theoretical explanation for the results obtained. Mr. Worrall took exactly a fringe of 45°, but of course the fringe has no definite limits. We know that it gradually decreases, and if we choose any other angle for the fringe the result is quite different. Dr. Rosenberg.

The ripples on the E.M.F. wave have a specially marked result if a dynamo works in parallel with another dynamo, or is driving a synchronous motor with another wave-shape. Then the higher harmonics produce a current in a circuit that contains nothing but the self-induction of the two machines, and this current may be higher than the main current. I once published * a no-load current curve of a synchronous motor with harmonics of the 5th, 6th, 7th, and 11th order that were nearly three times as big as the value of the fundamental current. The fundamental curve shows 2·6 amperes, and the current measured was 7·2 amperes. In another case the ratio was 5·5 and 15. I should like to ask Mr. Worrall in what manner he succeeded in getting exact resonance in his Fig. 7.

Dr. C. C. GARRARD: I do not wish to discuss Mr. Worrall's paper from the point of view of the dynamo designer, but would just remark that the practical importance of the subject is extremely great. Therefore all researches which might lead to the elimination of these ripples in wave-forms are of very large importance. Ripples on the wave of E.M.F. are of great importance with 3-phase star-connected machines. In this case, all harmonics which are multiples of three can flow out of the neutral point. I have heard of cases where transformers have been connected in star, and the currents flowing through the neutral point have been sufficient to render the transformers extremely hot under conditions of no load. Star-wound alternators in parallel which have their neutral points connected together are another case. The ripples in the waves cause, under some conditions, very large currents to pass between the neutral points. I have come across one station where by adjusting the excitations of machines in parallel it was possible to motor one machine from the others by means of the higher frequency currents flowing through the neutral. At the same time, however, the motored machine was giving out power at the fundamental frequency Dr. Garrard.

* *Elektrotechnische Zeitschrift*, vol. 24, p 111, 1903.

Dr. Garrard. from its phase terminals. In many cases, owing to neutral point currents, it is not possible to earth the neutral points of all the generators in the station. In cases where it is desired to earth the neutral point, it has now become a general practice only to earth the neutral point of one single generator. I know of a large 3-phase station in which it is proposed to install disconnecting switches, which are to be so interconnected with the main generator switches that only one neutral point can remain earthed at a time. If the machine whose neutral point is earthed is switched off, then the neutral point of another machine is to be automatically connected to earth. This, it will be realised, is a very expensive and complicated business, and if the machines had sine waves all this special gear could be dispensed with. I merely mention these points in order to show the importance of the subject with which Mr. Worrall has been dealing. It is a very large subject, and no doubt Mr. Worrall's results will be of great value to designers. Of course, all the irregularities of wave-forms are not due to the causes dealt with by Mr. Worrall in his papers. There is, for example, the irregularity introduced into the magnetising currents of machines due to hysteresis.

Mr. Worrall. Mr. G. W. WORRALL (*in reply*): Dr. Rosenberg appears rather to depreciate theoretical considerations in favour of experimental facts, but while theoretical considerations alone are likely to lead to wrong conclusions, experimental facts alone are subject to only particular application. It is much better either to prove theory by experiment or generalise experiment by theory. This latter I have endeavoured to do, and while admitting that many of the quantitative results given are necessarily of only particular application, I believe that the summarised results at the end of the paper are of general application. In this I am to some extent justified by the potential difference curves given in Fig. 8. It would be of very great interest and value if any one could either confirm or contradict these results by their own practical experience.

Referring in detail to the remarks made, Professor Marchant asked me about the humming. Although I did not particularly observe the humming, according to my ear the pitch remained the same for all sizes of polar arc, while the intensity was greatly reduced as the "pulsations" decreased.

Mr. Cramp asks if the small air-gap may not be the cause of the large change in the magnitude of the "pulsations." I do not think so, as the percentage change in magnitude of the "pulsation" as the polar arc is altered depends only upon the number of teeth covered by the pole. Regarding the ripples in the zero portion of the wave, due to what I term the "pulsation" of the flux, one reason for their apparent absence in the particular waves shown has already been given by Mr. Frith in the discussion, and that is, the wave is more or less vertical in that portion, and therefore the ripples would not be so apparent; but I think there is another reason, and that is that any variation in the main flux is bound to be subjected to a damping effect of the iron; and it

Mr. Worrall.

will be noticed from the tables I give that the actual percentage "pulsation" of the flux is not very large: the maximum in the pole-shoe is only 0·2 per cent. of the total flux, and the maximum in the limb is 0·0094.

With regard to the use of closed slots. In a previous paper I just touched upon the subject of the oscillations of the main flux. I showed there that the introduction of a bridge in magnetic contact over the slot made very little difference to the change in the whole flux as the teeth passed the pole-shoe; that is probably due to the fact that the reluctance of the whole circuit and the reluctance of the air-gap is so great that any thin bridge over the slot will hardly have much influence, but, of course, it has a very great influence on any local oscillation of the flux in the air-gap.

Mr. Peck drew attention to the effect of "flux displacement" in single-phase generators. I have not made any observations of this, so that I cannot say experimentally whether it takes place, but it appears to me reasonable that at all events there would be a very great deal more displacement in the case of a single-phase than a 3-phase generator.

Dr. Rosenberg states that I take a fringe of 45°; this is not quite correct. The boundary of the fringe I give in Figs. 5 and 6 is drawn from the pole corner to the tooth-tip, and I assume that if a tooth is not to some extent under cover of the pole-face practically no flux enters it. Resonance was obtained by placing a small capacity, about 6 microfarads, across each phase of the machine. The speed was then adjusted for maximum current, and in the actual experiment this was but a few revolutions below the normal. The resonance obtained was, of course, for the higher harmonic and not for the fundamental frequency.

LEEDS LOCAL SECTION.

THE COMMERCIAL ASPECT OF ELECTRIC POWER SUPPLY.

By W. B. WOODHOUSE, Associate Member.

(*Paper received from the* LEEDS LOCAL SECTION; *October* 9, *and read at Leeds, December* 19, 1907.)

Although it is now becoming more generally recognised that the development of a general supply of electric power in the industrial areas of England is a matter of vital importance to the country at large on account of its commercial value, there are yet many obstacles in the way of the full realisation of such a development. As these obstacles are largely due to an imperfect conception of the advantages and possibilities of centralisation, it is hoped that a general discussion of the commercial aspect of the question, together with a consideration of the future trend of developments, may be of interest.

Progress of Legislation.—A glance at the history of electricity supply in this country shows that in the beginning the business was built up entirely on the basis of supply in isolated areas, the boundaries of which, for the sake of convenience, were made coincident with those of parishes or other administrative districts. For various reasons it was enacted that a company wishing to undertake the supply of electricity in any area must obtain the consent of the local authority concerned, to whom also was conceded a right of veto without necessity of adducing a reason for their action. This privilege, which has acted so harmfully in retarding the development of the industry, was probably granted in order to prevent the creation of a monopoly similar to those enjoyed by previous concessionaires, such as gas, water, and railway undertakers, which have in certain cases proved somewhat burdensome. Recent events would appear to show that this fear of establishing an injurious monopoly still haunts the minds of our legislators, despite the fact that the supply of electricity for light or power is subject to as severe competition as any commodity which can be mentioned. To quote Mr. Ferranti, the monopoly conferred on authorised undertakers is, if it exists at all, "a monopoly of cheapness."

The electric lighting business developed along the definite lines laid down by the framers of the early Acts, but the growth of the use of electric power for urban tramways and for general purposes considerably altered the working conditions of the existing supply stations. The overflow of the population of large towns beyond their boundaries, the development of inter-urban tramways, and the foreshadowing of a

general electrification of the railways, led to the passing of Power Acts as a solution. The provisions of these Acts are based on the recommendations of Lord Cross's Committee of 1898, in which the benefits of supply over a large area and the damaging effect of the veto and powers of purchase possessed by local authorities were recognised.

The first batch of Power Bills was considered in 1900 by a committee presided over by Lord Airedale (then Sir James Kitson), and the declaration of that Committee as to the lines on which clauses were to be settled marks an important step in the progress of electric power supply. Prejudices die hard however, and the local authorities devoted their energies to the crippling of power supply by the insertion in Power Acts of restrictive clauses; not only was the veto of the local authority maintained in the case of towns having their own electricity works, but also in many cases where no supply of any kind was available. A comparison of the Acts of 1900 with those of later years shows the disastrous effect of such opposition.

The South Wales Act of 1900 may well be compared with the Lancashire Act of 1901 in this respect.

In 1902 our Institution marked its sense of disappointment with the state of affairs in the Resolutions of March 25th, which are worthy of quotation in full (see Appendix), but it is lamentable to state that legislation on this matter is yet unaltered.

The Technical Problem.—While the legislative changes mentioned above were being made, the technical problems of electricity supply had also changed in a remarkable degree. The growth of the demand for electricity for power purposes has led to the adoption of larger generating units, which has facilitated distribution over a larger area at high pressure, and the economy of the large generating station—and inferentially the uneconomy of small stations—is now an accepted fact.

The commercial limitations to the size of a generating station which underlie the whole financial problem of power supply may be briefly summarised as follows:—

The argument for centralisation is—

1. Reduced capital expenditure per kilowatt of generating plant due to increased size of units.
2. Increased efficiency of plant.
3. Improved load factor due to diversity of requirements and demand.
4. Reduced wages and management charges.
5. Small proportion of stand-by plant.

Against which must be set the disadvantages of:—

1 Extra capital expenditure on transmission.
2. Losses in transmission and conversion.

Practice has shown that the small generating station cannot produce electricity at a price sufficiently low to enable it to be supplied to power

users at a competitive price. A station of 1,000 k.w., for example, cannot hope to supply a power user requiring 300 k.w. as cheaply as the power user can generate for himself; this for several reasons. First, the capital cost of the generating plant in both cases will be practically the same per kilowatt, while the public supply is burdened with an additional cost for mains; secondly, the owner of an isolated power plant will usually instal a smaller proportion of stand-by machinery, setting the increased risk of shut-down against the reduced capital charges; thirdly, the public supply station must run continuously, often at very inefficient loads, whereas the isolated plant, a mill engine for instance, is only run during certain definite hours, so that the "working load factor" is greater and the works costs less in the case of the latter. Apart from this one finds that the owners of isolated power plants very frequently do not include any allowance for rent, management charges, etc., in estimating their power costs, so that the difficulty of the supply authority obtaining such a load is increased by the power user's want of appreciation of all the items making up power costs. Assuming the power user's load factor to be equal to that of the supply station, the author's experience is that the limiting size of a consumer's installation, which can profitably be supplied from the central station, is from one-tenth to one-fifth the capacity of the station plant.

As, however, the size of the generating station is increased, the advantages of centralisation come into fuller play, although to some extent counterbalanced by the increased capital expenditure on distribution and by the increased losses. These disadvantages of centralisation depend on local conditions, such as the density of the power load per mile of main, and on the nature of the load supplied. In any particular case a balance will occur at some point; that is to say, there is a limit to the economical radius of distribution, and, therefore, to the size of the generating station required. It does not appear that there is any limit to the size of the generating station on the score of efficiency and cost per kilowatt.

Capital Cost.—To take round figures, the capital cost of generating stations of various sizes is as given in Table I.

TABLE I.

Capacity of Station.	Cost per Kilowatt.	Relative Cost per Cent.
Kilowatts.	£	
60,000	10	100
6,000	15	150
600	20	200

That is to say, if one hundred consumers each requiring 600 k.w. combine together in a joint station, the total capital expenditure

on generating plant would be one-half of that required for isolated plants.

Efficiency.—The improvement in efficiency due to the use of large units may be seen by considering some typical figures of turbine steam consumption (Table II.) To the full load test figures the author

TABLE II.

Size of Generating Set.	Test Figures. Full Load Consumption in Lbs. per Kilowatt-hour.		Working Figures.	
	Water.	Coal.	Estimated Coal Consumption.	Relative Cost per Cent.
3,000	15	1·875	3·125	100
600	20	2·500	4·160	133
300	24	3·000	5·000	160

has added an estimate of the average working figures in stations with a load factor of 25 per cent.

It will be seen that the coal consumption of the smallest unit is 60 per cent. in excess of that of the largest.

Load Factor and Diversity.—Supply over a large area brings with it the supply to power users with diverse requirements—lighting, traction, general power users and collieries, for example, each have different hours of use, and in each case the peak load occurs at a different hour in the day.

TABLE III.

Item of Cost.	Load Factor. 15 per Cent.	30 per Cent.	60 per Cent.
Coal	100	70	55·0
Wages	45	25	12·5
Stores	10	8	6·0
Repairs	50	40	30·0
Total	205	143	103·5
Relative cost per cent.	198	138	100

Experience shows that the station demand will not exceed one-third of the total lighting connected to the system, nor one-half of the power installed by power users, and that if the individual maximum demands of a number of power users be added together that figure will be from 50 to 100 per cent. in excess of the demand at the generating station with a consequent improvement of the load factor.

Works Costs.—As to the saving in wages and management charges in the larger station, this depends on so many variables that general figures are not of great value. Since, however, one man can attend to a 3,000-k.w. set as well as he can to a 300-k.w. set the saving is obviously considerable. The effect of load factor on works costs is still more important.

Table III. is a statement drawn from the author's experience of the effect of load factor on works costs.

Summary of Advantages.—Summarising the advantages of centralisation and the consequent diversity of demand, it will be seen that diversity of demand has effect both on capital and running cost :—

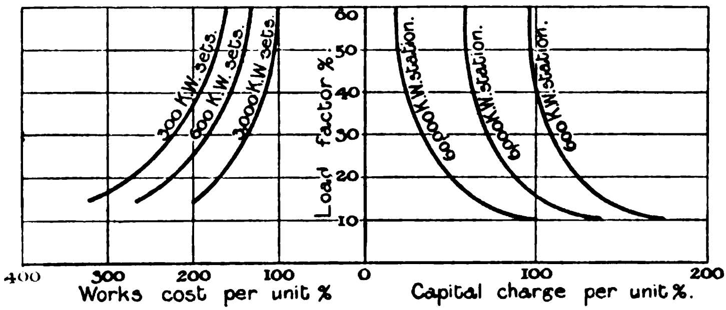


FIG. 1.

The capital cost of generating plant per kilowatt installed in the large power scheme may be taken at one-half that of a small power station.

The diversity factor will approach two, *i.e.*, the large scheme has the further advantage of requiring only one-half of the plant required by the individual consumers if isolated stations were erected, or, taking the two advantages together, a capital expenditure per kilowatt of demand of one-quarter.

The works costs are reduced both on account of increased size of generating sets and improved load factor, the latter advantage representing a saving of 50 per cent. over the works costs of the average power user.

We may express the total saving due to centralisation by a curve (Fig. 1).

It will be seen that before equality of expenditure on isolated and public supply plants is obtained, the expenditure on transmission

and distribution may be three times as great as that on the station, and the losses and costs of distribution may be increased by 100 per cent. Naturally these extreme figures are never reached in practice, and therefore the margin in favour of the power company is a very considerable one.

Sources of Fuel.—The prospect of the generation of electricity on a large scale has led engineers to seek for new sources of fuel, and the generation of power by the utilisation of waste heat from blast furnaces, coke ovens, and similar thermo-chemical processes now forms a separate study. It may be safely predicted that future developments will consist in the co-operation of electric supply undertakings with processes of manufacture in which fuel or heat is merely one of several products to be dealt with. The conditions of the electricity supply will no longer be the sole factor in determining the situation or magnitude of a generating station, but the economical handling of the other products of manufacture will also have a determining effect upon the decision.

The sources of power such as those above mentioned either are or should be placed outside large residential towns, and the utilisation of such sources to their full effect would appear to involve the joint working of a number of stations. Such joint working can only be economically carried on by running certain of the stations at their full capacity continuously, fluctuations of demand on the system being dealt with by stations specially designed for the purpose. For example, there are a number of collieries in the West Riding where waste heat from coke ovens is available to an amount equivalent to from 1,000 to 2,000 k.w. To erect at each battery of ovens a generating station with spare plant sufficient to ensure a continuous supply in the event of breakdown and to supply from such a station a fluctuating load such as a general power supply demands is not an efficient arrangement. The high cost of the small generating units per kilowatt, the cost of management, and the partial use made of the plant all tend to increase the cost of supply. When, however, a number of such stations are worked in conjunction by a power company, the amount of spare plant may be reduced to a minimum, and each station would consist of a single generating unit, with no spare plant, running continuously at full load or else shut down completely. The resultant economy of capital expenditure and operating costs of such an arrangement is considerable. This is rather a reversal of the present idea of the peak load station, the author's view being that the station dealing with fluctuations would contain the biggest and most economical units and would be the largest station of the system.

Electro-Chemical Development.—The development of electro-thermal and chemical processes introduces a class of load of high load factor, which, added to a general supply, increases the load factor on the system. But where, as is usual, the demand is a large one the price at which it could be generated in a station designed specially for that purpose is so low that only a large undertaking could offer a supply at a

corresponding price. There is, however, a possibility of development in the use of a restricted supply of current varied according to the power station's requirements and designed to equalise the demand on the station. The curves, Figs. 2 and 3, may make this clearer. Fig. 2 represents a typical variation of maximum demand on a station throughout a year, the growth of load being accompanied by the seasonal variation due to lighting, etc. Fig. 3 is a typical load curve for a winter's day on a station supplying light and power.

It will be seen from the two curves that there is necessarily a

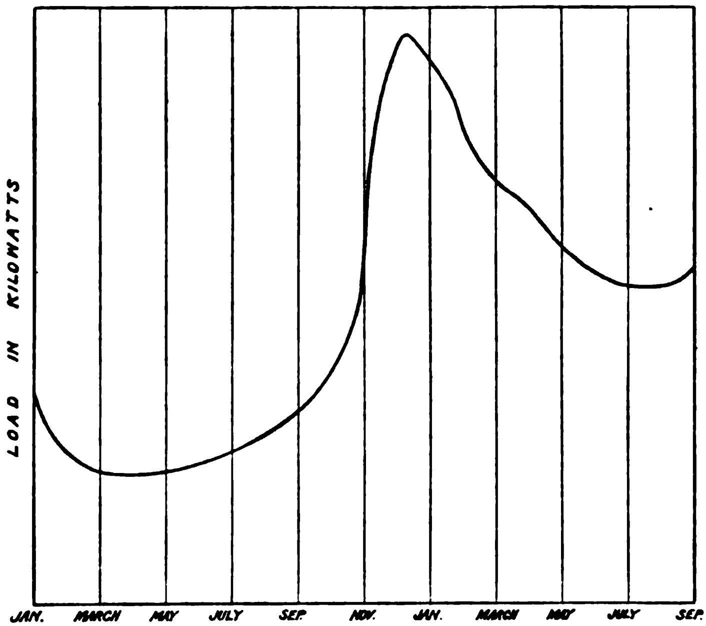


FIG. 2.—Typical Curve of Peak Loads, showing Monthly Variations, accompanied by Growth of Connections.

considerable amount of generating plant which is lying idle for the greater part of the year, its use being, in fact, only required for a few hours a day in the winter months.

If a load can be found which may be varied inversely as the normal demand, then a profit may be earned by such machinery throughout the year. The author has recently arranged an agreement on these lines for the Yorkshire Electric Power Company with a company manufacturing calcium carbide by electric furnaces. The improved conditions under which the station will operate should show a considerable decrease in the average costs of production.

Co-operation.—The arrangements mentioned above involve co-

operation between those who produce waste heat, the general power user and the power company, and, in fact, the power company may be regarded as a joint committee of such manufacturers. The co-operation of collieries alone through such an agency would effect

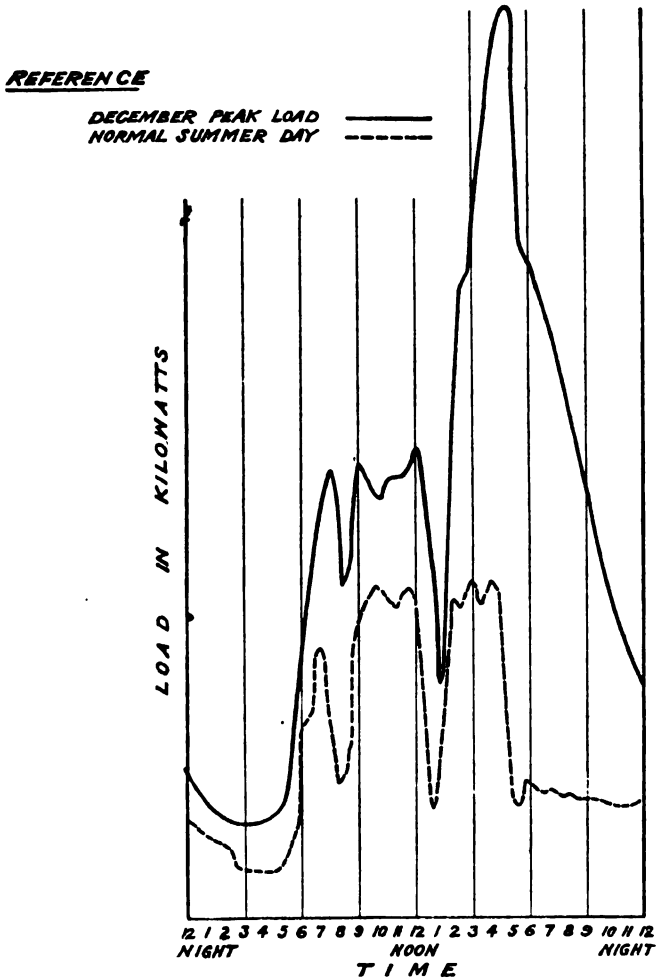


FIG. 3.—Typical Load Curves, Lighting and Power.

an enormous annual saving. The output of coal for Yorkshire in 1906 was 33 million tons, and assuming, as we fairly may, that 5 per cent. of this was used for power purposes by various collieries, we have an amount of fuel which, if consumed by the power company under

their economical conditions of working, would supply not only the collieries but practically all the textile mills in Yorkshire.

The blast furnaces of the kingdom also produce a large amount of waste heat, and a very large portion can be utilised by co-operation with the power companies.

The considerations dealt with above as to the advantages of a public supply to a power user apply with even greater force to small and medium-sized public supply generating stations, say, of less than 10,000 k.w. Co-operation between authorised undertakers seems to the author a vital necessity if progress is to be made. The older generating stations are situated in the centre of the towns they supply, and the developments in the economical generation outlined above cannot in such circumstances be taken full advantage of. Furthermore the large number of undertakings which are under municipal control are for this reason not in a position to deal with the problem as freely as a company can, even were it advisable for them to do so with money borrowed on the security of the rates. Where distribution of electricity is considered, however, the local authority controlling an electrical undertaking have more justification for dealing with the matter themselves—their local knowledge and the fact of being the road authority are good reasons for undertaking the distribution of electric power provided that they are prepared to offer as cheap a power supply as can be obtained in other districts. There is no doubt that local authorities have in many cases hesitated to allow a power company to supply in their area from a fear that the matter might grow in the course of years to be beyond their control. The joint control of a supply by a company and a corporation is an arrangement which the Board of Trade do not favour, and possibly it is one which would present many difficulties in working. It has occurred to the author that a way out of the difficulty might be found if the local authorities were empowered to become debenture holders in the company. They would thus exercise some control over the business if improperly managed and the provision of further capital would depend on the satisfaction given by the company to the local authority. This is, however, to look into the future and in the meantime economic conditions are bringing home to the local authorities the advantages of the general power supply in the face of many prejudices.

Smoke Nuisance and Decentralisation.—Of causes other than the direct advantages of power supply, which are assisting the development, two may be mentioned.

The abolition of the smoke nuisance in towns is receiving increased attention on all sides, and there are undoubtedly signs of decrease of the nuisance in towns due to the use of electric motors and gas stoves, as well as to the more stringent action taken by some local authorities. Doubtless the improvement in the atmosphere of districts where the electric supply is generally adopted will become so noticeable in a few years that other districts will be forced to take action. It is obvious,

however, that strong action cannot be taken fairly by the local authority unless a cheap power supply is available in their district, and they must first see that such a supply is provided. But a more important reason for the action of local authorities in the matter of a cheap power supply is the steady industrial exodus that is going on from all the large towns. High rates, crowded sites, and dear power all act as an incitement to a manufacturer to remove to a cleaner and better place. The facilities for transit of men and material have so increased of late years that with the aid of electric power we do seem to be approaching the ideal of industrial decentralisation, if not a return to cottage industries.

The ultimate effect of this decentralisation on the finances of large towns is a matter worthy of the most serious consideration.

Sales.—The sale of electricity can be developed by a well organised system such as one applies to the sale of any other commodity, but it can only be carried out successfully by a careful study of the requirements of the various power users and therefore by a staff of commercial engineers. To any one familiar with the state of power supply six years ago the development which has taken place in the business on the initiative of power companies is perhaps marvellous, but to the one who has knowledge of the possibilities of development yet to come the results are somewhat meagre.

The fact has been impressed on the industry lately by the technical press, and with considerable justice, that the selling organisation of most supply undertakings has been somewhat neglected. Here again is room and a necessity for co-operation amongst the various supply authorities. Any one who, like the author, has had to do pioneer work in the electrification for the first time of an industry such as the textile, realises how valuable may be an accumulation of data relating to the power taken by various machines, the unit consumption, the load factor, etc. If every undertaking were working together, such information could be collected by a central bureau and would be available to all. This refers not merely to technical data, but also to such points as legislative restriction and government regulations. For example, we cannot hope to obtain a general improvement of the law with regard to easements for overhead lines until a strong case is made out by the collection of data from all parts of the country as to the present disadvantages suffered.

Of much greater value would be a central advertising association. One is only too apt to assume that the general public are familiar with all the advantages and developments of the use of electric power. It may be safely said that the general public is more ignorant, except perhaps as regards electric tramways, of the extent of electrical development (in which, in this country alone, over £340,000,000 of capital is employed) than of any other industry of similar importance.

This can only be removed by systematic advertising and circulation of information, and this, again, can only be properly carried out by a combination of supply authorities each contributing to a common

fund for such work. A contribution of 1 per cent. of the nett revenue per annum would allow advertising to be carried out on a scale sufficient to give a great impetus to the business. On a small scale the various power companies are co-operating in this way through the medium of "*Electrics*," and the London supply companies also have a joint publication ; an advertising combination of every supply undertaking, municipal or company, would, however, be of enormously greater value.

The question of tariffs is one which cannot be dealt with within the limits of this paper. One thing may be said, namely, that electrical engineers have generally followed the principle laid down by Dr. Hopkinson in attempting to treat each consumer quite fairly and asking payment on a scale which varies the charges in proportion to the individual cost of supply. The old and pernicious principle of railway charge, which has been summed up as charging "what the traffic can stand," is open to damaging criticism. We are gradually establishing in the minds of the public the opinion that although our tariffs are somewhat difficult of comprehension, yet their basis is a just one. This must in the end be to the good of the industry as strengthening the confidence which is so necessary to the success of a public supply.

APPENDIX.

RESOLUTIONS

PASSED BY THE COMMITTEE OF THE INSTITUTION OF ELECTRICAL ENGINEERS ON ELECTRICAL LEGISLATION, *March* 25, 1902.

ADOPTED BY THE COUNCIL, *April* 10, 1902.

1. That, notwithstanding that our countrymen have been among the first in inventive genius in electrical science, its development in the United Kingdom is in a backward condition, as compared with other countries, in respect of practical application to the industrial and social requirements of the nation.
2. That the cause of such backwardness is largely due to the conditions under which the electrical industry has been carried on in this country, and especially to the restrictive character of the legislation governing the initiation and development of electric power and traction undertakings, and the powers of obstruction granted to local authorities.
3. That local boundaries have usually no reference whatever to the needs of the community in regard to electric supply and traction ; that the selection of suitable areas should be dealt with on the basis of economic principles and industrial demands ; and that this has been found to apply to gas-, water-, and sanitary engineering.

4. That the development of electric power and traction undertakings offers the most favourable means of relieving congested centres, and of thus contributing towards the settlement of the housing question.
5. That it is expedient in the national interests that the Electric Lighting Acts 1882–8, the Tramways Act 1870, and the Standing Orders relating to special Acts for Tramways should be amended in so far as they enable local authorities to veto or delay the carrying out of electric supply and traction projects of which the utility can be shown; and that effect should be given to the recommendations of the Joint Select Committee of Parliament, 1898, on "Electrical Energy—Generating Stations and Supply."
6. That excessive time is occupied and expense incurred in obtaining authority to carry out electrical undertakings, and that important and growing industries are thereby checked.
7. That while this Committee fully recognises the ability of the technical officials of the Government Departments concerned, it is of opinion that the staffs of those Departments, as at present existing, are wholly inadequate having regard to the great industrial interests involved; that it is essential that these Departments should be put into a position enabling them to keep in touch with all developments in engineering matters, both in this country and abroad, and that a sufficient sum should be provided annually by Government to enable them to employ and pay a proper staff for such purposes.
8. That the adjustment of departmental regulations to engineering development should not be delayed until the industrial interests concerned are seriously hampered, and that, with a view to preventing any such delay, the Institution of Electrical Engineers should be willing to take part in revising such regulations from time to time.
9. That this Committee recommends that the Institution should memorialise the Prime Minister to receive a deputation for the purpose of urging the removal of the present disabilities and restrictions which prevent electrical engineering from making the progress that the national interests demand, and attaining at least the same level as in America, Germany, and other industrial countries.

Discussion

Mr. S. D. Schofield: The author states on page 434 that a station of 1,000 k.w., for example, cannot hope to supply a power user requiring 300 k.w. as cheaply as the power user can generate for himself. He then goes on to point out that the two circumstances are not on a par; that the private user will not, or cannot, bring out all his charges, and that he neglects to take into consideration rent, management, and other charges in estimating his power costs. In consequence of the omission of these items, it appears that he cannot Mr. Schofield.

Mr. Schofield.

hope to get power from a supply company as cheaply as he thinks he can generate for himself. I wish Mr. Woodhouse had given figures showing the effect of omitting these charges, and how easily it is for these private users to produce misleading figures as to the cost per unit generated with their own plant. In one or two cases I have found that upwards of 50 per cent. of the charges properly due to the private plant have been left out of the calculation altogether.

With regard to the test figures on page 435, I find that test figures at full load are very rarely obtained in a central station, whatever size the plant may be. Take a 3,000-k.w. or a 5,000-k.w. set, or come down to a 200-k.w. or a 500-k.w. set. A certain steam consumption on full load is promised by the makers, and on test this result may be obtained; but how often does it occur that one has a load that is too big for one and not large enough for two sets to be run under their most economical conditions. Suppose we have a 4,000-k.w. load with 3,000-k.w. set, turbine driven, with an auxiliary valve for admitting high-pressure steam to carry the overload, the result will be an increased steam consumption, which considerably discounts the figures shown in the paper.

On page 440 Mr. Woodhouse says that municipal undertakings are not in a position to have combined stations where one station serves three or four towns. It is not the fault of engineers that these stations are not more numerous. Regarding his suggestion that municipalities might become debenture holders in power companies, I think that to induce a Corporation to take debenture shares in a supply company would be a more difficult proposition than to induce two neighbouring Corporations to have a joint central station for their own use. I am quite in accord with the author on page 441 that a central bureau should be established for detailing information which is collected from time to time. In twelve months an engineer may have perhaps sent out three or four circulars asking for information, and received anything between thirty and forty other circulars. Each engineer gets this information out for his own council or company, and files the matter away. Two or three weeks afterwards some one else requires the same information, and he has to send out circulars again; whereas, if it were filed at a central bureau, it could easily be consulted by anybody in the industry, and it would be a great advantage and save others a considerable amount of time.

With regard to the question of tariff, this certainly is a subject in itself sufficient for one evening's discussion, but no one can pretend to deal with central station supply unless the tariff is dealt with at the same time, because the crux of the whole matter is the rate per unit. I am rather afraid both municipalities and power companies are running too near the dividing line, between loss on the one hand and making a small profit on the other. In a number of cases power supply is given at a low rate simply because they have a fairly good lighting load. I could name towns where rates have been quoted which in themselves are certain to result in a loss, and these rates

are only possible because they are given at the expense of other classes of consumers. Mr. Schofield.

Mr. H. DICKINSON: With reference to the table of costs per kilowatt, I think the figures which the author gives are rather low, and that £10 per kilowatt for the 60,000-k.w. station should be increased. There is generally difficulty in convincing users as to what their actual costs are. They are apt to neglect many of the items that should be included in the cost sheet. If it is possible to give as an example a user working under similar conditions, I have always found such information very convincing. With regard to the diversity factor, Mr. Woodhouse mentions on page 436 that there is an advantage in favour of a large scheme as only one-half of the plant is required. He seems to be of the opinion that the diversity factor only operates in large stations, whereas it operates in a small station, but not to the same extent. I do not think his conclusion, therefore, that the capital cost is reduced in the ratio of 4 to 1 is correct. With regard to the general question of being able to supply power users with very high load factors as cheaply as they can supply themselves, I hope Mr. Woodhouse is right, but I have my doubts. I think many people forget that the distribution system is very costly, and that a great deal of extra capacity of mains has to be provided over and above that actually required for the actual maximum load. Take a town like Leeds: we have to run to the city boundaries in about a dozen different directions. It is necessary to have a certain amount of spare cable which may take years to load up, as it is necessary to provide for a considerable time ahead when laying cable. It will be found that in most cases there will be probably twice, and sometimes three times, the capacity of cable laid over and above that required for the maximum load, and this should be borne in mind when considering what the distribution costs really are. I think that the distribution costs are a more serious item than some engineers imagine. I think the author makes too much of the view that the power company can supply cheaper than the private user can generate at. It is to be hoped that the author is correct, and in many instances he, no doubt, is correct. I think he does not, however, lay enough stress upon the advantages to be gained by users in taking a supply from an outside source. These advantages are very material, and I think when users appreciate what they mean they will be prepared to pay for them, and I am glad to say that I think they are gradually beginning to accept this principle. It is a great advantage in a large factory for the management to have no anxiety regarding boilers, engines, steam pipes, economisers, etc., and thus be enabled to devote its whole attention to the firm's particular manufacture. Mr. Dickinson.

There is also the considerable advantage that additional power can be put in the shape of a larger or additional motors without interfering with the factory as a whole. On page 436 the author mentions that experience shows that station demand will not exceed one-third of the total lighting connected to the system, nor one-half of the power installed by power users. I think it is the reverse way. I had some

Mr. Dickinson. figures got out regarding the Leeds undertaking, and find that only half the lighting installed and one-third of the power installed is on at one time.

Mr. Wilkinson. Mr. GEORGE WILKINSON: I note that Mr. Woodhouse puts his finger on the weak spot right away, namely, the unfortunate restrictive legislation which prejudicially affects the industry, and the marvel is that the British industry stands so well in the world to-day. If we must have a chance to get alongside Germany and America in our electrical business (and at present we are distinctly behind) we must certainly have more generous legislation. Mr. Woodhouse quotes the South Wales Act of 1900, and asks us to compare it with the Lancashire Act of 1901, implying that the one is satisfactory while the other is not. Later in the paper, however, he expresses entire dissatisfaction with the statutory conditions relating to such undertakings. Again, the author states the conclusions which were arrived at in 1902 by the Institution of Electrical Engineers, and refers to the present lamentable state of legislation. These separate statements, to my mind, do not tally, and perhaps he might amplify and clear up the matter in his reply. The first table we come to in the paper is one of cost of stations of different capacity. Unfortunately the portion referred to is simply the station equipment, and the much more serious problem for power companies, namely, the cost of the distributing mains, is omitted. I remember, a few years ago, drawing up a lighting scheme for Otley, and at the same time I approached the Yorkshire Power Company for a price at which they would supply the current, but the price quoted was such that it would have paid them better to build their own works rather than take a supply from the power company; I hope since then the rates have come down. There was this disadvantage in the case of Otley, that it was right on the edge of the supply area, and, of course, the cost of transmission was thereby increased. Mr. Woodhouse puts the cost for a 60,000-k.w. station at £10 per kilowatt, and £15 for 6,000, and £20 for 600. In my opinion it could not be done for that; it certainly would not be done for £20 per kilowatt for a 600-k.w. station unless they had a tin building. If it was a substantial building it would go up to something like £28. On page 435 we have figures with regard to steam consumption for generating plant. Here, I think, he has gone on to the other side. For instance, a 300-k.w. set is put down as consuming 24 lbs. of water per kilowatt. I do not know whether the figures include the steam losses and the steam used in the auxiliaries, but the figure is certainly heavy. I know of a turbine which is six years old, of 300-k.w. capacity, which is consuming 19¼ lbs. of steam per kilowatt on full load. He states that the consumption of coal is 3 lbs. per kilowatt, which means an evaporation of 8 lbs. of water to 1 lb. of coal. That, in my opinion, is too low, as it is easy now to get 9 lbs.; so that a third should come off that item, and, instead of 3, they could do it for 2 lbs., unless the losses are increased owing to the steam set being too large for its duty.

On page 435 he mentions 30 per cent. and 60 per cent. load factors.

I do not know of any stations at present that are enjoying such load factors, especially the 60 per cent. On page 436 he states that the diversity factor will approach 2 in a large scheme. In the case of Harrogate we have approached a diversity factor of 2, and that is largely due to the fact that we have a peak load in the summer. Our heaviest load is in September, so that we get better distribution throughout the whole year. There is one point he has not touched upon, and one that is worthy of consideration, namely, electric cooking. I have been giving some attention to this matter lately, and I find that the diversity factor for cooking is likely to come out at something like 4 instead of 2, and that, of course, has a very material bearing on the price at which electricity can be sold for cooking, because the capital cost is the chief determining factor. If electric cooking can show a diversity factor of anything like 4, one can afford to sell current at a price that will make the business financially successful. The gas companies have hitherto considered the business as a monopoly so far as cooking is concerned, but they are likely to be undeceived before very long with regard to this matter. On page 439 Mr. Woodhouse mentions further uses for electricity, notably the manufacture of calcium carbide. Mr. Wilkinson.

I would like to ask Mr. Woodhouse how he arrives at his load factor. I think he is to be congratulated on having a 45 per cent. load factor.

Mr. E. G. Love: As regards the privileges of supply, there is no doubt that if these were not so limited, considerable benefit would accrue to all concerned in the industry. With reference to the table of capital cost, I think the cost per kilowatt as mentioned is about the correct figure. Referring to Table III., the question of the cost of current depends very largely on the cost at which it is generated and distributed. This, to my mind, is where the power companies take advantage of the local authorities—they certainly can get much better load factors, and consequently can afford to supply at a cheaper rate. I must, however, take exception to the figures given for coal in this table as not being consistent with actual practice. The consumption would probably be about 30 lbs. of steam per kilowatt for a load factor of 20 per cent., and that is a fair average figure for a central station. With a load factor of 100 per cent., this would not be decreased much below 20 lbs. per kilowatt, so that the difference in the coal bill with the three different load factors would not be anything like as great as that stated. It is a question whether the power companies are not giving a supply at too low a price, but it is, perhaps, a bait to induce consumers to come on. I find that when they get to know the advantages of electric motive power they consider our charges quite reasonable. Quite recently we had an offer from a consumer to take 150 k.w. He had been using a suction gas plant, but wanted to take it out and so get rid of the bother and expense of breakdowns, etc. They would be quite satisfied with the supply at $\frac{7}{8}$d. per unit, and as far as we are concerned the load factor is sufficiently good to allow of us supplying at this price. When the users of motive power get to Mr. Love.

Mr. Love. know more thoroughly the advantages of electric driving we shall probably get a good many more consumers, including some of the large mills, at this figure or a very little below. It is not advisable to go below ½d. per unit, and it is a question whether we really can expect to get any return at all at any thing under this figure, and we should go very warily before we offer it at even that price.

Mr. Churton. Mr. T. HARDING CHURTON: Mr. Woodhouse has made, I think, a bold proposition in suggesting that the collieries in the West Riding should form, so to speak, a joint stock concern of their electrical plant, but not only do the difficulties in the way of adopting such a proposal seem to be great, but the advantage appears questionable. The suggestion seems to be this, that a colliery owner, instead of installing a stand-by plant, is to be connected up to his neighbour's plant, upon which he can draw in the event of a breakdown to his own machinery. The difficulties in the way of managing such an arrangement appear considerable, but it also seems to me that in most cases, at any rate, the cost of the cable would be a great deal more than that of a stand-by plant. And the cost of management at the colliery would not generally be reduced, as one man only is usually employed to mind the plant, whether there be two or three or more sets installed. Further, as Mr. Woodhouse will probably have found to be the case, most colliery owners appreciate the advantage of autonomy, and of having control of their electrical supply, and they would not, in general, view favourably a proposal to be dependent upon a long length of cable or upon main supply fuses, to which they would probably not have access, and which might, of course, be blown by momentary excess current. Generating costs at collieries, particularly those at which the waste heat from coke ovens is utilised, are usually very low, and I really cannot see where a colliery with its own generating plant can obtain any particular advantage from the proposed combination.

The Chairman. The CHAIRMAN (Professor G. D. A. Parr): Before asking Mr. Woodhouse to reply to the various queries raised in the interesting discussion which we have had, there is one point to which I should like to refer. Mr. Woodhouse states that the cost of supply would be considerably reduced by combining the main station with a number of small stations in the surrounding districts which have waste fuel at their disposal. I am unable to understand where the economy will come in in such a case except under special conditions. If the peak of the load is to be taken in this way, machinery will have to be installed somewhere to take the peak, and this machinery may be either in the central station or consist of a number of detached units; it may, of course, happen that the diminished running cost may balance the interest and depreciation on the various units which are spread over a large area and are intended to take the peak of the load, but it seems to me that a large increase of costs will occur in connection with the mains, because in all probability the cheap fuel providers will be in some outlandish districts which will require considerable lengths of main to reach them. Therefore it seems to me that the saving in com-

bining such individuals and the central power company is likely to be more imaginary than real. This was partly covered by what Mr. Churton said, only I took his remark to mean that the advantage was not very real on the side of the cheap fuel supplier. There may be some advantage to the supply company, but I think the increased interest and depreciation would probably not allow the diminished cost of running to outweigh it. The Chairman.

Mr. F. M. Moody (*communicated*): Mr. Woodhouse has made out a good case for the power company against the private plant, but one which scarcely convinces. Taking the points in order. Capital cost per kilowatt: When comparing the two the cost per kilowatt of the power company must include all capital expended in supplying current to the consumers' switchboard, and if this is done the total cost per kilowatt will be nearer £20 per kilowatt than £10 per kilowatt. The cost of private plant is put too high, recent figures obtained for a 200-k.w. plant working out at £14 per kilowatt. Mr. Moody.

The use of gas and oil engines in these smaller units is an advantage open to the private plant, and must be taken into consideration.

In respect of the load and diversity factor, the works plant is in a unique position, inasmuch as the plant installed can be suited exactly to the load, which is ascertained before the plant is erected. The load factor in a works is always pretty high, and in some cases approaches unity when the running hours only are considered. The diversity will not work out so high on the class of load such as is found in mills, collieries, etc., where work generally commences at 6 a.m. and stops at 6 in the evening. When the lighting hours such as are experienced in winter occur, the peak will at such times be considerably higher. Thus in such a power scheme the peak loads would occur from 6 a.m. to 9 a.m., then a steady load up to 3 p.m., when the load would rise to a maximum up to 6 p.m., after which the load would fall very rapidly.

The costs in respect of wages and management will not show a big reduction in any case.

In a small plant the necessity of stand-by plant is not apparent; no mill-owner dreams of duplicating his steam plant, and for that matter no electrical engineer would favour duplicating the motors installed. That being so (and after several years' experience in running plant the experience is that breakdowns are mostly due to the steam plant, and those of very little moment), the necessity of duplicating plant is not apparent.

Considering the small works plant, which has no complicated system of mains, and has a high load factor, and can instal the one-unit type of plant, such a unit consisting of boiler, engine, and generator, with the shortest steam main possible, I fail to see how the larger station can supply current as cheaply as it can be generated on the works. A point in favour of power company supply is the unlimited power at the command of the consumer, which is important in many cases. The supply of steam power from coke ovens as mentioned by

Mr. Moody. the author would be all right for the colliery owners, but in connection with a power company would scarcely work out well, if we take the case of combined destructors with power plant as similar cases. Such steam power would be charged to the power company, and it must be borne in mind that free power such as this (as has been proved in the case of some water schemes) often costs more in the end at the consumer's meter. If current can be supplied from the power companies at 0·5d. per unit the small private plants would soon, from an economical point of view, cease to justify their existence; but such a time is scarcely with us yet, whatever may be in the future.

Mr. Woodhouse. Mr. W. B. WOODHOUSE (*in reply*): I must thank the members very much for the interesting discussion on my paper. Mr. Moody, like a number of other speakers, questioned the capital cost of the stations as given in the paper, and remarked that the expenditure of £20 per kilowatt for a 600-k.w. station was too high; some of the other speakers thought the figure was too low. It is very difficult to make out a table of costs to meet everybody's views. I gave £10, £15, and £20 as representing something like the real figures, and it would be easy to prove that for the particular type of plant I had in mind it would be sufficient. Mr. Love referred to gas plant as being an advantage on the side of a small station, but I do not know that there is any reason why the large power station should not use gas plant should it be thought desirable. Any argument that applies to the use of gas engines for one station seems to apply equally well to the others. The capital cost per kilowatt comes down in much the same proportion, but I do not think the fuel economy goes up to the same extent. The conditions under which coke ovens and refuse destructors work cannot be compared. I was glad to hear that Mr. Schofield was generally in agreement with what I said, and also most of the other speakers. One point that Mr. Schofield raised, namely, the addition of management and other charges outside water and coal, wages, etc., to power costs is, of course, a very difficult one to get any private user to admit, and, in fact, he never will admit that his power costs him as much as it actually does. Such points make it very difficult to get the large power user on to the public supply station, unless there are some special conditions such as the power user having a worse load factor than the station. Mr. Wilkinson referred to the comparison between the South Wales Act of 1900 and the Lancashire Act of 1901. I am sorry I did not print this. The South Wales Act of 1900 contains a clause which was recommended by Lord Airedale's Committee and drafted by the Board of Trade. It permits the power company to supply generally power and light in all districts within its area, but the local authorities have a reasonable veto. If they can show that they are in a position and willing to give a supply on the same terms, they can then forbid the power company going into their area, but if they are not prepared to do that, then the power company may come in in competition. With regard to the Lancashire Act of 1901. This practically gave every local authority an absolute veto. It has, however, since been

modified. Some power Acts have never been put into force, simply because of the protective clauses obtained by local authorities. It has just been waste of money to get the Acts, for they have been protected out of existence. Mr. Wilkinson referred to works costs. I did not want to take extreme figures, but the steam-consumption figures are test figures, and the coal figures ought, I think, to be got on test with almost any respectable coal. In working with a 15 per cent. load factor a public supply station includes running over long periods where the working is necessarily inefficient—from Saturday midday to Saturday evening there is no power needed, and from Saturday night to Monday morning there is practically no load. Mr. Wilkinson also mentioned that 30 and 60 per cent. load factors are not got. Take the case of the Yorkshire Power Company—their present load factor is 45 per cent. The Newcastle Company's load factor, I imagine, is something like 60 per cent. or very near it. We are expecting to get a load factor approaching 80 per cent., so that it will be realised that it is not difficult to get the 60 per cent. mentioned in the paper. I noticed recently in the electrical papers a description of the electric cab-charging stations in London where they are getting power very cheaply—they are taking power during hours when the ordinary load is low, and I think you will find that in time a considerable number of new users of similar nature will arise. Mr. Dickinson questions the cost per kilowatt, and emphasises the difficulty of convincing consumers as to just what their costs really are. The advantages of a public supply are so great as to balance works cost in a great many cases. At the same time, the power users do not come on as rapidly as they ought to, and I think there is no doubt that we shall not get the full benefit until we have got a very big proportion of power users taking the public supply; we shall have to get the first 50 per cent. on to the mains without utilising advantages he mentions in getting a higher price, and the other 50 per cent. will no doubt follow in due course. I was interested in the Leeds ratio between horse-power of demand and horse-power connected mentioned by Mr. Dickinson. At Thornhill our figure for power is 2. When I was at Newcastle it was 2½; this is accounted for by the more variable load in the shipyards, etc. The lighting figure of 3 I took out of the *Electrical Times*, and some other figures which I had by me at the time, but generally, I think, a figure of something between 2 and 3 is reliable. The main point is that we can by centralisation increase our diversity factor and reduce our station capital charges, and therefore have more money to spend on distribution than in a small station. With regard to the question raised as to the coal cost, I have worked this out carefully and the figures seem about right. I know a great many stations where the coal consumption at night-time per unit is nearly twice the coal consumption in the day-time, and it was not worth while putting a small unit in for the light load. Mr. Churton rather questioned the possibility of getting the collieries to co-operate, but I do not think he is right. I have talked with a number

Mr. Woodhouse.

Mr. Woodhouse. of colliery owners, and I find an increasing tendency towards co-operation. Of course the ideal thing from the power-station point of view is not to have to put a lot of little stations all over the country, but to get the colliery people to co-operate and make their coke at one central station.

Professor Parr and another speaker raised the point as to whether the advantage of cheap waste heat in these small stations would be outweighed by the cost of mains. I think if one considers the actual case where the load is distributed, as in most power districts, it is not quite so bad as it looks. Each colliery is a power user, and in between each colliery there may be a dozen or more power users, and consequently a fairly cheap system of mains will link up these small stations. The whole thing is designed for simplicity and security, and breakdowns are very remote. I think it is a feasible proposition, and I hope that some of these days we shall have it in operation. With regard to the co-operation of collieries I may remind Mr. Churton that in Durham a joint scheme is in operation by the Durham Power Company.

Replying to Mr. Wilkinson, my definition of load factor is the ratio of units generated to the units that would be generated if the plant was running at the maximum load; not the maximum kilowatts in the station, but the maximum load recorded in the period.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

Vol. 40. 1908. No. 189.

Proceedings of the Four Hundred and Sixty-eighth Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, January 23, 1908—Colonel R. E. Crompton, C.B., President, in the chair.

The minutes of the Ordinary General Meeting held on January 19, 1908, were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council:—

TRANSFERS.

From the class of Associate Members to that of Members:—

Geo. Broughall.
Ernest E. Eccles.
Osbert F. Francis.
Thomas Mather, F.R.S.

From the class of Students to that of Associate Members:—

Frederic Bacon.
Gomer B. Davies.
Bernhard P. F. Deane.
John S. Dow.
James Gray.
Fred W. Halford.
Alfred R. Harris.
Alex W. Harrold.
William Howes.
William Marden.
Leonard Murphy.

Donations to the *Library* were announced as having been received since the last meeting from Messrs. A. Constable & Co., Ltd.; to the *Building Fund* from W. McGeoch, jun.; and to the *Benevolent Fund* from Dr. E. Hopkinson, L. Miller, and T. S. Watney.

The following paper was read and discussed :—

STANDARD PERFORMANCES OF ELECTRICAL MACHINERY.

By Dr. RUDOLPH GOLDSCHMIDT, Associate Member.

(*Paper received October* 3, 1907, *and read in London, January* 23, 1908.)

Modern engineering is divided into so many branches, all so complex, that the specialist in any one of them can hardly be expected to be intimately acquainted with the qualities of machines built by specialists in the other branches. In such cases he looks for advice to the Standards Committees of our great engineering societies, who have not only aimed at standardising manufacture, but have also drawn up rules for the benefit of the user of machinery.

Little information of a definite nature as to standard performances of motors and dynamos is, however, available, probably because it is very difficult to obtain agreement between all the parties concerned. For instance, a purchaser of a 10-H.P. motor will look in vain for information as to what the efficiency of such a machine ought to be. If there were any standard rule prescribing that the efficiency should be 85 per cent., manufacturers would probably endeavour to obtain 85½ per cent., and it might become a point of honour to exceed the conditions of the rules. Suggestions for standard performances, standard power-factors, standard efficiencies, etc., from a private quarter may, however, be acceptable, and, after criticism and discussion, may become very useful as a private standard.*

Although they might not be quite indisputable, such normal figures would be of assistance in many respects. The mechanical engineer cannot be expected to know what will happen if, say, in a machine tool or crane a gear is arranged to be cut out and the motor speed reduced. The effect is entirely different with 2-phase, 3-phase, single-phase, or direct-current machines, depending on the periodicity and other considerations, and it therefore requires a good deal of special knowledge to decide upon the most suitable type of machine to employ. I will mention only one of the many cases I have come across. A 5-H.P. 2-phase induction motor for a crane had been fixed at as low a speed as 500 revs. per minute, the overload capacity had been specified as 150 per cent., and the frequency was 50 ∿. It does not appear at first sight that the current consumption of such a motor is about double that of a machine running 250 revolutions faster, or that a

* Some general data can be found in Hobart, "Electric Motors," pp. 70 and 297.

somewhat smaller overload capacity would have improved matters. The mechanical parts of the crane had been finished, and the motor had to be built contrary to the better judgment of "all who knew." In such a case a timely warning would have been of value.

It is needless to say that approximate standard performances, if fairly complete, can be used for the practical and perhaps also for the theoretical comparison of different classes of machines and of different supply systems.

For these reasons I was induced to write this paper, an additional encouragement being the well-known fact that the performances of different makes of machines do not differ very much in actual practice, so that there must exist something like a "standard performance."

In compiling the paper I have avoided as far as possible dealing with such generalities as are to be found in text-books, and have endeavoured to demonstrate the results by means of curves, restricting myself to comments on these only. By the exclusion of all questions of design as well the compass of the paper has been considerably reduced.

All electric motors and dynamos work through the interaction of the magnetic flux and the ampere-turns. Whatever machine one may select, whether it be an induction motor, an alternator, a synchronous motor, or a direct-current machine, one always finds a cylinder carrying ampere-turns on its circumference and a magnetic flux entering this cylinder in a radial direction. The torque is the product of both; or, taking—

$$\text{Torque} = \text{const.} \times \frac{\text{K W}}{\text{Revs.}}$$

and calling "power" the kilowatts reduced to 1,000 revs. per minute, then—

$$\text{"Power"} = \text{K W at 1,000 revs.} = \text{const.} \times \text{ampere-wires} \times \text{flux}$$
$$= \text{const.} \times \text{A W} \times \text{F}.$$

With direct-current machines, polyphase alternators, and polyphase induction motors, the constant averages $0{\cdot}165 \times 10^{-9}$; with single-phase machines it averages $0{\cdot}145 \times 10^{-9}$.

By "ampere-wires" is understood the total number of conductors which are cut by a plane in the middle of the armature at right angles to the shaft, multiplied by the number of amperes per conductor.

With induction motors the ampere-wires are those of the rotor only. The flux is the sum of all fluxes entering the armature from all poles together.

It is naturally possible to obtain a certain "power" with an infinite number of combinations of ampere-wires and fluxes. In actual practice the figure for both will be of the same order if we express A W in 10^3 (kiloamperes) as unit, F in 10^6 (megalines; 1 megaline = 167 kapp lines).

The conditions fixing A W and F vary for every class of machine, and are not always those of economy of design. With direct-current machines the commutation, with alternating motors the pull-out, with alternators the voltage drop, come into consideration. Of the two factors, A W and F, working together to produce the output, one, F, is constant, while A W increases with the load. A W tends to destroy F, and causes sparking or voltage drop. Therefore, in order to produce a good machine F must not be too weak. It is due to these well-known conditions that F and A W do not actually vary so very much. The effect on the efficiency, which is the chief item about to be considered, is very small indeed, even if F varies by quite a considerable amount, as will presently be seen.

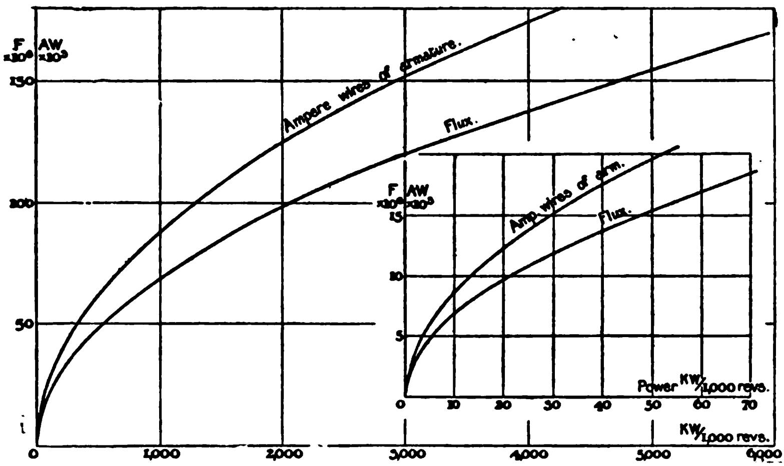


FIG. 1.—Direct Current. Standard Fluxes and Ampere-wires of Armature.

There are certain sections required for F and A W, as the density must not exceed a certain figure, limited by heating or saturation (teeth). Further, the length of path which the flux and A W have to flow in is also very nearly fixed, as both are interlinked, and have to surround one another. Therefore, if the A W and the size are given, the copper losses due to A W are pretty well fixed, as well as the core loss, provided the speed at which the flux revolves is known. Figures in detail on this point for direct-current machines are given below.

DIRECT-CURRENT MACHINES.

Fig. 1 shows F and A W for different K W at 1,000 revs., and the figures may be considered normal for machines having no commutating poles. In most cases these fluxes are identical with the most economical flux, especially with moderate speeds. With commutating poles the flux may be 25 per cent. or even 35 per cent. less. There is also

some variation, dependent on whether the machine is very narrow or excessively broad, on the number of poles, etc. We will disregard this for the present.

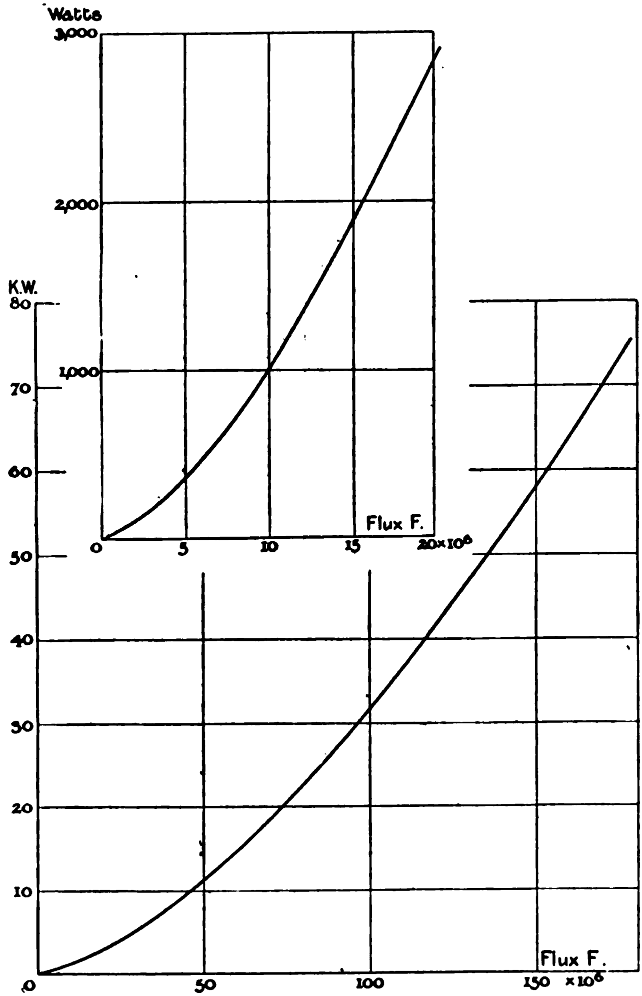


FIG. 2.—Direct Current. Core Loss at 1,000 Revs. per Minute with different Fluxes.

The core loss caused by a certain flux, F, is proportional to the speed. Taking 1,000 revs. per minute as the unit of speed, Fig. 2 shows the core loss at different fluxes. If it is desired to know what the core loss of a 500-k.w. machine is likely to be, the speed being 250 revs. per

minute, we take "power" $= 500 \times \frac{1{,}000}{250} = 2{,}000$ k.w. at 1,000 revs., and take from Fig. 1 flux F = 99 megalines; Fig. 2 shows that the core loss $p_{fe} = 31{,}000$ watts with F = 99 at 1,000 revs.; that is, at 250 revs.

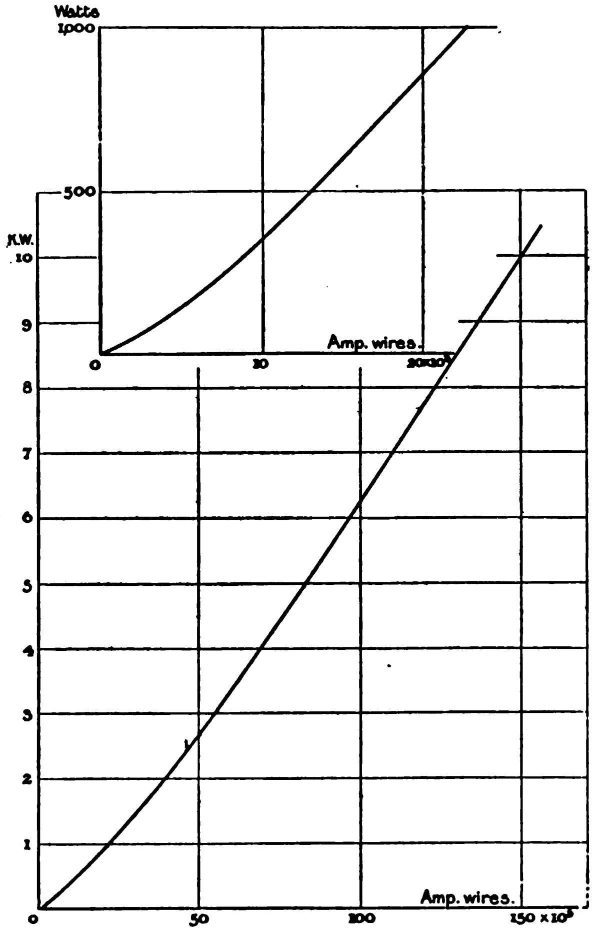


FIG. 3.—Direct Current. Copper Loss of Armature with different Ampere-wires.

$\frac{250}{1{,}000} \times 31{,}000 = 7{,}750$ watts, or $\frac{7{,}750}{500{,}000} = 1{\cdot}55$ pér cent. It must be understood that, if $p_{fe} =$ const. $\times$ revs., this is not correct for one and the same machine. There are eddy-current losses, which increase as the square of the speed, the hysteresis only being proportional to it.

But in our case the machine with more flux is a different machine, more iron section being provided. This is the reason why we are entitled to make the core loss practically proportional to the speed.

The armature copper loss is dependent on A W only, as shown by Fig. 3. In the case of our example, we take from Fig. 1 for 2,000 k.w.

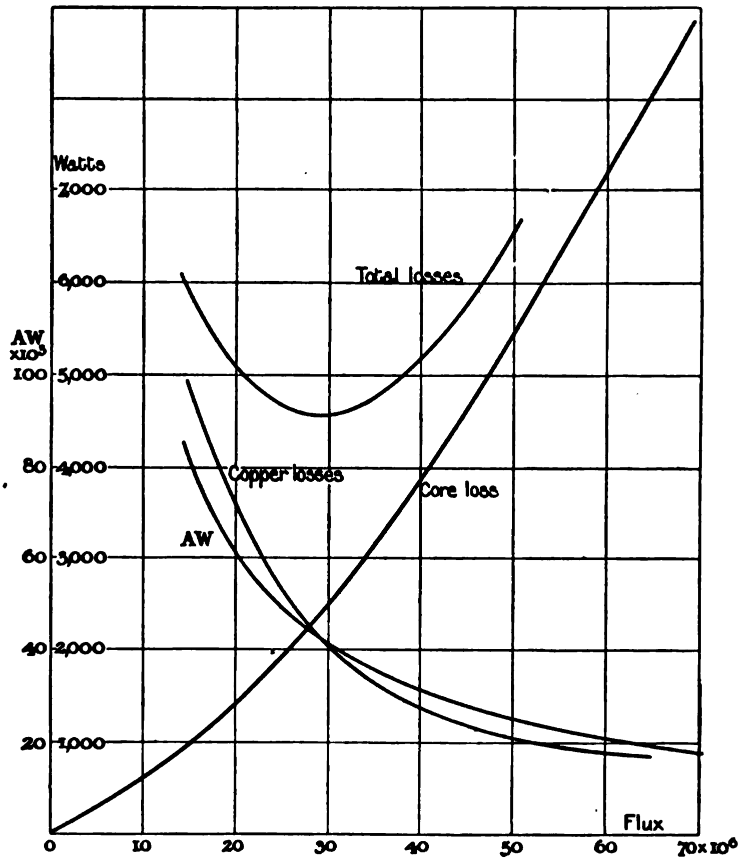


FIG. 4.—Direct-current Generator. 100 k.w., 500 Revs. per Minute, Losses in Armature with different Fluxes and A W.

at 1,000, A W = 125 kiloamperes, and from Fig. 3, 8,100 watts copper loss, $p_{cu} = \frac{8,100}{500,000} = 1{\cdot}62$ per cent.

Copper and iron loss together amount to $1{\cdot}62 + 1{\cdot}55 = 3{\cdot}17$ per cent.

Picking out the same figures for a 100-k.w. machine, running at

500 revs., we find, from Fig. 1, F = 31, A W = 40, and from Figs. 2 and 3—

Core loss = 2,650 watts;
Copper loss = 2,000 watts;
Total, 4,650 watts = 4·65 per cent.

If F were reduced by 20 per cent., or to 25 megalines, A W would rise to 50 kiloamperes, and—

Core loss = 1,950 watts;
Copper loss = 2,650 watts;
Total, 4,600 watts = 4·6 per cent.

The total losses, therefore, have not changed. In Fig. 4 the A W, copper, core, and total losses have been drawn up for 100-k.w. 500-rev.

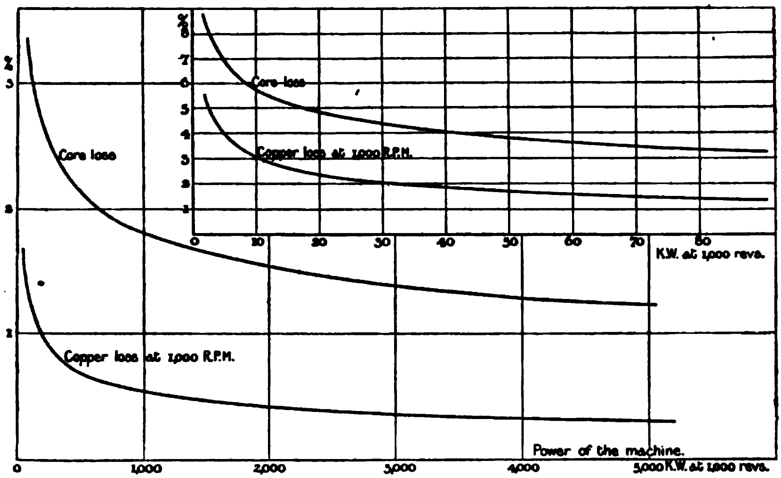


FIG. 5.—Direct Current. Armature Core and Copper Losses in Percent. of Output.

machines designed with different fluxes, and it will be noticed that the total losses are practically unchanged (that is, only between 4,550 and 5,000 watts) if the flux is altered from 21 to 37 megalines. The losses with well-designed machines cannot be far from this minimum, as it is very expensive to provide surface for extra losses.

For convenience' sake I have combined Figs. 1, 2, 3 to form Fig. 5, where the losses are expressed in percentages of the output, and as a function of the "power" K W at 1,000 revs. By expressing them as a percentage, p_{fe} is now independent of the speed, whilst p_{cu} is inversely proportional to it. As unit of speed we take again 1,000 revs. per minute.

For a 100-k.w. machine at 500 revs.: Power P = 200, core loss 2·65 per cent., copper loss $1 \times \frac{1,000}{500} = 2$ per cent. of 100 k.w.

The ampere-turns of the field magnets are a function of armature ampere-turns and flux, the former demagnetising and the latter requiring to be driven through the air-gap. These ampere-turns have to surround the flux coming from the armature, which fixes their mean path, so that there exists a law which connects the power of a machine with the losses in the field.

I will define the expression "field ampere-wires" as the field ampere-turns per pole multiplied by the number of poles and multiplied by 2.

The field A W and the losses as a function of the power are shown in Fig. 6. The losses are again expressed in percentages of the output, and, for correction, require to be varied in inverse ratio to the speed, 1,000 revs. per minute being again taken as normal.

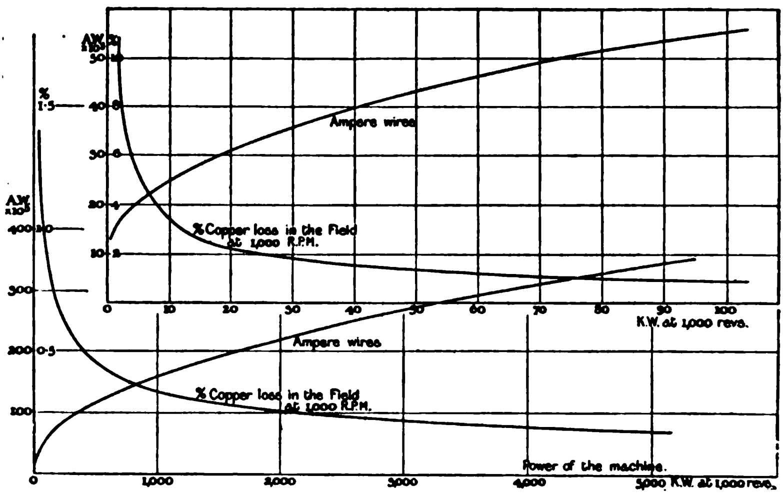


FIG. 6.—Direct Current. Field Copper Loss as Percent. of Output.

It may be noted that with large machines the field A W is about 25 per cent. more than the armature A W, and that 1,000 A W is about the lowest figure found with very small bi-polar machines. A 100-k.w. machine, running at 500 revs. per minute, would have (Fig. 6) $0{\cdot}67 \times \frac{1{,}000}{500} = 1{\cdot}33$ per cent. losses in the field magnets. In machines with commutating poles the losses in the main poles are reduced, but this reduction is made up for by the losses in the commutating poles.

It remains to consider the friction losses.

The most difficult question is that of the brush friction, but here also there exists a definite relation between current, current density in the brushes, and voltage drop, coefficient of friction, and friction losses.

Also the commutator diameter can be approximately determined from the output; so that, choosing 200 volts and 1,000 revs. as normal, a curve can be plotted (Fig. 7) giving the percentage brush-friction as a function of K W at 1,000. These figures vary inversely as the voltage and directly as the speed.

Example: 100 k.w., 500 revs., 500-volt dynamo. Power, 200 k.w. at 1,000. From Fig. 7: 2·1 per cent. $\times \frac{500}{1,000} \times \frac{200}{500} = 1 \cdot 7$ per cent.

The bearing friction and windage are practically independent of the "power," and depend almost solely on the angular velocity—that is,

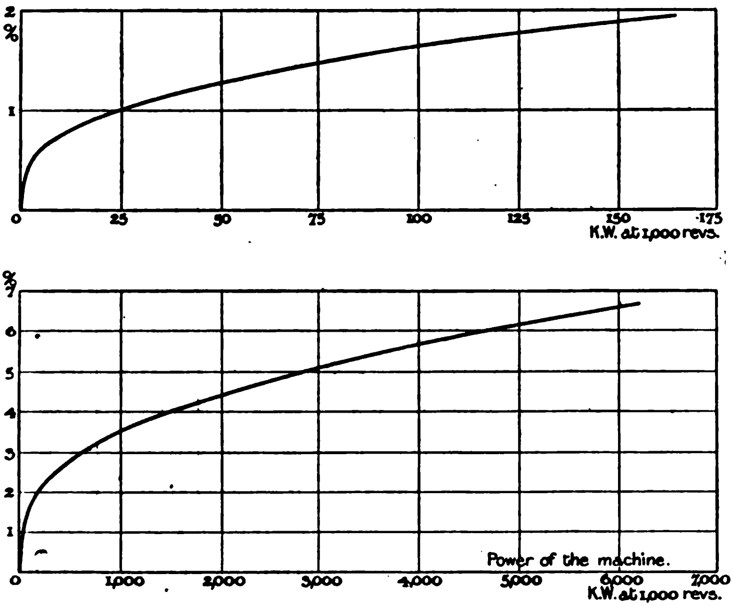


FIG. 7.—Brush Friction Loss at 1,000 Revs. and 200 Volts.

the revs. per minute. Fig. 8 gives practical average figures for this class of friction.

The voltage drop in the brushes expressed as a percentage may be taken as $\frac{200}{\text{volts.}}$ It is somewhat less with high-current and somewhat more with high-voltage machines. With copper brushes it may drop to about $\frac{100}{\text{volts}}$, but ought not to be assumed to be lower. For instance, a 500-volt machine would have a loss of $\frac{200}{500} = 0 \cdot 4$ per cent., due to voltage drop in the brush contact.

The Figs. 5, 6, 7, 8 enable the efficiency of any direct-current

machine to be estimated. Referring again to the example of a 500-volt, 100-k.w., 500-rev. machine, we find—

Core loss	2·65	per cent.	
Armature copper loss	2·00	„	
Field copper loss	1·35	„	
Brush friction	0·45	„	
Windage and bearing friction	0·75	„	
Brush drop	0·40	„	
Total	7·60	„	of output.
Output	100·00	„	
Input	107·60	„	
Efficiency	92·90	„	

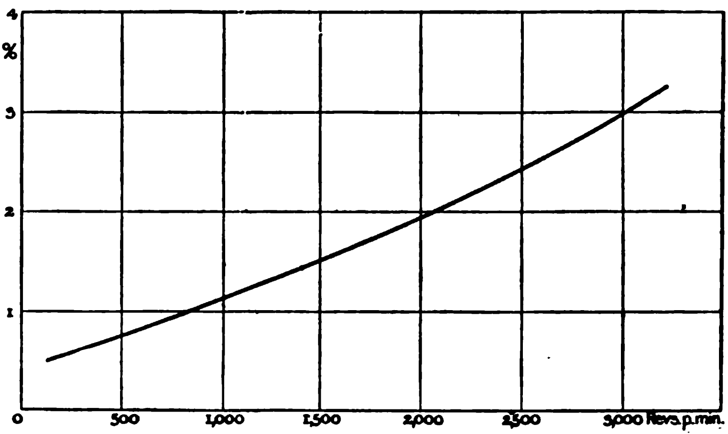


FIG. 8.—Bearing Friction and Windage.

To avoid going into details, the total efficiency has been plotted in Figs. 9 and 10 for different speeds and as a function of the actual output (not "power" in this case) of the machine, Fig. 9 giving the efficiencies of dynamos up to 5,000 k.w. In Fig. 10 the abscissæ represent brake horse-power, but naturally motor and dynamo efficiencies are practically identical, so that Fig. 10 can also be used for dynamos. The voltage has been assumed to be 500 volts. If the voltage is different, a correction may be carried out in the brush friction and brush drop. The comparatively low efficiencies at very high speeds are due chiefly to friction and core loss.

Figs. 9 and 10 give only the full load efficiency. An idea of the performance with smaller loads can be gained by referring to the curves giving the losses in detail.

Rotary Converters.

The efficiency of a rotary converter may be obtained from the efficiency of the corresponding direct-current machine, making allowance for the losses on the alternating-current slip rings. For this purpose the curves in Fig. 7 may be used, reducing the friction found in this way to about one-half. This is naturally only very approximate. The output of a direct-current machine can be increased by a certain percentage, if used as a rotary, with 3-phase current by 33 per cent., and with 6-phase by 90 per cent. As the power-factor is not always unity, and a slight deviation from this reduces the output again, it cannot be assumed that the latter is increased by the full amount, but

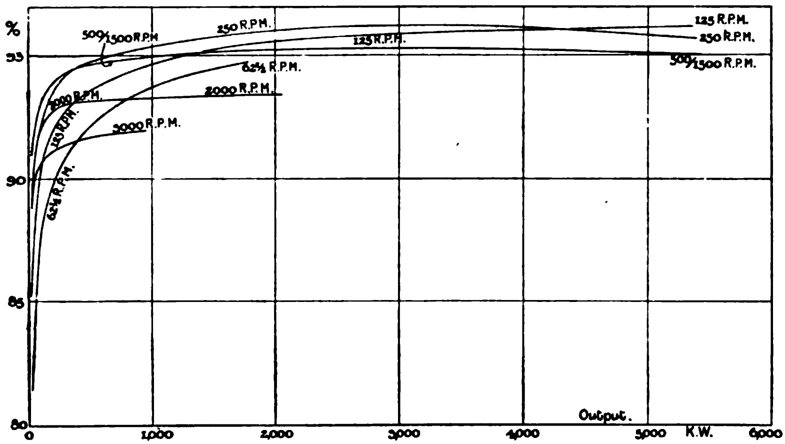


Fig. 9.—Efficiencies of Direct-current Machines, 500 Volts.

only, say, by 20 and 40 per cent. respectively. For approximate estimates the following formulæ may be used :—

$$g_r = \frac{g}{0{\cdot}93 + 0{\cdot}07 \times g} \text{ for 3-phase, and } g_r = \frac{g}{0{\cdot}81 + 0{\cdot}19 \times g} \text{ for 6-phase,}$$

where g_r is the rotary efficiency and g the efficiency of a direct-current machine which is $\left(\frac{1}{1{\cdot}20} - 1\right) = 17$ per cent. and 28 per cent. smaller; g is to be taken from Figs. 9 and 10. Thus a 500-k.w. direct-current machine at 250 revs. would as a 3-phase rotary have an efficiency of $\frac{94{\cdot}7}{0{\cdot}93 + 0{\cdot}07 \times 0{\cdot}947} = 95$ per cent.

Alternators.

The task of developing standard efficiencies for alternators is a very difficult one, as the law fixing the size of the machine is very com-

plicated if the number of poles and the diameter are small. We can speak of the "output" of the field and the "output" of the armature separately. Our output formula K W at 1,000 = 0·165 × A W × F refers to the armature only. Now, if we wish to make a good machine with not too much voltage drop, the A W of the field must be a multiple of the A W of the armature, say, three or four times as much. The flux in the field exceeds that in the armature by the amount of the leakage flux. Consequently the "output" of the field must be considerably more than that of the armature. As the field in the case of modern alternators is generally inside the armature, its mean diameter is smaller than that of the latter. Consequently it is often a difficult matter to provide room for sufficient A W and F, and the output of the whole machine has to be reduced. I should say that with machines

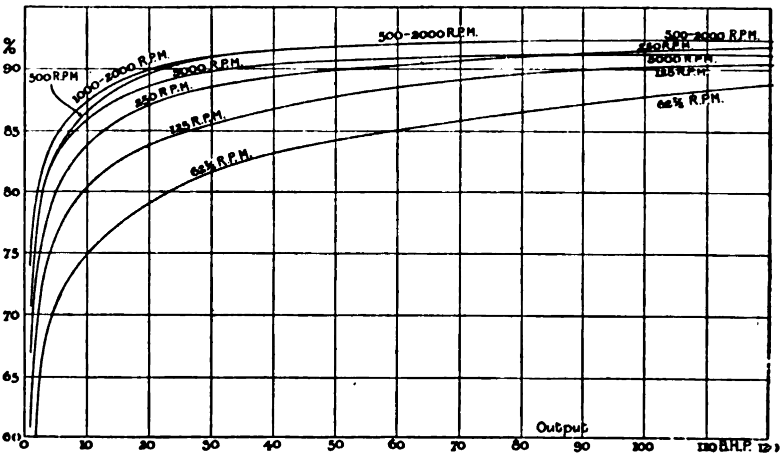


FIG. 10.—Efficiency of Direct-current Machines, 500 Volts.

below 5–6 ft. in diameter and a moderate number of poles this difficulty is always experienced. In such cases the output of a certain size of machine may be considerably increased by the admission of a higher voltage drop or by compounding.

On account of these difficulties I decided not to reproduce here the curves for the detail losses of alternators, as they would have been too numerous, but to give the results only.

Figs. 11 and 12 show the efficiencies of medium-volt polyphase alternators for 50 and 25 ∿ respectively, the power factor being unity.

The falling off of the efficiency with very high speeds due to the friction and core loss will also be noticed here. In comparing alternating-current with direct-current machines, large alternators are found to be slightly more efficient than large direct-current machines, on account of the commutator losses of the latter, but the reverse is the case with

small machines, where only small currents have to be dealt with on the commutators and where the alternators require considerable excitation.

To find the efficiency of single-phase alternators it must be remem-

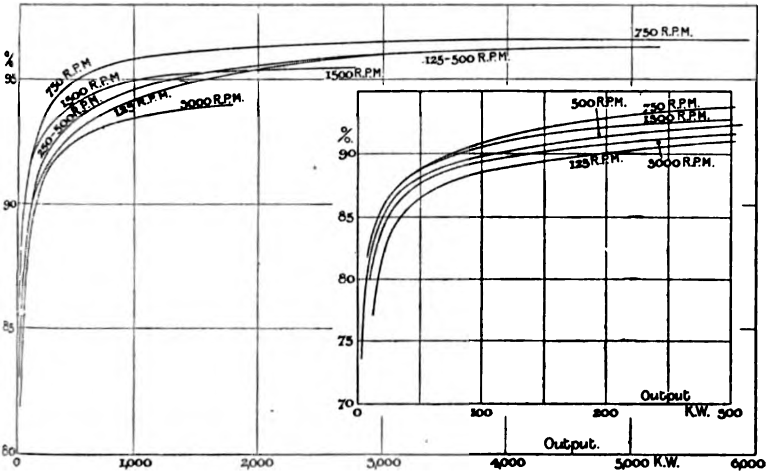


FIG. 11.—2- and 3-phase Alternator Efficiencies, 50 ∿.

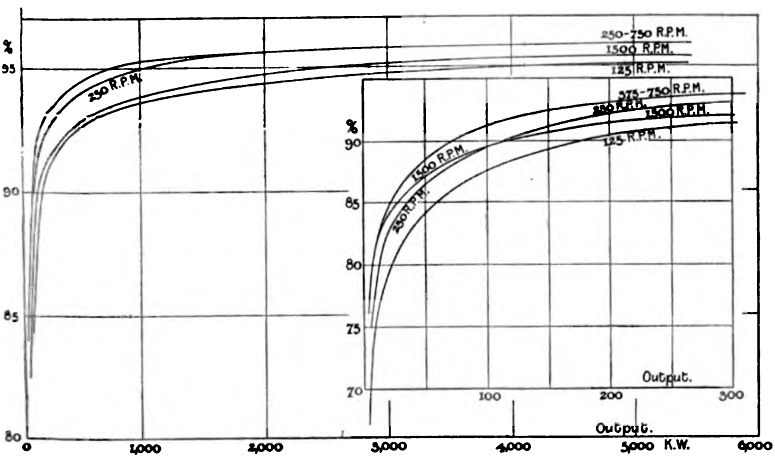


FIG. 12.—2- and 3-phase Alternator Efficiencies, 25 ∿.

bered that the output of a certain machine as a polyphase alternator is about 50 per cent. more than as a single-phase one. The losses of the latter may therefore be taken as those of a 50 per cent. larger polyphase machine. If we wish to know the efficiency of a 50-∿,

1,000-k.w. alternator, running at 250 revs., we pick out from Fig. 11 the efficiency for $1.5 \times 1{,}000 = 1{,}500$ k.w., polyphase, 250 revs. $= 95$ per cent. The losses are $\frac{1}{0.95} - 1 = 5.2$ per cent. of $1{,}500$ k.w. $= 1.5 \times 5.2 = 7.8$ per cent. of 1,000 k.w. Therefore, as 1,000 k.w. single-phase alternator the machine has $\frac{1}{1.078} = 92.7$ per cent. efficiency.

Generally speaking, if g is the efficiency of the 50 per cent. larger polyphase alternator, the efficiency of the single-phase machine is—

$$g_1 = \frac{g}{1.5 - \frac{g}{2}}.$$

If the power-factor is 0·85, the output of a certain machine is re-

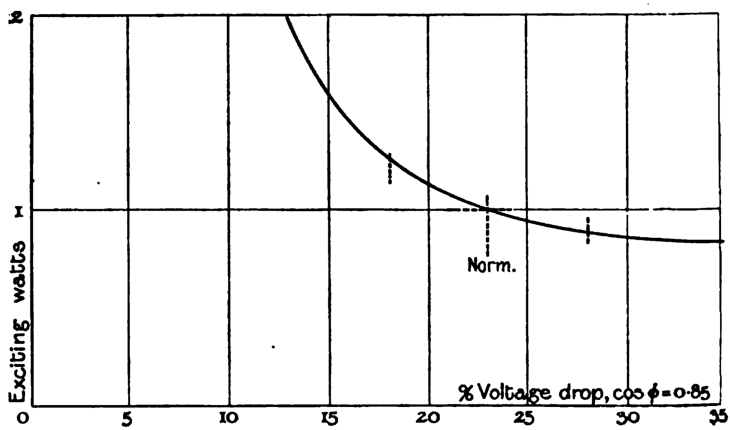


FIG. 13.—Voltage Drop of Alternators.

duced 15 per cent. (actually the output of the armature only), while the excitation losses rise. It may therefore be stated approximately as—

$$g_{0.85} = \frac{g}{1.3 - 0.3 \times g}.$$

I have pointed out what great influence on the size of machine the permissible voltage drop has. Therefore it is advisable not to specify it higher than can be helped. As a rule one finds about $7\frac{1}{2}$ per cent. drop specified with a power factor of 1, and 23 per cent. with a power factor of 0·85. Calling the excitation watts with this drop (23 per cent.) 1, and plotting a curve "exciting watts as function of the drop," the curve Fig. 13 is obtained. I have marked 23 per cent. as normal. If 5 per cent. less drop is required—that is, 18 per cent.—the exciting watts would go up 25 per cent., whilst an increase of 5 per cent. to 28 per cent. drop only alters the watts 12 per cent. A voltage drop of 23 per cent. therefore appears to be very well chosen,

Induction Motors.

The Efficiency.—The design of induction motors is far more definite than that of alternators, so that I may venture to reproduce a curve for the normal flux as a function of the "power" H.P. at 1,000 in Fig. 14. The overload capacity is the chief determining factor for these fluxes. In our case they refer to machines for about 125 per cent. overload capacity. With large and high-speed machines this figure is somewhat higher, owing to the fact that considerations of economy become important when A W and F are chosen. The percentage core loss and copper loss for different powers are shown in Figs. 15 and 16.

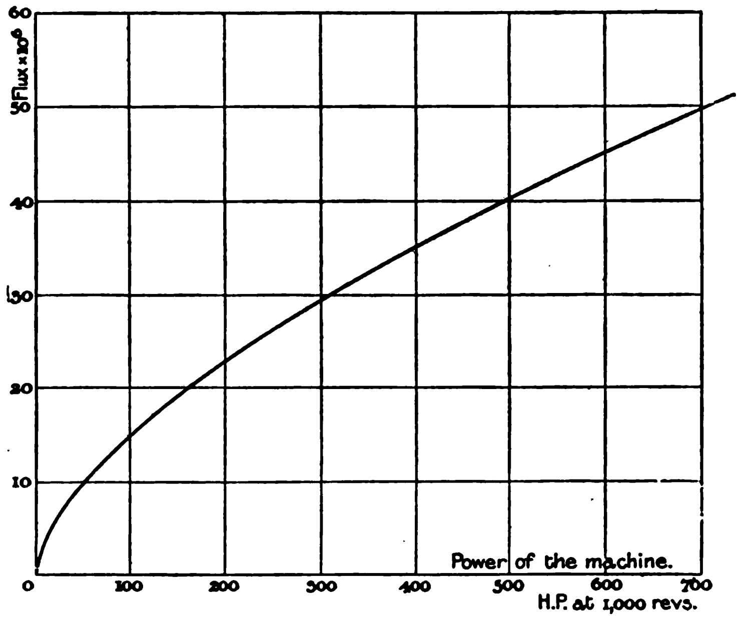


Fig. 14.—Induction Motors. Fluxes.

In induction motors the percentage core loss is also independent of the speed, and of the periodicity, if plotted separately for different numbers of poles. Three curves have been drawn: for 2 poles, 16 poles, and 48 poles. For any intermediate number the method of interpolation may be used. For instance, if it is desired to find the percentage core loss of a 50-∿ 500-H.P. 500-rev. (synchronous) motor, we have—

$$\text{Power} = 500 \times \frac{1{,}000}{500} = 1{,}000 \text{ H.P. at } 1{,}000$$

$$\text{Number of poles} = 120 \times \frac{50}{500} = 12.$$

From Fig. 15: Core loss with 2 poles, 1·4 per cent.; with 4 poles, 2·1 per cent.; consequently with 12 poles, 1·9 per cent.

The copper losses have been plotted for 4 poles and 50 ∿ only. Here it has to be borne in mind that the percentage copper loss

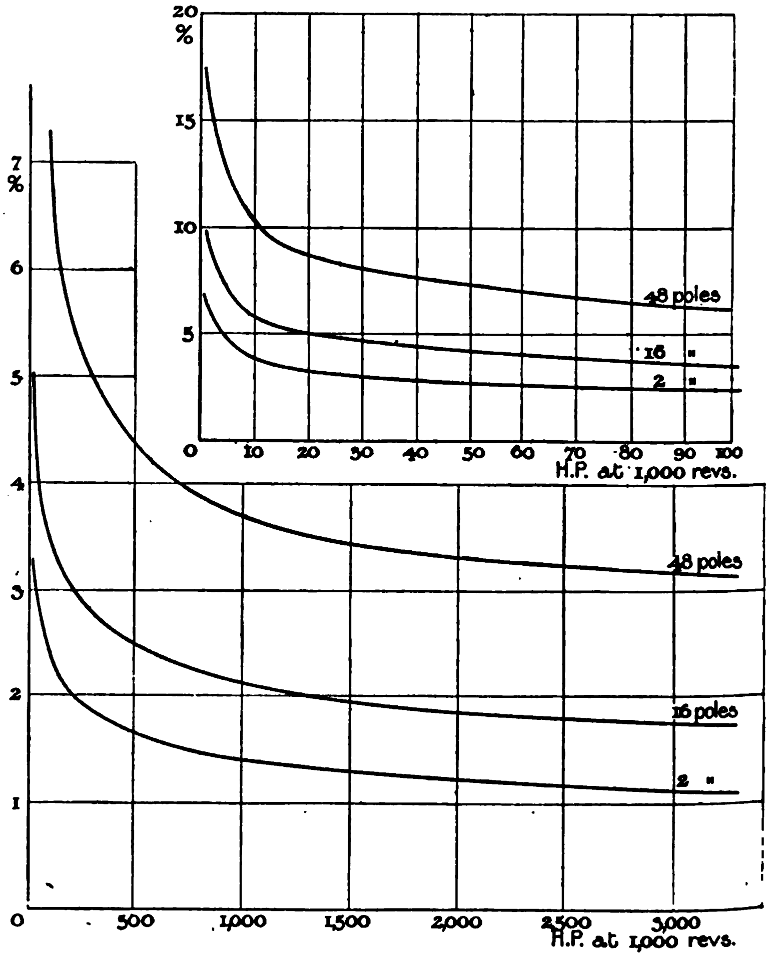


FIG. 15.—Core Loss of Induction Motors.

increases as the square root of the number of poles, and in proportion to the periodicity. The reason for this is that with an increasing number of poles the output falls proportionately, but the mean length of turn is reduced as the length of the end connections decreases. With different frequencies the copper losses remain constant, whilst the

output falls with the periods. If it is desired to know the copper loss of an 8-pole machine for 25 ∿ (375 revs. per minute), having a "power" of 100 H.P. at 1,000, it will be seen, from Fig. 16, that for 50 ∿ and 4 poles it is 2·7 per cent.; consequently in our case it amounts to—

$$2 \cdot 7 \times \sqrt{\frac{8}{4}} \times \frac{50}{25} = 7 \cdot 6 \text{ per cent.}$$

This copper loss is the stator and rotor loss combined. With

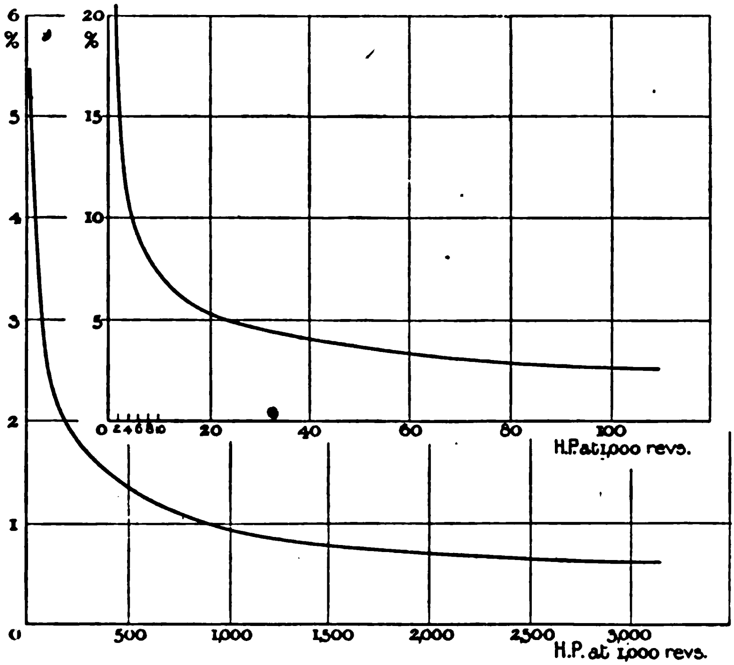


FIG. 16.—Copper Loss of 4-pole Induction Motors.

moderately good machines about 44 per cent. of the copper losses are in the stator, 56 per cent. in the rotor, so that the rotor losses in our case are $7 \cdot 6 \times 0 \cdot 44 = 3 \cdot 4$ per cent. of the output of the machine, unless a squirrel-cage rotor with specially high resistance is employed. This 3·4 per cent. may be called the natural rotor losses, or, what is practically the same, the natural slip. With squirrel-cage machines it is about 20 per cent. less than this amount, which refers to wound rotors. For obtaining high-starting torque this slip can be artificially increased by the use of end rings of high-resistance metal in the rotor. I shall refer to this in the section on starting.

Figs. 17 and 18 show the complete efficiency curves for 50 and 25 ∿ for polyphase machines. Fig. 19 gives the efficiency of single-phase induction motors for 50 and 100 ∿.

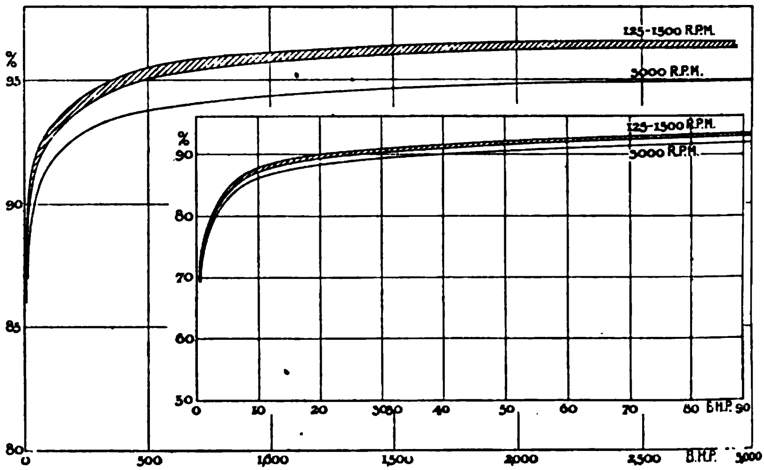


FIG. 17.—Efficiency of 50-∿ Induction Motors, Polyphase.

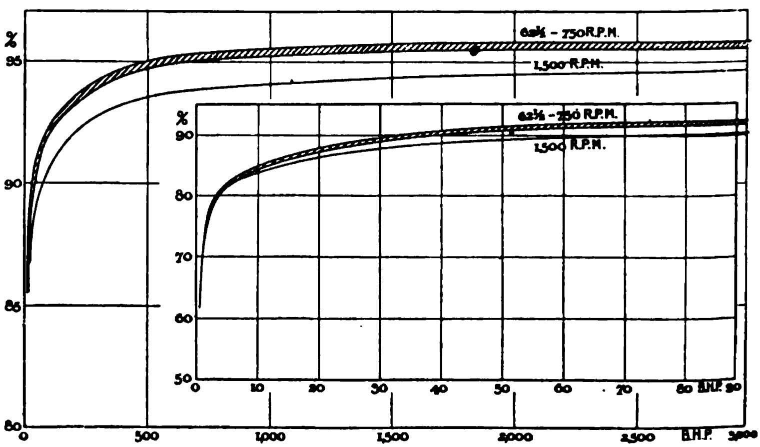


FIG. 18.—Efficiency of 25-∿ Induction Motors, Polyphase.

In these curves the most striking feature is the fact that the efficiency is almost independent of the speed. It has been possible to show a narrow shaded area within which the efficiencies fall for any speed between 125 and 1,500 revs. per minute. Since the percentage core loss falls and that of the copper and friction loss rises as the

speed goes up, the total losses remain nearly constant for a certain output.

A comparison will show that the difference between the efficiencies of direct-current machines and alternating-current induction motors is not considerable. Single-phase motors naturally are considerably worse than both (Fig. 19).

The Power Factor.—In saying that the efficiencies of direct-current and induction motors are practically identical, we have to restrict this to the real efficiency, the apparent efficiencies naturally being much worse with the latter class of machines.

It is often important to know clearly beforehand what the power factor is likely to be before deciding on the speed, as a few revolutions may often convert a bad motor into a moderately good one.

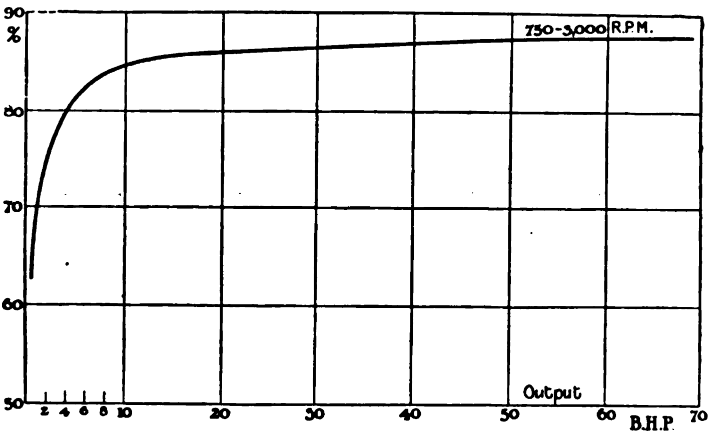


FIG. 19.—Efficiency of 50- and 100 ∿ Induction Motors, Single-phase.

It is hardly practicable to draw a great number of power-factor curves which might enable one to read off the figures at a glance, as there are too many items influencing the power factor, namely, the size of machine, expressed by the "power" H.P. at 1,000, the number of poles, and the overload capacity. The air-gap also influences the power factor, but, as hitherto, this detail of design is taken account of in determining the "size," that is, the power of the machine. Instead of a great number of curves for Cos ϕ we use the two Figs. 20 and 21.

It is well known that the power factor of an induction motor greatly depends on the leakage of the machine, this leakage being smaller the smaller the air-gap, the larger the diameter, and the lower the number of poles. Apart from this latter item the leakage only depends on details of design, so that a factor which is an expression for the tendency to admit leakage can be plotted as a function of the "power" of the machine.

We will call this factor the "*coefficient of lag,*" A.*

All details of design have been crammed into this coefficient, and it has been so selected that if multiplied by the number of poles Hey-

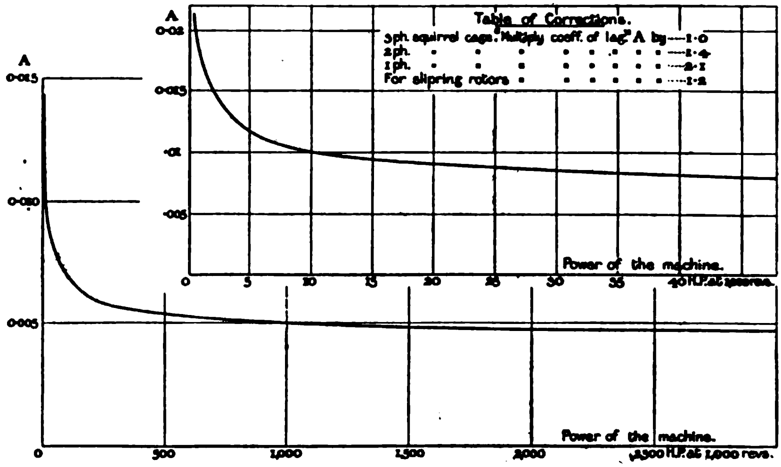


FIG. 20.—Coefficient of Lag of Induction Motors.

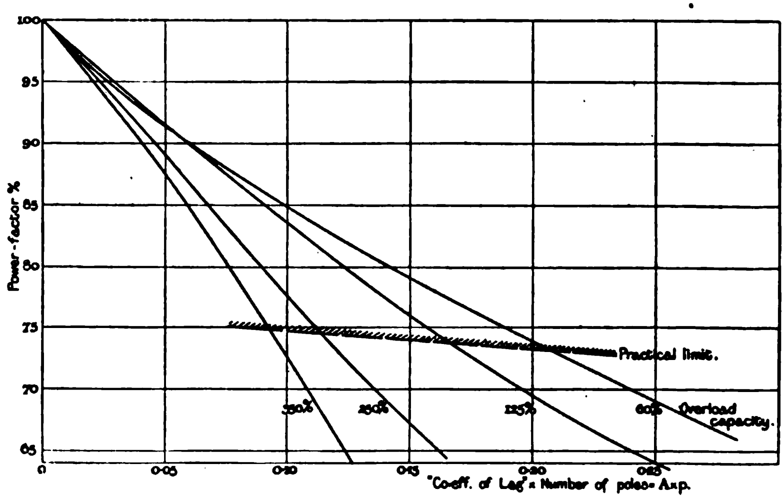


FIG. 21.—Power Factors of Induction Motors.

land's leakage factor appears. The meaning of A can be easily understood by studying the treatises by Behrend, Hobart, S. P. Thompson, and others on induction motors.

* I have to thank Mr. F. C. Aldous, of Manchester, for this term.

Fig. 20 shows the coefficient of lag A dependent on the "power" H.P. at 1,000 for 3-phase motors with squirrel-cage rotors, whilst Fig. 21 directly gives the power factor at normal load for different overload capacities, dependent on the product "A × number of poles." If it is desired to know the power factor of a 20-H.P., 500-rev., 50-∿, 3-phase motor, having, say, 125 per cent. overload capacity, that is, a machine with $120 \times \frac{50}{500} = 12$ poles $= p$, and a "power" of $20 \times \frac{1{,}000}{500} = 40$ H.P. at 1,000, we pick out from Fig. 20 $A = 0{\cdot}0082$; $A \times p = 0{\cdot}0082 \times 12 = 0{\cdot}099$. From Fig. 21 for this $[A \times p]$ and 125 per cent. overload capacity the power factor $= 83\frac{1}{2}$ per cent. If an overload capacity of 350 per cent. were required, the power factor would have been reduced to 73 per cent.

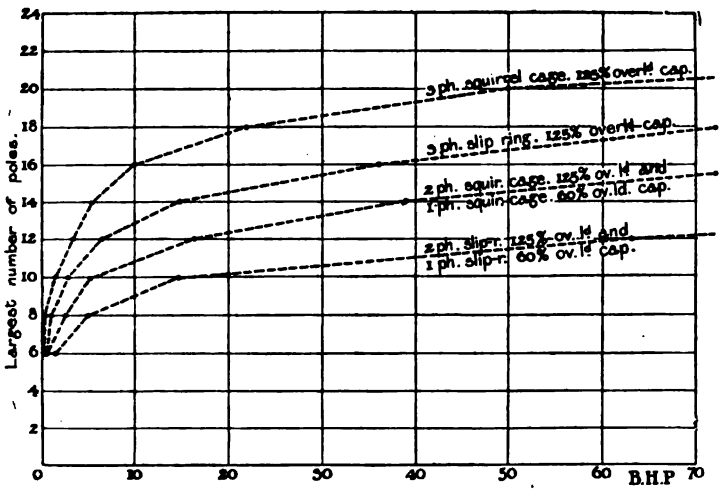


FIG. 22.—Limit Number of Poles of Induction Motors.

The coefficient of lag A in Fig. 20, referring to 3-phase squirrel-cage motors, is smaller than with any other class of motor, so that 3-phase squirrel-cage machines have the highest possible power factor. A has to be multiplied by the following constants: with 2-phase machines by 1·4, with 1-phase machines by 2·1.

In all cases where the rotor is a wound one the coefficient of lag increases another 20 per cent. The motor in the above-mentioned example, having as 3-phase squirrel-cage machine an $A = 0{\cdot}0082$, would as 2-phase slip-ring motor have an $A = 0{\cdot}0082 \times 1{\cdot}4 \times 1{\cdot}2 = 0{\cdot}0168$. $A \times p = 0{\cdot}0168 \times 12 = 0{\cdot}20$. With 120 per cent. overload capacity the power factor works out at $69\frac{1}{2}$ per cent., compared with $83\frac{1}{2}$ per cent. as a 3-phase squirrel-cage machine. I should call this an almost impossible machine, as the wattless component of the current is larger

than the watt component, and the no-load current will not differ very greatly from that at full load. I have drawn a line in Fig. 21 which represents the line of desirable practical limits, which only in exceptional cases ought to be exceeded. To make this still clearer the chart, Fig. 22, has been added, giving the highest number of poles a motor ought to be built for, the overload capacity being 125 per cent. in the case of polyphase and 60 per cent. with single-phase machines. From this chart it will be seen that a 20-H.P. motor as 3-phase squirrel-cage machine ought not to have more than 16 poles, as 3-phase slip-ring not more than 14 poles, as 1- or 2-phase squirrel-cage not more than 12 poles, as 1- or 2-phase slip-ring not more than 10 poles.

This is rather hard on single-phase 100-∿ motors, but these are troublesome machines in many other respects. They can, however, be

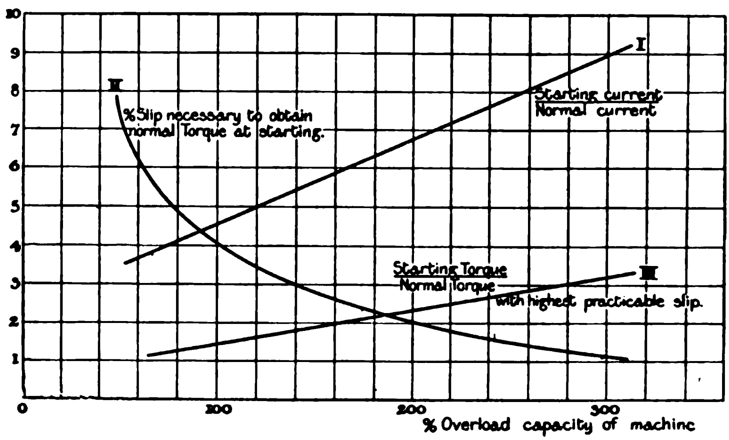


FIG. 23.—Starting of Squirrel-cage Motors, Polyphase.

built if one is cautious in the selection of speeds. By building specially narrow 100-∿ motors the performance may be slightly improved.

It is necessary to point out that, when finding the coefficient of lag for single-phase motors from Fig. 20, this is to be taken at an abscissa for 50 per cent. larger output, as the "size" of a single-phase machine is 50 per cent. larger than the corresponding 3-phase one.

The Starting of Polyphase Squirrel-cage Motors.—The starting performance of a slip-ring motor where resistance is inserted in the rotor is too well known to be mentioned here, and it does not offer any special points which depend on size or design.

The starting of squirrel-cage motors, however, often causes uncertainty as to the current consumption and the torque obtainable.

We have to distinguish between machines being thrown straight on to the line and those started by means of a starting transformer, which reduces the voltage on the motor terminals.

If thrown straight on to the line, the current taken by the motor depends on the overload capacity it has been designed for, the current being larger the stronger the motor. Fig. 23 shows in a curve the approximate ratio $\frac{\text{starting current}}{\text{normal current}}$ dependent on the overload capacity. With very bad machines this ratio is somewhat smaller than that given here, due to the high no-load current. It will be noticed that with 125 per cent. overload capacity the starting current is about five times

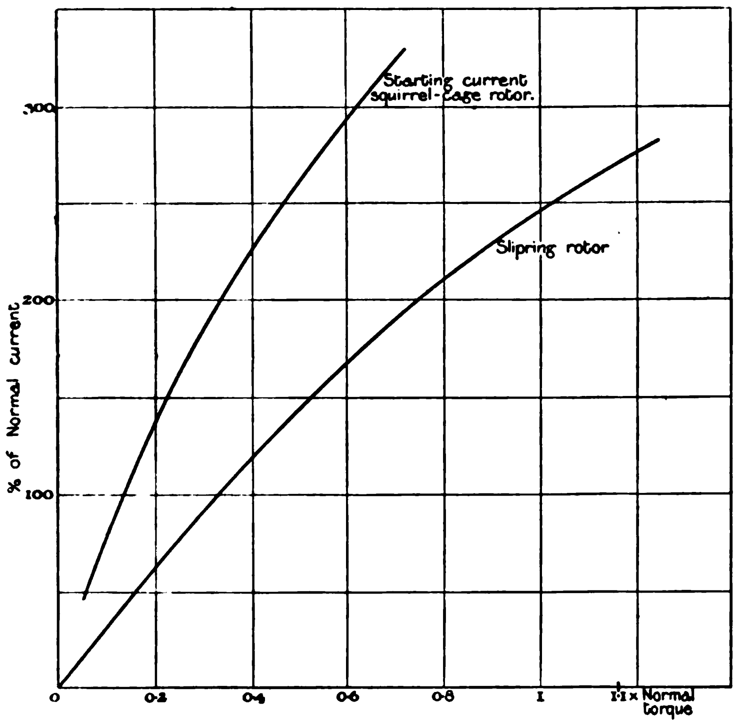


FIG. 24.—Starting Current of Single-phase Motors.

the normal current. If we wish to obtain a starting torque equal to the running torque, the rotor must have its resistance adjusted to give the percentage of slip as shown by curve II. in Fig. 23.

In some cases, especially with American machines, a good efficiency is not regarded as of great importance compared with good starting qualities. Also, short-circuit rings of the squirrel-cage are made of very high resistance metal, and slips of 6 and, with small machines, even 10 and 12 per cent. are considered quite permissible. In this case very high starting torques can be obtained. Curve III. shows about the highest starting torque practicable. For instance, a machine with

150 per cent. overload capacity, having a starting current of 5¾ times the normal, can be expected to develop about 1·9 times the normal torque at starting.

If in this case the normal torque is required a starting transformer can be used, cutting down the starting current in the ratio $\frac{1}{1·9}$ to $\frac{1}{1·9} \times 5\frac{3}{4} = 2\frac{3}{4}$ times the normal.

Very often the windings are switched in star connection at starting, being in delta at normal load, in order to reduce the starting current. Then the starting currents are reduced, together with the torques, to one-third of the figures given in Fig. 23. A motor having 200 per cent. overload capacity, the starting current being 6¾, and the starting torque 2¼ times the normal, would, in the star-delta method of starting, develop a torque of $1/3 \times 2\frac{1}{4} = \frac{3}{4}$ normal torque, and would consume $1/3 \times 6\frac{3}{4} = 2\frac{1}{4}$ times normal current.

Starting of Single-phase Motors.—Single-phase induction motors with slip-rings are as a rule started with choking-coils, and squirrel-cage machines with resistances as a means of splitting the phase. Only rarely are condensers employed.

Whatever means are used for phase-splitting, certain practical limits are encountered which without very much difficulty may also be followed up theoretically. I have given in Fig. 24 the starting current of single-phase motors as a multiple of the normal for different starting torques, the overload capacity being assumed to be about 75 per cent.

Maximum Voltage.—Before concluding the section on induction motors I should like to allude to a question often asked, What is the maximum voltage for which a motor can conveniently be built? When ought a transformer to be used with the motor? This naturally depends very much on the design. Often the end brackets are too tight to take high-voltage end connections, and questions of detail of design have to be taken account of. If this is not the case, I may say that the voltage of an induction motor ought not to be so high that with large machines the current falls below 2½ amperes and with small machines below 1½ amperes.

SINGLE-PHASE COMMUTATOR MOTORS.

The real efficiency of a single-phase commutator motor is generally worse than that of a similar machine of the induction type, though the efficiency of the latter is not particularly good. The cause of this is chiefly to be attributed to the losses in the resistance connections between armature winding and commutator lugs and to the enormous core loss taking place in the whole body of the machine. These losses are particularly high with high frequencies, and this is one of the reasons why generally the frequency of single-phase motors is kept as low as possible, so that actually its performance approaches that of

direct-current machines. But naturally the performance of an induction motor would also be excellent at very low frequency, though it would under no condition approach the perfect behaviour at starting of the commutator motor.

As characteristic motors I have chosen a small, a medium, and a large machine—5 H.P. at 1,500 revs. per minute, 50 H.P. at 750 revs.

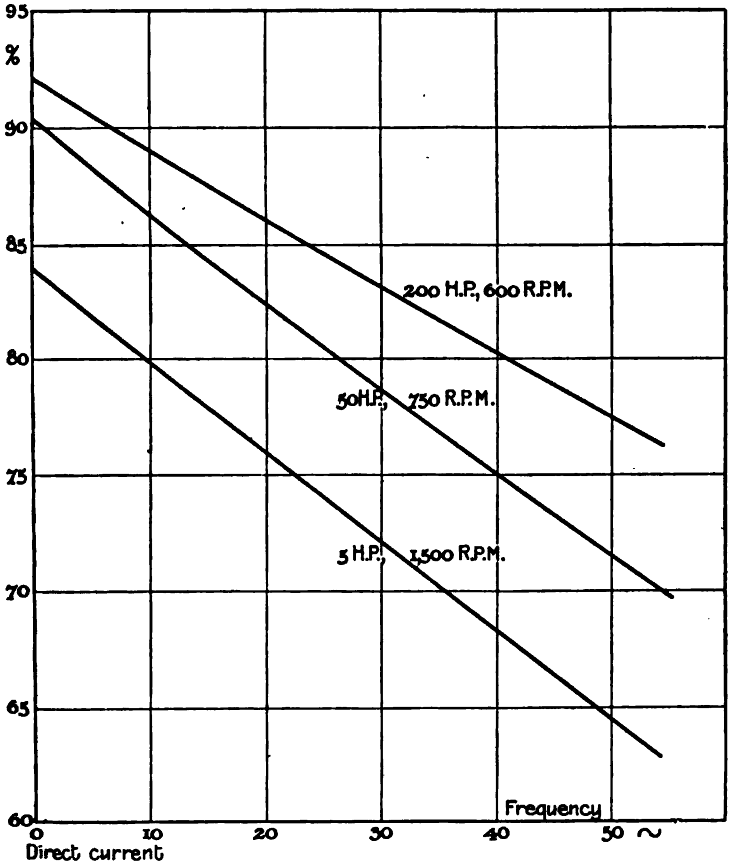


FIG. 25.—Efficiency of Alternate-current Commutator Motors.

per minute, 200 H.P. at 600 revs. per min.—and efficiencies and powe factors have been plotted as a function of the periodicity (Figs. 25 and 26). It must be borne in mind that these curves do not represent tests on one machine, say a machine designed for 25 ∿ and run at different frequencies; but the 50-∿ machine is to be considered as a design entirely different from the 25-∿ one, each being built so as to form the best machine for its special conditions.

The power factors of Fig. 26 are those of plain single-phase commutator motors with compensating winding. They may be improved by using machines of the Latour and Winter-Eichberg type or by applying special means of other kinds to raise the power factor, generally at a sacrifice of 1 or 2 per cent. in the efficiency.

TRANSFORMERS.

Transformer efficiencies at normal load only do not always serve very well as an indication of the real quality of the apparatus, less so

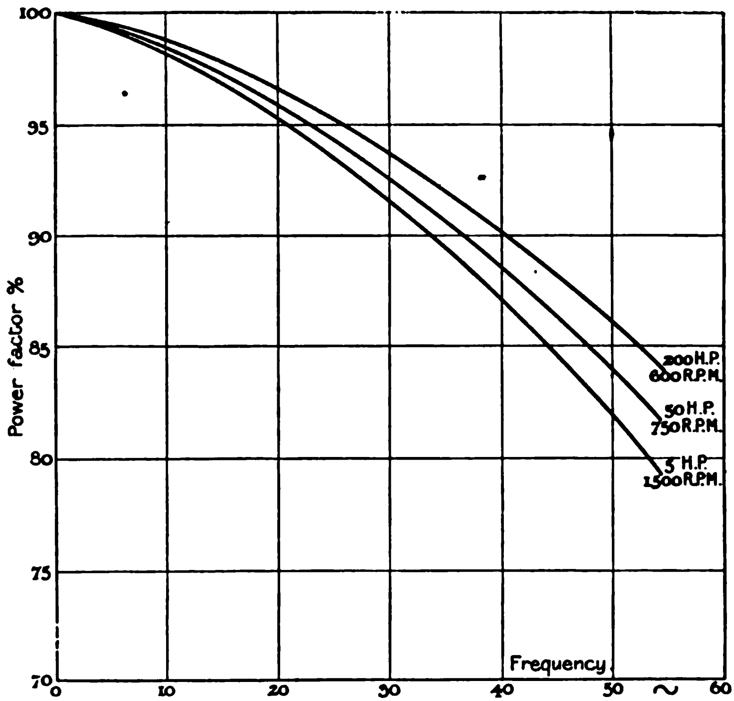


FIG. 26.—Power Factors of Alternate-current Commutator Motors.

than in the case of the average motor and dynamo. This is true, at all events, with lighting transformers. With power transformers the normal efficiency is quite a useful figure.

The efficiencies plotted in Fig. 27 are not the normal but the maximum figures obtainable from a certain transformer—that is, when core and copper losses are equal. Usually it is possible to shift this maximum somewhat, putting it with lighting transformers near half-load, with power transformers near full load. By stating the maximum efficiency only, the curves in Fig. 27 will prove useful, even for finding

efficiencies at very small loads, if one only takes account of the fact that one half of the losses are core losses and the other half copper losses, which vary as the square of the output.

I have not extended the scale of the K W beyond 70 k.w., as such a transformer as a rule has as good an efficiency as a 700-k.w. one. The following may be given as efficiency limits: 100-∿ transformers

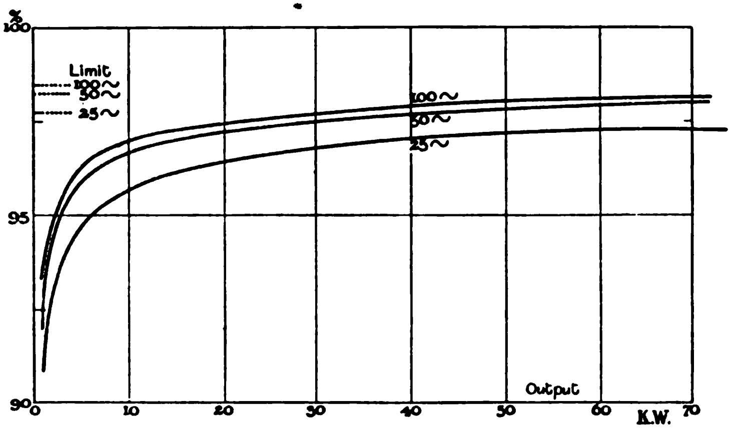


FIG. 27.—Transformer Efficiencies.

98½ per cent., 50∿ transformers 98¼ per cent., 25-∿ transformers 97¾–98 per cent.

It was my intention to deal also with special machines as boosters, induction motors with pole-changing devices, motors for accelerating heavy loads, and others; but I feared that by introducing further developments and theories in this direction it would only tend to divert discussion from the main question, What is the normal performance of an ordinary machine?

DISCUSSION.

Dr. SILVANUS P. THOMPSON: I think, sir, it is fortunate that in this, the second paper that has been read under your presidency, we have Dr. Goldschmidt, who was so happily connected with your firm, giving us another contribution to the subject of dynamo design. This paper strikes me as valuable, for it gives us a means of studying the comparative values of designs from different sources and of different classes of machinery. Every method that enables us to compare the goodness of the design and performance of a machine with those of other machines is of great use in the making of further improvements in design, and many of the curves in the paper are particularly helpful from that point of view. I have not yet been able to examine whether they fit the data that are known to me; they may, or they may not. I presume they are derived

Dr. Silvanus Thompson.

Dr. Silvanus Thompson.

mainly from experience which Dr. Goldschmidt has himself had, and are approximately therefore in agreement with the best practice of various firms. Taking them only at that, they are exceedingly valuable, because we can now, all of us, plot on the same framework the corresponding values for machines by other makers with which we are acquainted, and see whether the points so plotted come anywhere near these, and in that way find out what any discrepancy between them may indicate. If I am not mistaken, these curves are nearly all derived from experience, and correspond to the actual average performance or design of the machines. At the same time, if any of them are derived or derivable from first principles it is well that we should know that they are so derivable, and are not dependent upon mere empirical results obtained from any particular make of machine. I therefore think it would be useful if the author, in his reply to the discussion, would indicate whether any of these curves are deduced from first principles, and which of them are the results purely of practice.

Having said that, I next want to refer to page 456, in which, I take it, the main principle of the paper is laid down. We are invited to compare together machines on a particular basis, namely, that of the kilowatts which the machine in question would give out if its normal number of revolutions per minute were to be altered to the standard number of 1,000. Of course, it is not proposed that we should take any and every machine and drive it at 1,000 revs. per minute, but we are to consider what its nominal power would be if we altered its speed in that proportion to the normal of 1,000 revs. per minute, and consider the nominal kilowatts on that basis. And so we get a rather puzzling expression where the word "power" appears in quotation marks, on page 459. This word is used for the imaginary power that the machine would have if its kilowatts were altered in the proportion of its own revolutions to 1,000 revs. per minute. One does not like to be too critical over the use of words, but I do feel inclined to raise a protest against putting power in quotation marks there, because it is not power. It is power altered in an arbitrary proportion for the convenience of comparing one machine with another. I wish the author could have invented some other name instead of putting power in quotation marks. For instance, he might have called it the "powerage" of the machine, terminating like voltage, or other words of that form. But although that might have been desirable from the point of view of clearness of language, a little further reading has enabled me to see there is no need to do that, for looking at the definition of this quantity "power" its real meaning will be seen.

Now taking the kilowatts and multiplying by 1,000 gives us the number of watts of that machine, and dividing by its own revolutions per minute gives us the number of watts per revolution per minute. That is the meaning of this quantity called "power"; it is the number of watts at 1 rev. per minute of the machine reckoned from its full load. We can all of us think of that without any quotation

Dr. Silvanus Thompson.

marks at all. Here is a machine which at full load produces a certain number of watts and is running at a certain speed, therefore so many watts per revolution; and we have the statement then, reading that in that the number of watts per revolution per minute is equal to a certain constant, multiplied by the total number of ampere-wires round the armature of the machine, multiplied by the total flux coming in or out from all the poles. That is an extremely simple expression, and it does not depend upon any maker's particular kind of machine. It is derivable literally from first principles. I wish to show quite simply in three lines how it is derivable from first principles, and what is the meaning of the constant that comes in, taking the case of continuous-current machines—

$$E = \frac{p}{c} \times \frac{\text{RPM}}{60} \times Z \times N \div 10^8 \quad . \quad . \quad . \quad . \quad . \quad \text{(I.)}$$

$$C = \frac{\text{AW} \times c}{Z} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(II.)}$$

In equation (I.) E is the generated volts, p the number of poles, c the number of circuits through the armature winding, Z the total number of armature conductors or "wires" all round the periphery, and N the flux from one pole. In equation (II.) C is the whole current (at full load), and A W is the number of "ampere-wires" as defined by Dr. Goldschmidt. The first of these equations is the fundamental equation of electromotive force derived straight from Faraday's principle of induction and the definition of the volt. The second results from the definition of the "ampere-wires."

Now multiply the two equations together, and divide both sides by the revolutions per minute, when we obtain the third equation—

$$\frac{\text{EC}}{\text{RPM}} = \frac{\text{AW} \times p\text{N}}{60 \times 10^8}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(III.)}$$

when pN is the same as Dr. Goldschmidt's F, so that we may write it also—

$$\frac{\text{EC}}{\text{RPM}} = \text{F} \times \text{AW} \times 0{\cdot}166 \times 10^{-9}.$$

For any and every continuous-current machine, of anybody's make, the watts per revolution are equal to the total flux from all the poles, multiplied by the ampere-wires all the way round, and multiplied by a constant which cannot be any greater or less. No matter who makes the machine, it must be $0{\cdot}166 \times 10^{-9}$. What value does the author give? He says, "With direct-current machines, polyphase alternators, and polyphase induction motors, the constant averages $0{\cdot}165 \times 10^{-9}$ in. Now 0·165 is practically the same as 0·166; so that we are in agreement as to that figure; only I say here that this is the absolute and unalterable value of the constant for every continuous-current machine, and Dr. Goldschmidt gives it as the average for continuous-current machines, polyphase alternators, and polyphase induction motors.

Dr. Silvanus Thompson.

Can it be any different for polyphase alternators or polyphase induction motors? It can of course be, because the real power of a polyphase alternator or a polyphase induction motor is not the same as its volts and its amperes multiplied together, in the sense in which we have it in continuous-current machines. We have to think, in the first place, of virtual values, which involve the square root of mean square (or form factor); we have also to think of the power factor involved in the amount of idle current. It is therefore true that for polyphase alternators and for induction motors, assuming that there is no change arising from the form-factor question, this number would be the same if the power factor were unity. If the power factor is less than unity then this will be smaller than 0·166. That may in some cases be compensated for by the circumstance that, taking the form factor as about 1·1, then, if the value of our coefficient is raised 10 per cent. by form factor, and its value is lowered about 10 per cent. by power factor, the two effects may thus nearly balance one another. Hence with other classes of machines we should have nearly the same value for the coefficient. Now for the single-phase machines it averages, according to the author, 0·145, a lower value. I do not understand that particular result, because I cannot see how it can be a very much lower value unless it is to be supposed that the power factor for a single-phase machine is always a bad one, or that its form factor is always abnormally low.

The next page of the paper gives us an exceedingly interesting diagram, from which, I presume as the result of experience, the values are plotted in curves, of the ampere-wires all the way round, and the flux from all the poles taken together, in terms of the watts per revolution per minute. The two curves in Fig. 1, which are, I was going to remark, of general parabolic outline, are exceedingly interesting, because they show—again I presume as the result of experience—that for small machines and large machines alike (that is to say for machines that have a small number of watts per revolution or a large number of watts per revolution per minute), from one end of the range to the other, you have about the same proportional height between the curves Take the scales into account, the flux being given in millions and the ampere-wires in thousands. Take, for instance, the machine having 2,000 watts per revolution, it will be seen, by following up the ordinate, that such a machine will, as built to-day, have about 1,000,000 for the total flux from its poles, and about 125,000 ampere-wires all the way round; and the proportion of the one to the other is 800. It is about 800 for all machines apparently. That is to say, in this equation for the output of watts per revolution per minute, the ratio of those two factors, as machines are built to-day, is always of the order of 800. The factor F is always about 800 times the factor A W. Why should that be so? Apparently because trouble will occur with that machine if we depart seriously from that proportion. If we have too much copper and too many ampere-wires on the armature in proportion to the flux that is coming in from the poles, then there will be too much distortion

or demagnetisation and probably commutation troubles.* If we require a machine to work without giving trouble in the commutation, the total loading of the armature must not be too great in proportion to the total flux coming in. I do not say that is the only requirement, as everybody well knows, and nobody better than the author; but apparently it is something of that sort which determines the relative magnitudes of these two important factors of continuous-current machines, whether large or small, and fixes this proportion at 800. It certainly simplifies the roughing out of a design if one has a notion, based on experience, that one factor should be about 800 times the value of the other. Dr. Silvanus Thompson.

As I have already said, I have not had time to go into the details of the meaning of all these other curves. I imagine we have in this paper a fund of most valuable information as to what is the average practice with particular classes of machines when compared with one another on this exceedingly simple and rational plan. Therefore I desire to express my own thanks, at any rate, to the author for having put before us an exceedingly interesting and useful basis of comparison.

Mr. V. A. FYNN: My first point refers to the constant or coefficient about which Dr. Thompson has just been speaking. In discussing the constant in the author's formula— Mr. Fynn.

$$\text{“Power” or specific output} = \text{constant} \times AW \times F,$$

I will confine my remarks to motors, but the general line of thought, of course, holds good for generators as well. Considering the continuous-current motor shown in Fig. A when revolving, say, in a counterclock direction, we know that two fluxes are present in such a machine: the actual motor flux N, due to f, and which is responsible for the torque of the machine in conjunction with the armature ampere-turns, also the flux R, due to the armature ampere-turns themselves. This latter is kept as small as possible in all continuous-current machines, for it tends to distort and eventually to weaken the effective flux N. In the formulæ written by Dr. Thompson N stands for this effective motor flux, and I agree with him that if the effective flux *only* is considered, then the constant in question must be the same for all machines. I believe, however, that the author's intention is to consider the *total* resultant flux F in any given machine; in fact, he says as much in the last paragraph but one on page 456. When F in the author's formula is given this meaning, then his constant will indicate that proportion of the total flux in a given machine which is useful from the point of view of torque production; in other words, his constant will then be that coefficient with which the total flux of a given machine is to be multiplied in order to obtain that flux for which the letter N is used in Dr. Thompson's formulæ.

If, then, a continuous-current motor be considered such as shown in Fig. A, then N will be smaller than F, because R differs from zero

* See a somewhat similar treatment of the question in my Howard Lectures on "High-speed Electric Machinery," pages 9 and 10.—S. P. T.

Mr. Fynn. to an extent depending on that number of armature ampere-turns which are effective in a direction perpendicular to that of N, and also on the reluctance of the path available for the flux R. The author's constant must in this case be *smaller* than $0{\cdot}166 \times 10^{-9}$ for $F = \overline{N + R}$.

If the neutralised continuous-current motor shown in Fig. B be considered, then R is obviously zero, being annulled by the neutralising winding *n*. In this case the author's constant is $0{\cdot}166 \times 10^{-9}$ for $F = N$.

In the case of alternate-current motors we must, of course, consider effective values of currents and fluxes and take their mutual phase relation into account; in what follows small phase differences will be ignored. Fig. B can be taken to represent a neutralised single-phase series conduction motor as well as a continuous-current machine. It is clear from the above that for such a single-phase motor the author's constant must also be $0{\cdot}166 \times 10^{-9}$. An alternate-current motor without neutralising winding as shown in Fig. A, for instance, need not be considered, for such machines are practically useless unless the periodicity is extremely low.

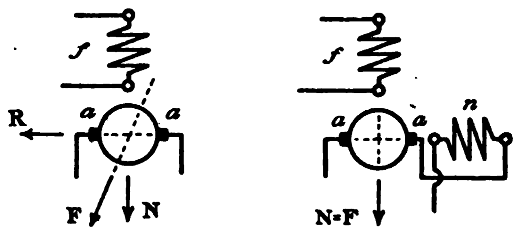


FIG. A. FIG. B.

The 2-phase shunt-induction machine shown in Fig. C has two motor fluxes N_1 and N_2, due respectively to the transformer windings f_1 and f_2. N_1 is generally equal to N_2, and the two differ in phase by 90°. There are also two currents induced in the rotor, these are i_1 along the axis of f_1, and i_2 along that of f_2. These currents are equal, that is, $i_1 = i_2$ if $N_1 = N_2$, and they differ in phase by 90°. There are two torques in the machine, which are respectively proportional to $(i_1 z_2 \times N_2)$ and to $(i_2 z_2 \times N_1)$, where z_2 is the number of turns in the rotor. The total torque is therefore proportional to $2(i_1 z_2 \times N_2)$, or to $2(i_2 z_2 \times N_1)$. Considering the resultant flux and the resultant ampere-turns instead of their real components, we find $F = 1{\cdot}41\ N_1 = 1{\cdot}41\ N_2$, and $i z_2 = 1{\cdot}41\ i_1 z_2 = 1{\cdot}41\ i_2 z_2$. The axis of the resultant ampere-turns $i z_2$ is indicated in Fig. C at *aa*. The total torque is therefore proportional to $(i z_2 \times F) = (1{\cdot}41\ i_1 z_2 \times 1{\cdot}41\ N_2) = 2(i_1 z_2 \times N_2)$. In the case of a 2-phase induction motor the author's constant is therefore $0{\cdot}166 \times 10^{-9}$, for the whole of the rotor ampere-turns and the whole of the motor flux are effective. It is interesting to note that in this respect the continuous-current machine is inferior to the polyphase one, unless the former is neutralised as shown in Fig. B.

Mr. Fynn.

The single-phase shunt-induction motor shown in Fig. D also has two fluxes at right angles in space and in time quadrature; of these T is induced by the stator winding s, and N is generated when the rotor revolves. At or near synchronism N practically equals T. The armature current i_2 is induced in the rotor along the axis of T, the exciting current i_3 is generated in the rotor along the axis of N. These two currents are here of same time phase and not in time quadrature as was the case in the 2-phase machine. This being so, it is obvious that if N is of same phase as i_2 and i_3 (which is actually the case), then T must be in time quadrature with both currents, and cannot produce a torque with either. But the space position of N is such that it can only produce a torque with i_2, so that there is only one torque in the machine. T is idle as far as torque production is concerned; in fact, it is only the vehicle by means of which the energy necessary for the operation of the motor is conveyed from the stator winding s to the rotor. Roughly speaking, the torque here is proportional only to $i_2 z_2 \times$ N.

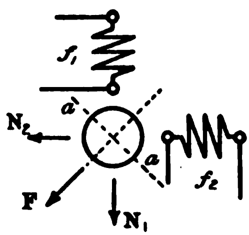


FIG. C.

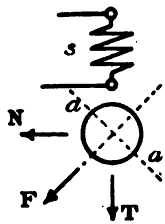


FIG. D.

Considering the resultant flux F and the resultant ampere-turns $i z_2$ instead of their real components, we find, as in the case of the 2-phase machine $F = 1{\cdot}41\ N = 1{\cdot}41\ T$, but the resultant ampere-turns $i z_2$ along the axis $a\ a$ are here only equal to $i_2 z_2$. Because i_2 and i_3 are of same phase they simply add arithmetically in each quarter of the rotor (assuming the machine to be a 2-pole one), with the result that the ampere-turns along the axis $a\ a$ are the same as those along the axis of T, and which latter are the true armature ampere-turns. We must therefore write the torque as proportional to—

$$0{\cdot}7 \times F \times i z_2 = 1{\cdot}41 \times 0{\cdot}7 \times N \times i_2 z_2 = N \times i_2 z_2.$$

The author's constant for the case of a self-excited self-compensated shunt-induction motor should therefore be—

$$0{\cdot}7 \times 0{\cdot}166 \times 10^{-9} = 0{\cdot}116 \times 10^{-9},$$

and not $0{\cdot}145 \times 10^{-9}$. My point is that this constant varies, as I have shown, with the type of machine, and no general statement relating to this constant and such as given in the paper can possibly be true.

Mr. Fynn.

There is another point I should like to mention, which refers to single-phase commutator motors. The author says on page 478, "The real efficiency of a single-phase commutator motor is generally worse than that of a similar machine of the induction type." I feel certain that the author does not wish us to believe that there are no induction commutator motors, although the statement referred to might lead one to think so. In all his latter remarks he deals, I think, with conduction machines only. He speaks on page 480 of a "plain single-phase commutator motor." I understand that it is a series conduction machine, and in that case I daresay the power-factor curves, Fig. 26, are roughly correct for speeds below the synchronous. If, however, such a motor is worked at some 40 to 50 per cent. above the synchronous speed, as it should be, then the curves are too low. The efficiency curves, Fig. 25, are, I think, decidedly too low. I can remember series-conduction motors built by Messrs. Ganz & Co. as far back as 1888 or 1890, and their favourite size was a 5-B.H.P. machine. This size had an efficiency of 75 per cent., and hundreds were built. I suggest that Fig. 25 be altered roughly as follows: If machines with a shunt characteristic are referred to, then a 5-B.H.P. motor can safely be said to have an efficiency of 70 per cent. at 50 $\sim$, that is to say, an increase of about 6 per cent. on the figure given by the author; the efficiency of the 50-B.H.P. motor should be increased to 81 per cent. at 50 $\sim$; that of the 200 B.H.P. to 87 per cent. at 50 $\sim$. These are quite conservative figures. If the machine is of the series-conduction type, then all the efficiencies may be written up another 3 per cent. In the case of a series-induction machine the efficiencies will, as a rule, be the same as those of a machine with a shunt characteristic. I think this distinction between single-phase motors with a shunt and single-phase motors with a series characteristic might very well be introduced even in this paper, although the latter must, of course, be kept on as general lines as possible. The difference between these two types is so very marked that it must be taken into account. Single-phase shunt motors of the induction type, or even those of the conduction type, can be easily compensated for power factor, and machines of that sort are on the market; they are being built on the Continent and also in England. Their power factor is well above 0·95 for all loads, and is in the neighbourhood of that figure even at no load.

I think it most desirable, in view of the great variety of alternate-current motors now known, that precise language should be used when dealing with such machines, and particularly is this desirable in the case of single-phase commutator motors. I have proposed a nomenclature which makes it possible to define each type in a few words and beyond any possible misapprehension. I think that some such nomenclature should be generally adopted in preference to vague expressions such as have been used by the author.

Mr. Macfarlane.

Mr. J. C. MACFARLANE: The first thing that I wish to draw attention to in Dr. Goldschmidt's valuable paper is at the foot of page 455, and has reference to a 5-H.P. 2-phase induction motor for crane work,

which runs at the low speed of 500 revolutions on a 50-∿ circuit, and which also has a large overload capacity. Dr. Goldschmidt's paper would be a valuable one indeed if it prevented buyers of electrical plant from making such mistakes as this. The combination of low speeds and high frequencies in induction motors is a very common error made by buyers of such motors, and the trouble they cause to the manufacturer, due to this, does not appear to be appreciated by them. Turning now to the curves (Fig. 1) on page 457, it would appear that Dr. Goldschmidt has selected two curves, one representing the flux or magnetic loading of an armature as a function of the output at 1,000 revolutions, the other representing the ampere-wires, or electrical loading of an armature, also as a function of output at 1,000 revolutions, and it may be noted that the corresponding ordinates of these two curves bear a fixed ratio to one another. This ratio is approximately 800 : 1. that is, for any power whatsoever the magnetic loading of an armature is given by the curve as 800 times greater than the electric loading of the same armature. I cannot understand why Dr. Goldschmidt has selected this ratio in preference to any other ratio; nor can I see that any fixed ratio will suit the case—for I have known quite modern machines which have this ratio as great as 2,000 : 1, that is, the magnetic loading is 2,000 times as great as the electric loading of the armature. And I have also known machines having this ratio as small as 250 : 1, that is, the magnetic loading in this case is only 250 times as great as the electric loading of the armature. Dr. Thompson, in one of his Howard Lectures before the Society of Arts, has recently given this ratio as about 1,000 : 1, but it must be remembered that this figure depends to a very great extent on whether a machine has an armature core length which is abnormally great compared with its diameter. The very long machines will usually have a high ratio of magnetic to electric loading, whereas in narrow machines this ratio will usually be low. The machines already referred to—that is, the machines in which the magnetic loading was 2,000 times the electric loading—and the machines in which this ratio was 250 : 1, were quite normal machines, so far as core length goes. Another thing which may be noticed about the curves in Fig. 1, page 457, is that these are parabolas, and I should like to ask Dr. Goldschmidt why he has selected these curves for the purposes of this lecture. At the bottom of page 457 Dr. Goldschmidt makes two statements which I consider are not quite correct. The first is, "In most cases these fluxes are identical with the most economical flux, especially with moderate speeds." The second is, "In commutating-pole machines the flux may be 25 per cent. or 35 per cent. less than the flux given by the curves in Fig. 1." I presume he considers these are about the most economical fluxes for interpole machines. I do not know whether Dr. Goldschmidt refers to economy in the sense of electric magnetic losses, or whether he refers to economy in material, but in neither of these cases are his statements quite correct. For, in the case of electric and magnetic losses it is a well-known fact that the maximum economy is to be

Mr. Macfarlane.

Mr. Macfarlane. obtained when the sum of the constant losses of a machine is equal to the sum of the variable losses of a machine. Put more clearly, this means that the core loss, plus the shunt loss, should be about equal to the armature copper loss, neglecting for the moment the commutator losses. Turning to page 464, the losses are given for a 100-k.w. machine, and by adding these core and shunt losses together it will be found that they are exactly double the armature copper loss. If we go further than this, and add to the constant losses, the brush, bearing, and wind-friction losses, and add to the variable losses the brush drop, we shall find that the case will be still further aggravated. With regard to economy in material, the maximum economy in material is to be obtained when the sum of the costs of the iron and steel, etc., in the magnetic circuit, plus the cost of the shunt copper, is equal to the cost of the armature copper; or, for interpole machines, when the sum of the iron and steel costs, plus the shunt copper cost, is equal to the cost of the armature copper plus the cost of the interpole copper. What has been said may be more briefly put as follows: The electric and magnetic losses are a minimum when the total losses in the magnetic circuit are equal to the total losses in the electric circuit. Also the cost of the material in any machine is a minimum when the cost of the material in the magnetic circuit is equal to the cost of the material in the electric circuit. I have with me some figures on two machines, one of which has the same ratio of magnetic to electric loading as given by Dr. Goldschmidt's curves in Fig. 1. The first of these machines is a non-interpole machine, and has an output of 470 k.w. at 1,000 revolutions. The second machine is an interpole machine, and has an output of 475 k.w. at 1,000 revolutions. The total magnetic loading of the first machine is 50,000,000 C.G.S. lines, and that of the second is 30,000,000 C.G.S. lines, that is, the magnetic loading of the non-interpole machine is 65 per cent. greater than that of the interpole machine. The electric loading of the first machine is 60,000 ampere-wires, and that of the other machine is 100,000 ampere-wires, that is, the electric loading of the interpole machine is 60 per cent. greater than that of the other. The total losses of the first machine amounted to about 14½ k.w., and the total losses on the second machine amounted to about 13 k.w. (It will be noticed that the total loss in the interpole machine is about 10 per cent. less than in the non-interpole machine.) The loss in the electric circuit of the first machine was 3·5 k.w., and the loss in the magnetic circuit was 11 k.w., giving a ratio of about 3 : 1 in favour of the magnetic circuit losses. The loss in the electric circuit of the interpole machine amounted to 6·2 k.w., and in the magnetic circuit it amounted to 6·8 k.w., making a ratio of 1·1 : 1 in favour of the magnetic circuit losses. Note that the losses, in the case of the interpole machine, in the magnetic and electric circuits are practically equal, whereas the ratio of the losses in the case of the non-interpole machine is about 3 : 1, which tends to prove the rule given already referred to. With regard to costs, the total cost of the material in the non-interpole machine came out at about £154,

whereas in the case of the second machine (interpole machine) the cost came out at about £128. Therefore there is about 20 per cent. more material on the non-interpole machine than there is on the interpole machine. Mr. Macfarlane.

The material in the electric circuit of the first machine costs about £12, and the material in the magnetic circuit of this machine costs about £142, giving a ratio of about 12:1, whereas in the interpole machine the cost of the electric circuit material was about £38, and the cost of the magnetic circuit material amounted to £90, making a ratio of only about 2½:1. Although the interpole machine does not appear to be designed to give quite the most economical distribution of material, it has a considerable advantage over the non-interpole machine in this respect. It will be readily seen from figures given, and also for other reasons, that the fluxes given by Dr. Goldschmidt in his curves, Fig. 1, are not by any means the most economical fluxes, either from the point of view of efficiency or the economy of material. The temperature rise on both these machines was approximately the same, that on the interpole machine being slightly lower, if anything. As the magnetic loading in the non-interpole machine is about 50,000,000 C.G.S. lines, and the electric loading in this machine is about 60,000 ampere-wires, then the ratio of magnetic to electric load is about 800:1, whereas in the interpole machine this ratio is about 300:1. The non-interpole machine is therefore a very similar machine to that which would be obtained by designing on the curves in Fig. 1, and the armature core, armature copper, and shunt losses obtained from tests compare very well with the figures obtained from the curves Figs. 2, 3, and 6 in the paper. The objects of this discussion are: (1) To show that the selection of a fixed ratio for the magnetic to electric loading of an armature is inadmissible, for in practice this ratio varies greatly, and although the author's method may give efficiencies within 1 or 2 per cent. in most cases at full load, yet it will not give any idea of the relative values of the various core and armature losses, etc., within the machine. This is important because of the estimation of the efficiencies at other loads than full load. (2) To show that the fluxes given by the curve in Fig. 1 are by no means the most economical, either from the point of view of power loss or economy in material in non-interpole machines, and in interpole machines fluxes of 25 or even 35 per cent. less than those given on the curve referred to are also not the most economical for these machines. Turning to the question of induction motors, the author gives on page 469 a curve for the fluxes of induction motors as a function of outputs at 1,000 revolutions. I should like to ask Dr. Goldschmidt why he has not made this curve a parabola in the same way as for the direct-current machines. I should also like to say that this curve does not represent by any means the most economical curve from the point of view of outputs of induction motors for the given fluxes. That is, for a certain flux I should think in standard practice one would be able to get from 30 to 50 per cent. more output from induction motors than is given by this

Mr. Macfarlane. curve. Then at the bottom of page 473 the author states: "It is well known that the power factor of an induction motor greatly depends on the leakage of the machine, this leakage being smaller the smaller the air-gap, the larger the diameter, and the lower the number of poles." I think the author must mean by leakage in this sense "leakage factor," because the leakage of an induction motor does not decrease with the decrease of the air-gap. The leakage of an induction motor increases with the decrease of the air-gap, and does not necessarily decrease with a larger diameter, nor does it necessarily increase with a lower number of poles.

Mr. Burge. Mr. H. BURGE: Having been associated with Dr. Goldschmidt as his assistant a few years ago, I naturally take a great interest in his paper, which I think will be very useful to those who are not intimately acquainted with the ins and outs of electrical machine design. It enables them to estimate the performance they ought to expect from a certain machine. There is one point to which I should like to refer on page 470, where Dr. Goldschmidt says, "It must be borne in mind that the percentage copper loss increases as the square root of the number of poles, and in proportion to the periodicity." This is really not quite correct. If one makes the best design, increasing the number of poles lowers the speed proportionately and the output proportionately; but the carcase being the same size the same copper loss may be allowed. That is to say, if one is working for a certain temperature rise the copper loss may be the same, and therefore the percentage copper loss increases in proportion to the number of poles instead of the square root. There is another point I should like to refer to in connection with heavy flux machines and machines with heavy ampere-wires. It is found in comparing those two machines that, although the total watts lost on the armature may be the same in each case, an armature with heavy ampere-wires will probably be about twice as hot as that of the machine with the heavy flux. The surface is the same on both the armatures and the total watts are the same, but the result is that the heavy ampere-wire armature always shows a higher temperature rise. Perhaps Dr. Goldschmidt can give us some explanation of that.

The President. The PRESIDENT (Colonel R. E. Crompton, C.B.): There is no time for me to add to the detailed discussion on this paper. I feel that we owe a great deal to Dr. Goldschmidt, who has introduced to us many very precise and useful methods of working out windings, and advantageous proportions between flux and ampere-turns. I may say that I am in sympathy with two of the last speakers, and, in addition, I cannot quite understand how it is possible that the standardisation which the author proposes can be obtained to any considerable extent, except in the cases of belt-driven machinery, where the proportion of length to diameter of the armature is settled generally so as to obtain the greatest economy in the use of material. Wherever other considerations, such as the nature of the prime mover used, require these proportions to be considerably varied, the possibility of the generalisation and standardisation desired by the author very greatly disappears.

No doubt most of the figures given by Dr. Goldschmidt, and on which he bases his facts, refer to the belt-driven sets, but I am sure he will agree that in the case of turbo-driven sets, where the proportion of diameter to length of rotating parts is varied greatly from mechanical considerations, his conclusions will be found not to apply. I also feel inclined to protest, from the point of view of an old member of the Standards Committee, against any attempt to suggest that design can be standardised by laying down hard and fast rules which are practically only based on existing practice and not on the practice which future requirements may call for. I do not make this as a hostile criticism to the paper, but simply to point out that in my view the author is premature in laying down these rules of proportion, and that he could not do so without standardising the design to an extent which I believe would be hurtful to the progress of our industry. The President.

Dr. Goldschmidt (*in reply*): When attempting to standardise the efficiencies, power factors, etc., or rather to give figures which I considered normal for the performance of electrical machinery, I could not start from a certain design, from a certain ratio of diameter to length of armature, and the like. The majority of manufacturers design on the expansion system—that is to say, they have a very narrow machine of a certain diameter, a second type somewhat broader, and a third type broader still, but with the same diameter, so that the machines of a single manufacturer have a great number of ratios of diameter to length which vary 100 per cent. or more. The efficiencies vary accordingly. By drawing a curve of the efficiencies for machines of a certain manufacture it will be found that it rises steadily if the core length is increased. Dr. Goldschmidt.

As soon as the diameter is altered there is a break in the curve. Consequently there does not exist a uniform curve for the efficiency as a function of kilowatts per revolution. If an average line is drawn the efficiencies which I have given are arrived at.

Perhaps a manufacturer prefers narrower or broader machines for some reason or other. Then the curves will be modified, and they correct themselves in use. Any one who employs these curves will find from his own practice if the actual figures from his own experience come out lower or higher. Then my curves simply serve him as a guide. They will be of special assistance to the mechanical engineer, who cannot go into the details of design at all, and who has not made up his mind where to buy his machine, or whether he is to get a broad one or a narrow one, but wants to know beforehand what the performance of his machine is likely to be. I think I should not have got any results at all if I had gone to the length of introducing details of design into this paper.

I have given flux curves, and also curves for the ampere-wires of the armature, for the purpose of showing approximately how a machine is built nowadays. In curve 4 I am trying to show that the influence the flux has on the total loss is not very great, and that one may vary it a great deal without actually affecting the total efficiency. Dr. Thomp-

Dr. Goldschmidt.

son asked which of these efficiency curves have been developed from first principles, and which from actual practice, or from practical averages. I may say that, for sake of uniformity, all of them have been based on first principles, but these principles are derived from practical experience. I have taken average flux curves and ampere-turn curves as a guide, and then I have designed machines on this basis. Machines designed by other people have also been taken into consideration, in order to show results of the experience of others besides my own, and I have then arrived at figures for the efficiencies and the power factors which I consider to be normal. Consequently they are developed from first principles and at the same time from actual experience.

With regard to the curves for single-phase commutator motors, Mr. Fynn thought that the efficiency curves were rather too low. One can get efficiencies better by a few per cent. by employing a more liberal design, by making a cooler motor. It all depends on the temperature. If material is wasted and more copper provided than is actually necessary, the efficiency can be raised, but I think it will be found that these average efficiencies are about right, perhaps slightly on the low side. The efficiencies put forward by leading manufacturers of commutator motors do not differ much from my figures. To make distinctions between the different classes of commutator motors is far beyond the scope of my paper. The curves given refer to series conduction motors.

Dr. Thompson pointed out that the curves for the flux and the ampere-wires were parabolic. I have not gone into that question. I have not assumed them to be parabolic, but it is very interesting to me to hear that they are. Mr. Macfarlane asked why the curve of the flux for the induction motor is not a parabola. I cannot answer, because I did not know that the first one was. I simply plotted these curves in order to give averages according to my own experience. Moreover, I do not see why the law governing the amount of flux to be employed should not be quite different with induction motors than with direct-current machines.

I did not say that my standard fluxes are the most economical fluxes, but those which I assumed and on which I based my calculation. If a manufacturer makes machines with *entirely* different fluxes and ampere-turns, he may develop for himself "standard performances," and I hope that my paper will be useful to him merely as a suggestion on what lines this can be done.

I wish to emphasise here again that the object of the paper will have been misunderstood if, in order to prove something, a design or other is quoted having ratios $\frac{\text{Flux}}{\text{A.-wires}}$ differing considerably from those put forward by me. Even with widely divergent ratios the normal efficiencies will not be very much different from those given in the curves, as Mr. Macfarlane admits himself in his contribution to this discussion.

With regard to the output formula:—

Dr. Goldschmidt.

$$\text{“Power”} = \text{K W at 1,000 revs.} = \text{const.} \times \text{amp.-wires} \times \text{flux}$$
$$= \text{const.} + \text{AW} \times \text{F}$$

where "power" means the kilowatts per 1,000 revs. or watts per rev., I think that the expression "powerage" suggested by Dr. Thompson is a very good one. Dr. Thompson has shown that the constant factor in this formula is something quite definite, but, as Mr. Fynn pointed out, it depends on what is meant by the flux F. The constant is with direct-current machines always 0·166, if F denotes the *resultant* flux. If F is the *no-load* flux the constant is somewhat smaller. With single-phase machines it is smaller still, due to the influence of the winding constant appearing in the formula for the E.M.F. In complicated cases, as with alternating-current commutator motors, where the armature carries exciting current, with alternators producing wattless current, and direct-current machines having demagnetising ampere-turns in the armature, the formula must be interpreted and applied cautiously, excluding the "wattless" portion of the current.

In reply to Mr. Burge I may say that for calculation of temperature rise it is not correct simply to add the total losses together, those in the core and those in the winding, as the influence on the heating of either part of the stator must be taken account of in a quite different way. The end connections of the winding may be quite cool and the core hot, or the reverse may be the case. So the explanation for the well-known phenomenon mentioned by Mr. Burge lies in the incorrect way of forming the "total loss." From the same reason follows the incorrectness of Mr. Burge's opinion that the same copper loss is permissible if the number of poles is increased, and that therefore the copper loss as a percentage of the output increases proportionally to the number of poles.

I quite agree with the President that standardisation ought not to prejudice manufacture, and that it ought not to be hurtful to industry. This end will be attained if standardisation follows manufacture (not leads it), and gives results and facts relating to its present state. Perhaps in four or five years' time new standards will have to be worked out, if sufficient progress has been made to make it worth while. Its object is to assist the buyer and generally those people who wish to use our machines but do not know what qualities they have. I anticipate only beneficial effects for all parties concerned from detailed standardisation—that is, statements of facts carefully kept up-to-date—and I hope that before long it will be carried farther than I have done, in spite of the work involved in this task.

On the motion of the President, a hearty vote of thanks was accorded to Dr. Goldschmidt for his valuable and interesting paper.

The meeting adjourned at 9.15 p.m.

Proceedings of the Four Hundred and Sixty-ninth Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, February 6th, 1908—Colonel R. E. Crompton, C.B., President, in the chair.

The minutes of the Ordinary General Meeting held on Thursday, January 23rd, were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members :—

Frank Fairley.	Andrew F. Rock.
Francis J. Moffett.	Chas. F. Smith.
Theo. C. Parsons.	Chas. P. Taylor.
Harry L. Riseley.	Roger H. Willis.

Harry E. Yerbury.

From the class of Associates to that of Associate Members :—

Henry F. Jay.	Stuart Roseveare.
Walter E. Nicoll.	John E. Schofield.
J. H. S. Phillips.	Thomas Wadsworth.

From the class of Students to that of Associate Members :—

John D. Billington.	Maurice J. Penford.
William G. H. Cam.	Walter Pintner.
H. G. Y. Crowder.	W. A. Ritchie.
Ernest C. Handcock.	Leonard Roseveare.
William Hanna.	K. W. Sutherland.
G. L. Kirkpatrick.	James Summers.
Thos. H. Langford.	Albert Williams.

From the class of Students to that of Associates :—

Harold R. Lloyd.

Messrs. B. S. Cohen and W. W. Cook were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

As Member.

Dr. E. Rosenberg.

As Associate Members.

Burdett Wyke Bayliss.	Herbert Maurice Fulton.
Herbert Moxon Boydell.	Ernest William Heather.
Lawrence George Clark.	Sydney Morse Hopewell.
John Edward Elliott.	Albert Horace Leopold Lucas.
H. Nigel Fullarton.	John Muir McGown.

Cecil Robert Whipple.

As Students.

Harley Autton-Carter.	Vernon Kilvert.
Harry Alexander Bailey.	Nathaniel Martin.
George Francis Barbour.	Arthur Mizzi.
Basil G. Bellamy.	Brendan Francis Mulholland.
Douglas Betts.	Stanley Griggs Nottage.
John Randolph Boyer.	Henry John Kyle Osborn.
Reginald Vernon C. Brook.	Herbert Leslie O. Parish.
William Brown.	Charles Alexander Pilson.
James Cunliffe.	Frederick Herbert Pullum.
Arnett Richardson Dunton.	Alec. Walker Puttick.
Walter Gordon Edwards.	Joseph Phillip Quinn.
Alexander Gardner.	Norman Stuart Sim.
Surendranath Ghosh.	Leonard Solomon.
Randolph Douglas Gifford.	Harry Arthur John Stanton.
Austin Yorke Jarrett.	Maurice Swift.
Lauret Marshall Jockel.	Fred. Edward Windross.
John H. C. Kann.	Maurice William Wood.

Donations to the *Library* were announced as having been received since the last meeting from The National Brake and Electric Company, T. Sewell, J. S. Warren, Messrs. Whittaker & Co. ; to the *Building Fund* from A. von Boschan, R. H. Burnham, H. J. Eck, S. Evershed, J. C. Smail, J. M. Smyth, Sir J. W. Swan, H. W. Young ; and to the *Benevolent Fund* from O. H. Bishop, Lord Blythswood, H. Codd, S. Evershed, H. W. Kolle, C. C. Paterson, F. W. Topping, to whom the thanks of the meeting were duly accorded.

The following paper was read and discussed :—

PROTECTIVE DEVICES FOR HIGH-TENSION TRANSMISSION CIRCUITS.

By J. S. PECK, Member.

(*Paper received from the* MANCHESTER LOCAL SECTION, *January* 3, *read in Manchester on February* 4, *in London on February* 6, *and in Dublin on February* 13, 1908.)

Comparatively little has been done in Great Britain toward the study and development of apparatus for protecting high-tension transmission systems against lightning or other high-voltage discharges. At least three reasons may be assigned for this apparent lack of interest:—

1. There are very few systems working at pressures above 11,000 volts.
2. The great majority of transmission systems are laid under ground, and are therefore not subjected to lightning disturbances.
3. Severe thunderstorms are of very rare occurrence.

The question of protective apparatus for high-voltage transmission systems is, however, one which even in Great Britain deserves careful consideration, for it is probable that voltages much higher than 11,000 will be used in future, and that overhead transmission will come into more extensive use both for present voltages and for higher ones, and although climatic conditions are not likely to become more severe, static disturbances are not produced by lightning alone, but by switching, grounding, short-circuiting, etc. Underground cable systems are particularly subject to such disturbances on account of their great static capacity as compared with overhead systems.

In America and on the Continent, where very high voltages are used, where overhead transmission systems are employed extensively, and where thunderstorms are extremely severe and of frequent occurrence, the development of protective devices has been essential to the success of long-distance transmission. In America especially, where exceptionally high voltages are used, and where the transmission distances are very great, these devices have been carried to a high state of perfection. Much study has also been given to the laws governing static disturbances, and much which was shrouded in mystery is now clearly understood. There are, however, many points both theoretical and practical still in dispute, which will not be touched upon in this paper, as it is intended here to explain only a few of the

simpler phenomena and to describe some of the protective devices now in use. Several engineers have taken a very active interest in this field of work, and have devoted a great deal of time to the study of static phenomena.* Among them the names of Wurtz, Elihu Thompson, Steinmetz, and Thomas stand out prominently, but Thomas was probably the first to take up the study of these phenomena in a scientific way, and to apply his knowledge to perfecting protective devices. His classic paper † is remarkable not only on account of the amount of new information it contains, but for the clear and simple manner in which difficult problems are handled. In this paper was pointed out and explained the concentration of potential on the end turns of electrical machines during switching, the rise of potential at the end of a transmission line, or where branch circuits join the main circuit, and numerous other phenomena. A careful study of this paper will amply repay any one interested in the subject of static phenomena.

Static Disturbances.—Static disturbances may be produced in a number of different ways: namely, by switching, short-circuiting,

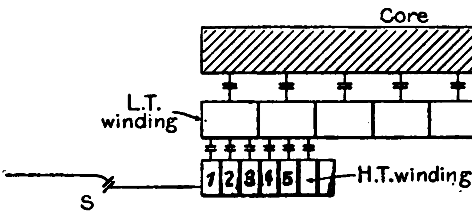


FIG. 1.

grounding, and lightning, or in any other way which produces a very abrupt change in potential. The distinguishing features of static disturbances are their oscillatory nature and their extreme suddenness of action. In the case of lightning discharges, the frequency may be thousands or even millions of cycles per second. In the case of switching, grounding, etc., the changes in potential are practically instantaneous on those parts of the circuit nearest to the point where the grounding or switching occurs.

Concentration of Potential.—One of the best-known effects due to static is the concentration of potential upon the outer turns of the windings of electrical apparatus. Fig. 1 shows diagrammatically the high-tension and low-tension windings of a transformer. Each turn of the high-tension winding has a static capacity to the low-tension

* The term "static" as applied to phenomena which are distinguished chiefly by the extreme suddenness with which they act seems certainly a misnomer, and it has been proposed to call them "lightning phenomena," distinguishing between cloud lightning and lightning due to switching, etc., by the terms "external" and "internal"; but as this point has not yet been settled, and as "static" is the term in most common use, it will be used in this paper.

† *Transactions of the American Institute of Electrical Engineers*, vol. 19, p. 213, 1902.

winding and to ground. Each turn also has a certain amount of inductance. S represents a switch, by means of which one outer turn of the high-tension winding is connected to the line circuit. Before switch S is closed both windings are at zero potential. When switch S is nearly closed a spark passes from the line to turn No. 1 at the instant of maximum line voltage. This turn is immediately brought up to line potential, and turns 2, 3, 4, etc., follow it rapidly, but no turn can reach full potential until the condenser connected to it is fully charged. For example, turn 3 cannot reach its full potential above earth until the condensers connecting it with earth have been fully charged, but the current to charge these condensers must pass through turns 1 and 2, and since these turns possess an appreciable self-induction, an appreciable time is necessary for the current required to charge these condensers to flow through turns 1 and 2. Thus for an extremely short period of time the full potential of the line will be concentrated upon a very few of the outer turns of the transformer. In a 10,000-volt transformer there may be normally a difference of potential of, say, 40 volts between turns. One of the line terminals is

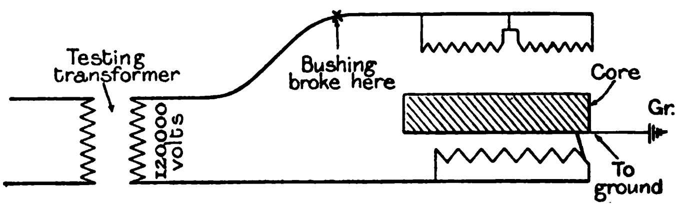


FIG. 2.

5,000 volts above earth potential, and the other 5,000 volts below. At the instant of closing the positive line switch the full potential of the line above earth, *i.e.*, 5,000 volts, may be concentrated upon a half-dozen turns, thus giving, perhaps, 500 volts or more between individual turns, and unless very heavily insulated a spark may pass between them. In a small fraction of a second the whole of the high-tension winding will have assumed the potential of the line. If, now, the other terminal of the winding be connected to the negative side of the circuit a similar action takes place, except that whereas in the first case the high-tension winding was at earth potential it is now 5,000 volts above earth, so that when the positive end is connected to earth the strains are approximately double what they were when the first switch was closed, and there may be over 1,000 volts per turn where there should be but 40 normally.

This effect is produced not only by switching, but by any action which causes a very abrupt change in potential of the end turns of a transformer. For example, if one terminal is 5,000 volts above earth, and is suddenly earthed, the first turn is reduced instantly to zero potential, and there is the same concentration of potential as in the

case of switching the transformer on to the line. A static charge produced by lightning, surging along the line and meeting the transformer windings, produces a similar effect.

This effect of concentrated potential was clearly shown in the case of a large high-voltage transformer which was under test. The transformer was connected as shown in Fig. 2, and 120,000 volts applied between three points of the high-tension winding and the low-tension winding which was connected to the core. While the voltage was being applied a bushing on the testing transformer punctured and grounded the terminal connected to the high-tension winding. The bushing was replaced, the test repeated, and the insulation proved to be satisfactory. As the transformer had been tested previously under full load, it was prepared for shipment, but as it was desired to check the iron loss, voltage was put across the terminals, when it was found that the transformer was short-circuited. When dismantled it was found that a static discharge had passed over several turns adjacent to the three points where the testing transformer was connected to the high-tension winding. This discharge had punctured the insulation between adjacent turns and short-circuited them, but caused no other damage whatever.

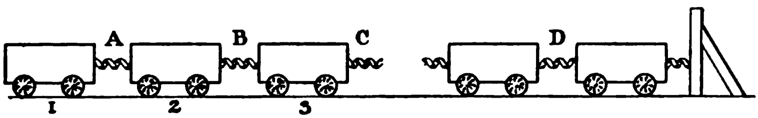


FIG. 3.

This concentration of potential may occur in a high-voltage motor or generator in exactly the same way as explained above in the case of a transformer; in fact, high-voltage motors and generators are particularly subject to damage from this cause on account of the great difficulty of insulating the individual turns from each other. Transformers are much easier to insulate, and, in general, it is not necessary to take any special precautions in switching transformers wound for voltages not exceeding 11,000, but for very high voltages special precautions must be taken. The voltage between turns depends upon the suddenness of the discharge and the capacity and inductance of the windings. It should be noted that this concentration of potential has no tendency to cause a breakdown to earth, but to produce short-circuits between adjacent turns. It is one of the most frequent sources of trouble in high-voltage apparatus.

This concentration of potential may be illustrated in a rough way by means of a mechanical analogy. Fig. 3 represents a line of wagons separated by springs. If wagon No. 1 is moved slowly toward the right for a certain distance, spring A is slowly compressed; wagon No. 2 begins to move toward the right, spring B is compressed; wagon No. 3 begins to move, etc. The whole movement is a gradual one, and no excess pressures are brought on any of the springs between the wagons.

The wagons will come to rest with a slight and equally divided compression upon all the springs. If, however, wagon No. 1 is moved with great suddenness to the right, spring A will be suddenly compressed, wagon No. 2 will begin to move, but on account of its inertia it cannot get under way immediately. As it does begin to move, spring B is compressed, but wagon No. 3, because of its inertia, cannot be set in motion instantly. Therefore, for a short period of time, the full compression represented by the movement of wagon No. 1 will be concentrated on the springs between two or three of the wagons. The extent to which the springs between the first few wagons will be compressed depends upon the suddenness with which wagon No. 1 is moved, upon the flexibility of the springs, and upon the inertia of the wagons. It is evident that the spring compression moves on from one wagon to the other until it reaches the end where it is reflected, and the wagons oscillate back and forth until they all come to rest. In this analogy the springs correspond to capacity and the inertia of the

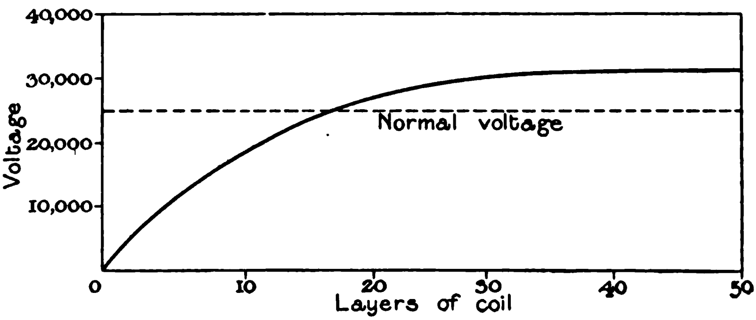


FIG. 4.

wagons to inductance, though the distribution is not the same as in the transformer winding.

Fig. 4 shows a curve illustrating this concentration of potential on the end turns of a transformer.

Reflected Waves.—If a condenser is fully charged and then discharged through a reactance, the current will increase until the condenser is entirely discharged, when the whole of the energy will be stored in the reactance; but the energy cannot remain thus stored, and the reactance begins to discharge into the condenser, which is charged to its original potential, but in the opposite direction. The condenser cannot remain charged, but again discharges into the reactance, so that the charge oscillates between reactance and condenser until it is used up in ohmic loss and leakage.

Referring again to the wagon analogy, if the wagon in Fig. 5 is moved slowly toward the right until the spring A is under 100 lbs. pressure and then released, the wagon will begin to move toward the left with increasing velocity. The velocity will reach a maximum when

the spring is in its normal position, but the inertia of the wagon will carry it to the left until the spring is under 100 lbs. tension. The spring will then begin to move the wagon to the right, and the cycle will be repeated until the energy originally imparted to the wagon has been absorbed in friction.

If a condenser is without charge, and we charge it from a constant potential line through a reactance, current flows into the condenser through the reactance at an increasing rate, which reaches a maximum when the condenser reaches line pressure. The current through the reactance cannot stop, however, but continues to flow into the condenser until it is charged to double-line pressure. The condenser, however, cannot remain at double potential, but discharges through the reactance into the line, and continues to oscillate between zero and double-line pressure until it finally comes to rest at line pressure. Thus it is possible to obtain double the original pressure on a condenser when charging it through reactance. If in Fig. 5 we push against the wagon with a steady pressure of 100 lbs., it will move toward the right with increasing velocity, which reaches a maximum when the spring is under 100 lbs. pressure, but the inertia of the wagon carries it forward

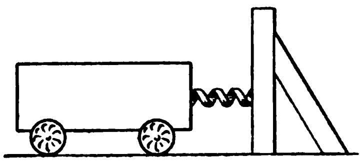


FIG. 5.

until the spring is under 200 lbs. pressure. The spring then forces the wagon to the left until the spring is under zero compression. The wagon then starts again toward the right, and the cycle is repeated until it finally comes to rest with the spring under 100 lbs. pressure.

A transmission line is made up of condensers and reactances in series. If an unloaded line is connected at one end to a constant potential source, a charge passes into the line and advances at a certain rate depending upon the constants of the line. When it reaches the open end of the line it can go no further, but on account of the inductance of the circuit the charge is increased until approximately double potential is reached at the end of the line (somewhat as double pressure was put upon the spring in Fig. 5), but the end of the line cannot remain at double potential, and the charge is reflected back along the line, bringing it to double potential. The next wave reduces it to line potential, and the reflected wave to zero potential. Thus the line oscillates from zero to double-line potential until it finally comes to rest at line potential.

The phenomena just described may perhaps be understood better from a water analogy. If we have a long, narrow trough containing water, and displace the water at one end so as to start a wave along

the trough, the wave progresses until it strikes the end of the trough. Here it reaches double its original height and is reflected back along the trough.

It is not necessary that the line should be open-circuited in order to obtain a reflected wave and a potential rise, for if there is a branch line at the end of the main line and the branch has less capacity than the main line, it will be unable to absorb the charge as rapidly as it arrives at the junction, and a rise in potential will occur. Thus, if there is a circuit made up of underground cables and overhead wires, and a potential is impressed on the far end of the underground circuit, a charge will travel along this circuit until it reaches the overhead conductors. As their capacity is much less than that of the underground circuit, a high voltage will occur at the junction. The maximum possible pressure at this point is double the line pressure, but it is probable that this maximum value is very seldom reached. The high potential at the juncture passes along the overhead circuit, and is reflected at the open end, where it may again be doubled, thus giving as a maximum four times the original line pressure.

Resonance.—If the wagon and the spring in Fig. 5 are set in motion they will oscillate back and forth at a certain definite rate which will be determined by the inertia of the wagon and the flexibility of the spring. If we start them oscillating, and every time the wagon moves to the right give it a push in that direction, and every time it moves toward the left give it a push in that direction, the travel of the wagon and the compression and extension of the spring will gradually increase until the spring breaks, or until the resistance to further movement is greater than the push given at each oscillation. The push may not come every oscillation, as the same effect is obtained if it occurs every second or third, or as long as the push is in the same direction as that in which the wagon is moving. A similar condition may occur on a transmission line which is made up of condensers and reactances in series. Every line has a natural period of oscillation, and if the frequency of the generator is such as to increase the magnitude of the oscillation, resonance occurs, and there is practically no limit to the voltage which may result. Fortunately the frequency of oscillation of a transmission line is so high that there is very little chance of resonance with the generator frequency.

On very long lines there is, however, the possibility that there may be resonance with some of the higher harmonics, and for this reason it is desirable that the generator should deliver an E.M.F. wave which has as nearly as possible a sine-form.*

* The natural frequency F of a circuit—

$$= \frac{1}{2\pi\sqrt{LC}}$$

where L is expressed in henrys and C in farads. For an ordinary transmission line 20 miles long, L may be taken as 0·075 and C as $0{\cdot}16 \times 10^{-6}$.

Therefore—

$$F = \frac{1}{2\pi\sqrt{\frac{0{\cdot}075 \times 0{\cdot}16}{10^6}}} = 1{,}600 \sim \text{per second,}$$

The static disturbances described above may be said to be caused by internal workings, but the most severe strains to which apparatus is subjected are due to an external cause—lightning.

Lightning.—There are three ways in which lightning may affect a transmission line:—

1. By static induction.
2. By magnetic induction.
3. By direct stroke.

If a charged cloud approaches a transmission line, a charge of opposite sign is induced by static induction on the wires, the charge of like sign being repelled and leaking gradually from the wires to earth. If the cloud is suddenly discharged by a stroke to earth the line wires are left charged to a high potential, and this charge tries to escape to earth. If the line is well insulated, so that the charge cannot reach ground over the insulators, it rushes along until it reaches the apparatus connected to the line, and if there is a weak spot it jumps to earth at this point. If the insulation is strong everywhere in the system, or the potential of the charge not sufficiently high to jump to earth, it oscillates back and forth on the wires until dissipated in ohmic losses and leakage.

If a lightning discharge occurs parallel to a transmission line, the field set up by the discharge induces a charge in the wires by magnetic induction. As the lightning stroke is of an oscillatory nature, the charge on the wires is of the same nature, and either jumps to ground or oscillates back and forth until its energy is dissipated.

When a direct lightning discharge strikes a transmission wire it usually jumps to ground over the nearest insulators, often destroying the pole. Many cases have been known where a number of poles have been shattered by a single stroke. In the case of direct stroke the apparatus in the station may be uninjured, as the discharge to ground over the insulators is the means of saving the station apparatus.

It should be borne in mind that these lightning discharges are characterised by their extreme suddenness and by their oscillatory nature. Their frequency is enormous as compared with that of the highest frequency alternating systems in operation. At these extremely high frequencies circuits which offer practically no inductance to currents of normal frequencies become highly inductive, and capacity effects are correspondingly increased.

It has been found also that in dry climates wind blowing over the transmission wires, or other climatic conditions, will gradually build up a high static potential which may cause damage to the apparatus. The best way to relieve the line from charges of this nature is to connect a high resistance between wires and ground through which the static may escape to earth.

It is evident from what has been said that, regardless of the source of the static disturbance, there are at least two dangers to electrical apparatus on transmission systems:—

1. Concentration of potential on the end windings, causing shorts between adjacent turns.

2. Excessive voltage between wires and ground, causing breakdowns to ground over insulators, bushings, or through the insulation of the windings.

In order to prevent trouble from the first cause, two courses are open—

(*a*) To insulate the end windings so that they will withstand the strains. This method is often used, but it is always expensive, and on certain types of apparatus very difficult to apply.

(*b*) To prevent the windings being subjected to the abrupt change of potential. In the case of switching, danger may be prevented by closing the circuit through a resistance or adopting certain other means, but the most effective remedy, and one which is good for practically all cases, is to place choke coils between the apparatus and the lines from which the static disturbance comes. With this arrangement the shock is taken by the choke coils, which can be heavily insulated to withstand it, and if the coils should break down they are readily repaired or replaced. The use of a choke coil may be said to be the same (so far as the static strains are concerned) as placing the end turns outside the apparatus, where there is plenty of room for insulation. The effect of a choke coil in reducing strains on the end windings of a transformer is shown in Fig. 6, where the voltages across different numbers of turns near the line are measured with and without a choke coil.

The greater the reactance or choking power of the coil, the greater is its retarding effect upon the discharge, and therefore the greater its protective power. On the other hand, the greater is its cost, and the greater drop it introduces into the circuit. In general, however, a very fair degree of protection may be obtained from a coil of comparatively low cost and with negligible drop. Two types of choke coils are shown in Figs. 7 and 8.

In order to prevent trouble from the second cause, that is, high voltage to earth, the lightning arrester is used.

Lightning Arresters.—The lightning arrester is intended to afford a path to earth for static discharges of lower resistance than that offered by the insulation of the transmission line or of the apparatus connected to it. The requirements for a good arrester are conflicting. It must offer an easy path to earth, yet must be able to hold back the line voltage, and when a discharge occurs it must be able to interrupt or suppress the arc which follows the discharge. All arresters, except the water-jet type, consist of at least one air-gap in series with an arc-suppressing device. In the case of alternate-current arresters there are often a very large number of gaps in series.

Horn-type Arrester.—A great many different types of arresters have been developed from time to time. The earliest form was probably the horn type, which consists of two wires, or rods, each bent at an acute angle, the two being placed in such a position that the air-gap is

small at the bottom, but increases rapidly from bottom to top. One horn of the arrester is connected to line and the other to ground, and the arrester so adjusted that the gap at the bottom is just large enough to withstand normal or slight increases above normal voltage. When there is an excess voltage an arc passes across the small part of the gap, and due to its heat rises until it is drawn out so long that it is ruptured. The advantage of this type of arrester is that it is cheap and can be installed out of doors.

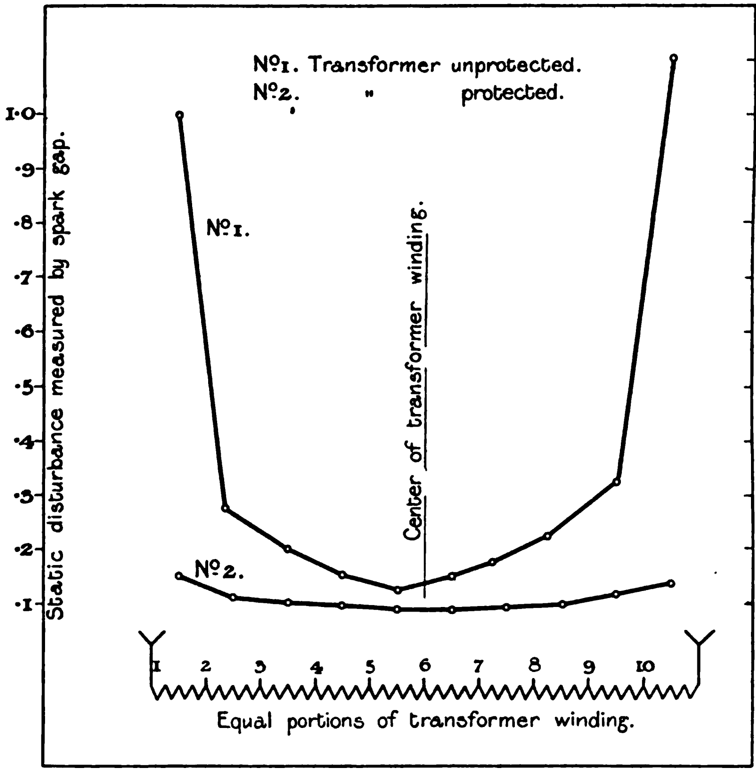


FIG. 6.—Curve showing Protection afforded by Choke Coils.

The objections are that the arc takes a long time to break, and, since it constitutes a short-circuit on the system, will throw synchronous apparatus out of step. It is now customary to instal a high resistance in series with this arrester in order to limit the current which flows on short-circuit, but this resistance retards the static discharge. This arrester requires a much higher resistance in series with it than certain other types. This form of arrester is shown in Figs. 9 and 10.

Non-arcing Multi-gap Arresters.—It was discovered by Wurtz that

certain metals had the property of suppressing alternating arcs. The discovery was made by inadvertently short-circuiting two brass cylinders spaced about $\frac{1}{32}$ in. apart, and connected to the opposite sides of a 1,000-volt circuit. A spark passed between the two cylinders, but instead of destroying them there was only a slight spark and no serious burning. Experiments were made to determine how great was this power of suppressing an arc, what its limitations were, and what metals possessed it. A large number of experiments were carried on with different metals, and the best one was adopted for the manufacture of non-arcing metal arresters. This arrester consists of a number of small metal cylinders separated by air-gaps of approximately $\frac{1}{32}$ in. (Fig. 11). The number of gaps between line and ground is made great enough to suppress any arcing which occurs after a static discharge. This arrester proved a great success on circuits of 2,000 volts, and was

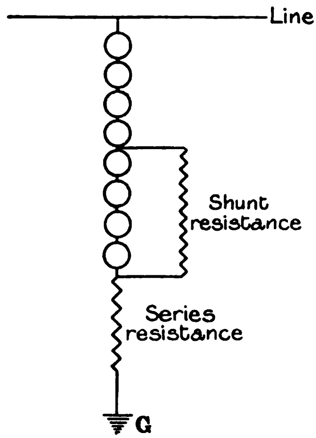


FIG. 12.

used to some extent on voltages as high as 15,000 and 20,000. It was found, however, that on large generating systems, and particularly with high voltages, the arresters did not always retain their non-arcing quality, or else that an enormous number of gaps were required. At this time Thomas began his investigations with a view to producing an arrester which would be satisfactory on circuits of any voltage and of any capacity. He discovered that in order that an arrester should be non-arcing it was necessary to limit the amount of current which flowed after the arrester discharged. This at once suggested an ohmic resistance in series with the gaps, but on high potential circuits the insertion of sufficient resistance to render the arrester non-arcing increased greatly the resistance which the arrester offered to static discharges. He found, however, that the resistance of the arrester to static discharges could be greatly reduced by using a

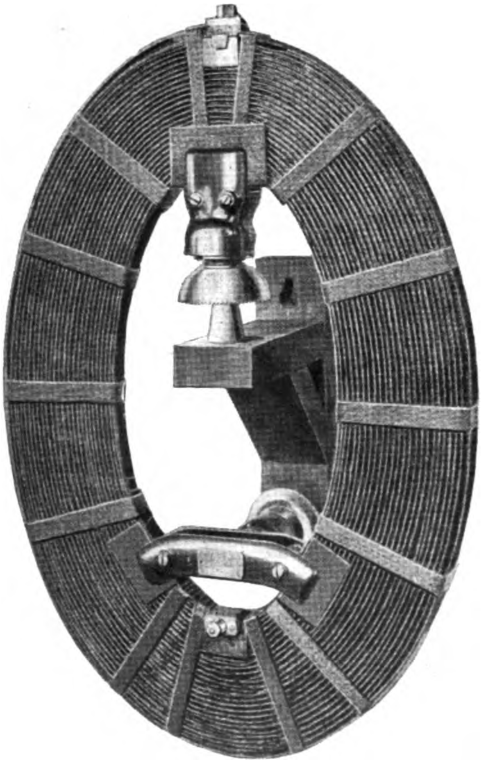


FIG. 7.—Choke Coils for Alternating-current Circuits.

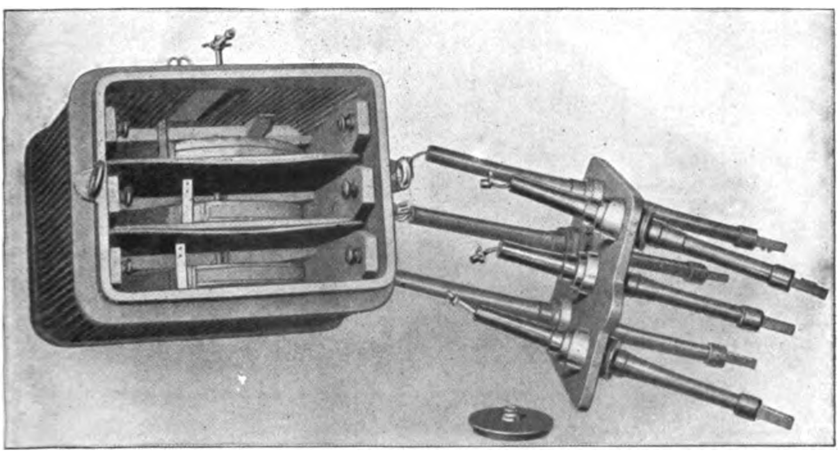


FIG. 8.—Triple-pole Oil-cooled Choke Coil, showing Coils in **Tank.**

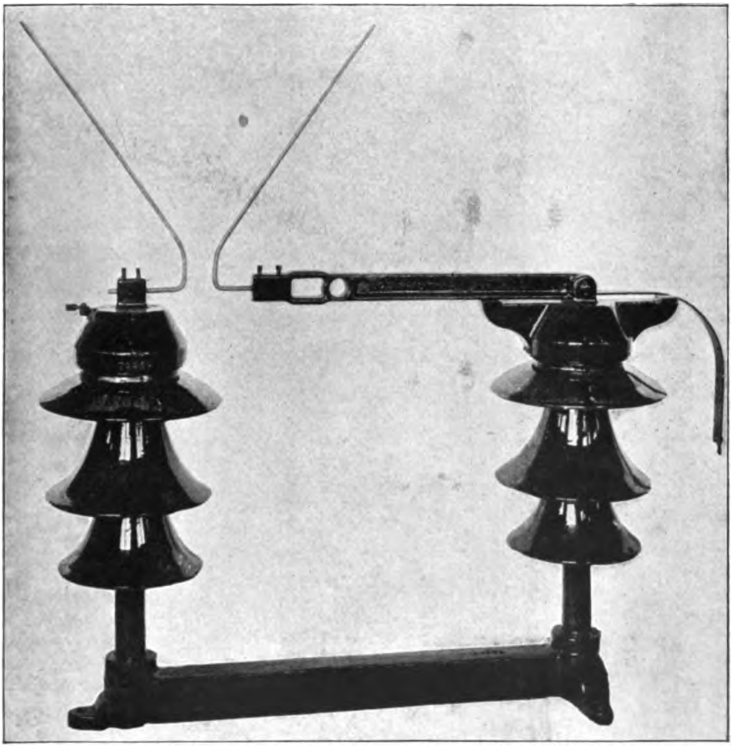


FIG. 9.—Horn-type Gap.

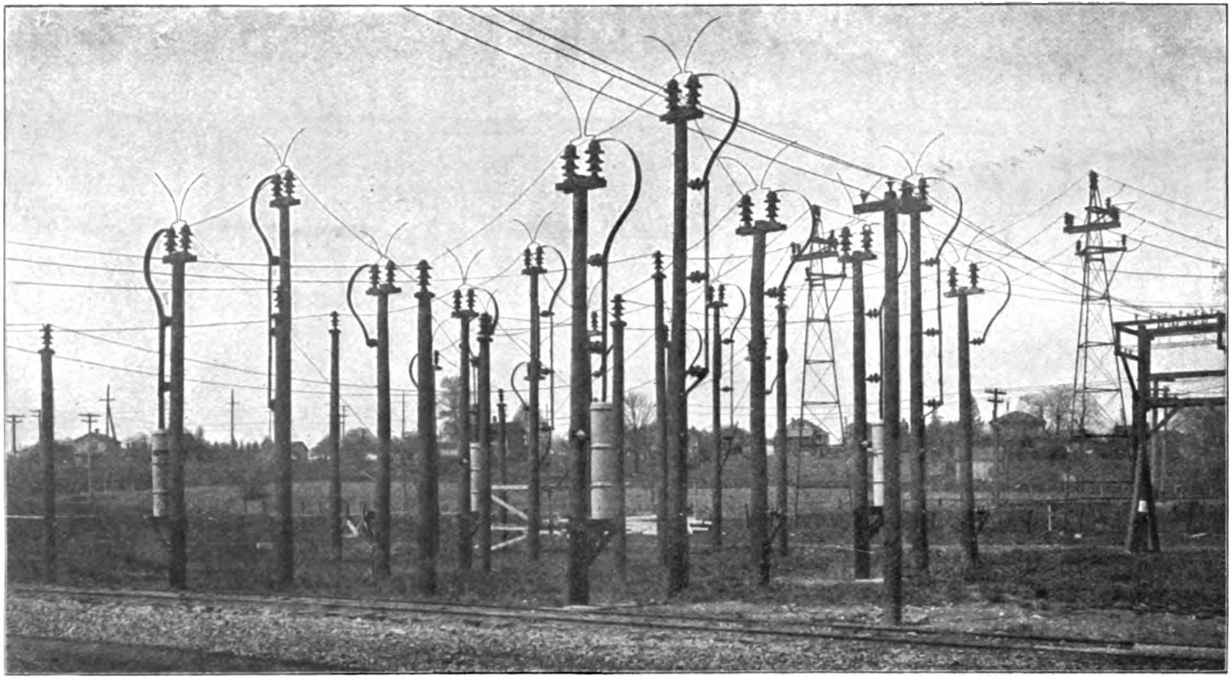


FIG. 10.—General View, 60,000-volt Lightning Arresters.

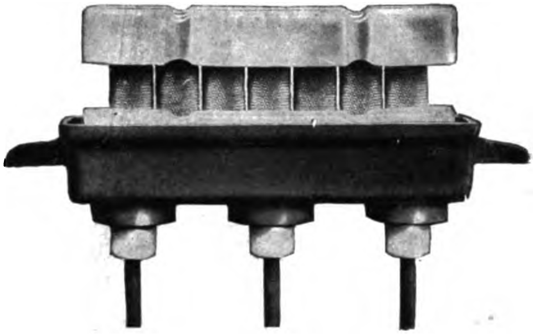


FIG. 11.—Non-arcing Metal Arrester Unit.

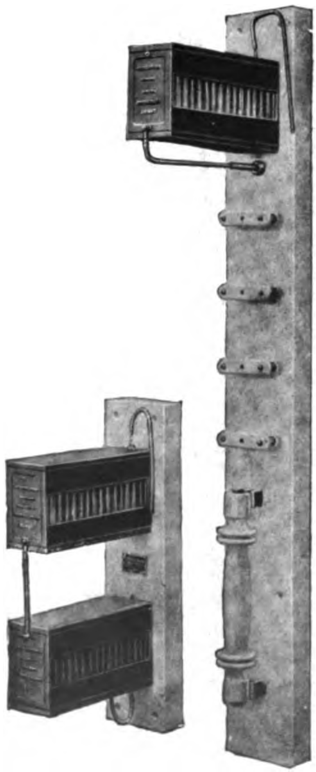


FIG. 13.—8,000-volt Low Equivalent Lightning Arrester.

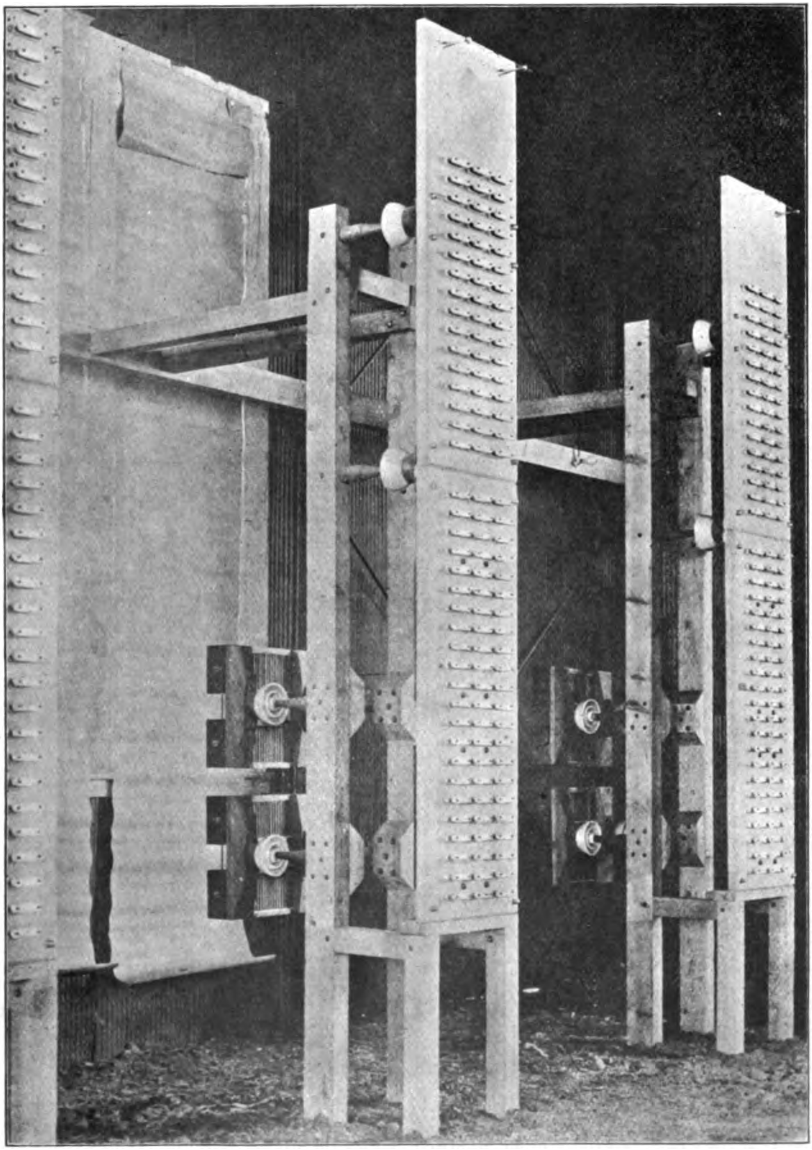


FIG. 14.—Low Equivalent Lightning Arrester, 40,000 Volts.

large number of gaps and comparatively small series resistance, and shunting part of the gaps with an ohmic resistance. This he called a low equivalent arrester, the general appearance of which is shown in Figs. 12, 13, 14. The action of the arrester is as follows :—

Approximately twice as many gap units are taken as are required to hold back the line voltage. Approximately one-half of the gaps are shunted with an ohmic resistance. When no discharge occurs the ground potential is brought up to the middle of the series. Thus there are between line and ground only one-half the total number of gaps. When there is an excessive potential on the line, the non-shunted gaps break down first, so that full potential is thrown across the shunted gaps, and they also break down. The arc through the shunted gaps is, however, very unstable and quickly drops out, the current passing through the shunt resistance, but the non-shunted gaps now have in series with them the shunt resistance and the series resistance, so that the current passing through them is very small and the arc is quickly suppressed. Although the two series of gaps do not break down simultaneously, the whole action is, of course, very sudden and appears instantaneous. This arrester has been very successful, and the principle with various modifications is now used on practically all arresters of the multi-gap type.*

One objection to the multi-gap arrester is that on high voltages an increase in the number of gaps out of all proportion to the increase in voltage is required in order to hold back the line voltage and to make the arrester non-arcing. It was found also that the number of gaps varied greatly on different circuits, and this difference was finally traced to the position of the arrester with reference to grounded objects. The explanation of the phenomena is believed to be as follows :—

When ground potential is brought near the arrester, the potential gradient across the gaps near the line becomes so high that the air between the cylinders becomes ionised, and the line potential is gradually moved towards the grounded end of the arrester until there are not sufficient gaps to hold back the line voltage and a discharge occurs. When the arrester is well removed from grounded objects, *i.e.*, from the walls of buildings, etc., the total potential strain from line to ground is divided more equally between the cylinders, consequently the arrester will stand a higher potential, or a smaller number of gaps are required for holding back the same potential.

With high-potential arresters a hissing sound is often noticeable. This is caused by static sparks which pass continuously from the line wire to the cylinders which are adjacent to them. The closer the arrester is to grounded objects, the further will this display extend downward toward the ground from the line terminal. To overcome

* Recently a number of low equivalent arresters have been installed in which the series resistance is shunted by a fuse in series with a spark-gap. Under very severe discharges the fuse blows, showing that the voltage across the resistance is high enough to jump the gap, while the melting of the fuse shows that a heavy current has passed and afforded relief to the system.

this unequal distribution of potential across the gaps, a metallic ground shield connected to the line is placed adjacent to a portion of the gaps furthest from the ground, thus the line potential is brought near to these gaps, and the excessive potential gradient removed. This arrangement is shown diagrammatically in Fig. 15. It is found in practice that the use of the shield often permits a great reduction in the number of gaps to be made.

Multiplex Arrester.—While the lightning arrester is intended primarily to afford an easy path to earth for static discharges, it has been found that a high static potential sometimes exists between the different wires of a transmission circuit. To take care of this, the General Electric Company of America brought out the multiplex arrester. In this arrester, which is of the multi-gap type, an ohmic resistance is connected from about the middle point of the series of gaps in one phase to the corresponding point of the arrester in the next phase. Thus all the arresters are connected together by a high ohmic resistance at approximately their middle points, and it is possible for a static discharge to pass half-way through one arrester, through the ohmic resistance, and through half the number of gaps in

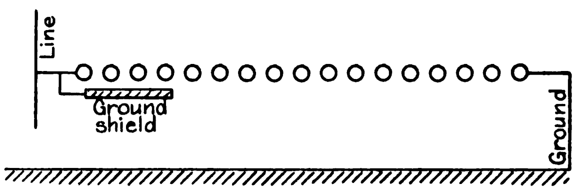


FIG. 15.—Arrester Units with Earth Shield.

the other arrester to the opposite side of the circuit. Service tests indicate that this arrangement is sometimes of advantage, though practically the same result is obtained by the arrangement of the low equivalent arrester.

Water-jet Arresters.—On the Continent an arrester consisting of jets of water playing upon the wires has been used to some extent. The chief objection to this type is that it causes a leakage of current to earth, which represents a loss of power, and if the resistance is sufficiently high to prevent serious loss due to leakage, it may offer considerable resistance to static discharges. It is, however, comparatively cheap, and appears to have given satisfactory protection, and there is no reason why an air-gap should not be placed between the line and the jet, though this would increase the resistance to the discharge. Its use is limited to places where there is a supply of water, unless pumping is resorted to. There are a number of different forms of this arrester, two of which are shown in Figs. 16 and 17.

Arresters in Parallel.—Much discussion has taken place regarding the effect of series resistances in lightning arresters in retarding static discharges. The static charge is represented by a certain current at a

certain voltage. If the resistance of the arrester is low enough, so that the voltage of the charge can force the current through the arrester resistance as fast as it arrives at the arrester, then there will be no further rise in potential, but if the resistance cannot pass the current at this rate, there will be reflection and a rise in potential. To overcome this difficulty a number of arresters may be installed in parallel, the arresters having different gaps and different resistances. The arrester with a minimum gap has the maximum resistance, and the arrester with a maximum gap has the minimum resistance. The theory of the operation is as follows :—

When a discharge occurs it will jump the smallest gap, and if the current is not too large for the resistance there will be no rise in potential, but if the current is too large there will be a rise in potential

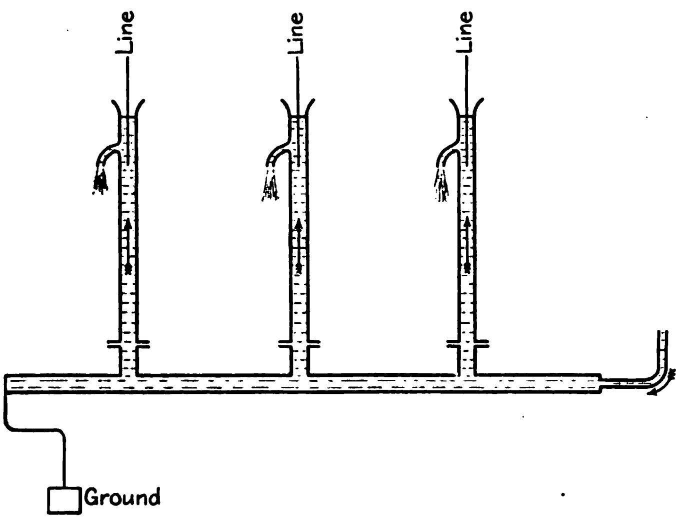


FIG. 16.—Water Column Lightning Arrester.

and the next larger gap will break down. With a very high rise the largest gap with minimum series resistance will break down. On some transmission systems arresters have been installed which consist simply of a horn-gap in series with a fuse. The gap is set so high that it will be broken down only with a very high rise in potential. This arrester, which offers a direct path to earth, is probably as good a means as any yet devised for taking care of a direct lightning stroke. Several of these arresters are connected in parallel, and usually only one discharges at a time, so that the line is not left unprotected, and arrangements are made for replacing fuses quickly.

The Electrolytic Arrester.—It has been found that aluminium in certain electrolytes forms a non-conductive film on its surface. This film, although extremely thin, will withstand about 400 volts. At higher

voltage the film is punctured with countless small holes and a large current is permitted to pass, but when the voltage drops below the critical value the film is reformed and prevents further passage of current. A sufficient number of these cells may be placed in series to withstand almost any voltage desired. It is evident that the aluminium cell possesses almost ideal characteristics for a lightning arrester, as it permits very little current to pass at normal voltage, but breaks down at higher voltage and re-seals itself as soon as the high voltage has passed and the line has been relieved.

When voltage is applied to the aluminium cell, a small current, partly capacity and partly leakage current, flows through it, and for a lightning arrester it is desirable to use a gap in series with the cell in order to prevent this current flow.

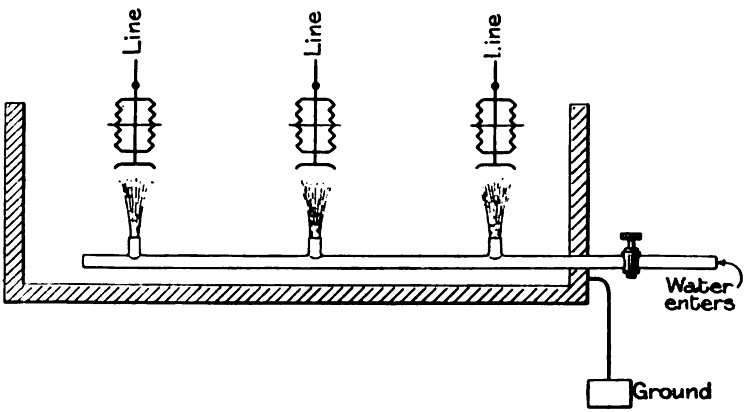


FIG. 17.—Water-jet Lightning Arrester.

The Water sprays upwards and strikes against the Plates which are connected to the Line Wires.

In order to obtain a large number of cells in series, the aluminium plates are pressed into tray form. These trays are set one inside the other (Fig. 18) and separated by suitable insulating washers. They may thus be built up into a column comprising a large number of cells in series. The columns are enclosed in suitable earthenware jars, and the trays filled with the electrolyte. This is done by filling the top tray and letting the electrolyte run over into those below. After the trays have been filled, the jar is tipped slightly, so that a small amount of the electrolyte runs out of each tray, and a small amount of transformer oil is then poured into the top tray and permitted to run over into those below. This oil prevents evaporation of the electrolyte. A number of jars may be mounted one above the other for high voltages (Fig. 19), and as the edges of the trays do not touch the jars, current must pass from tray to tray.

For voltages not exceeding 13,500, a gap consisting of the usual non-

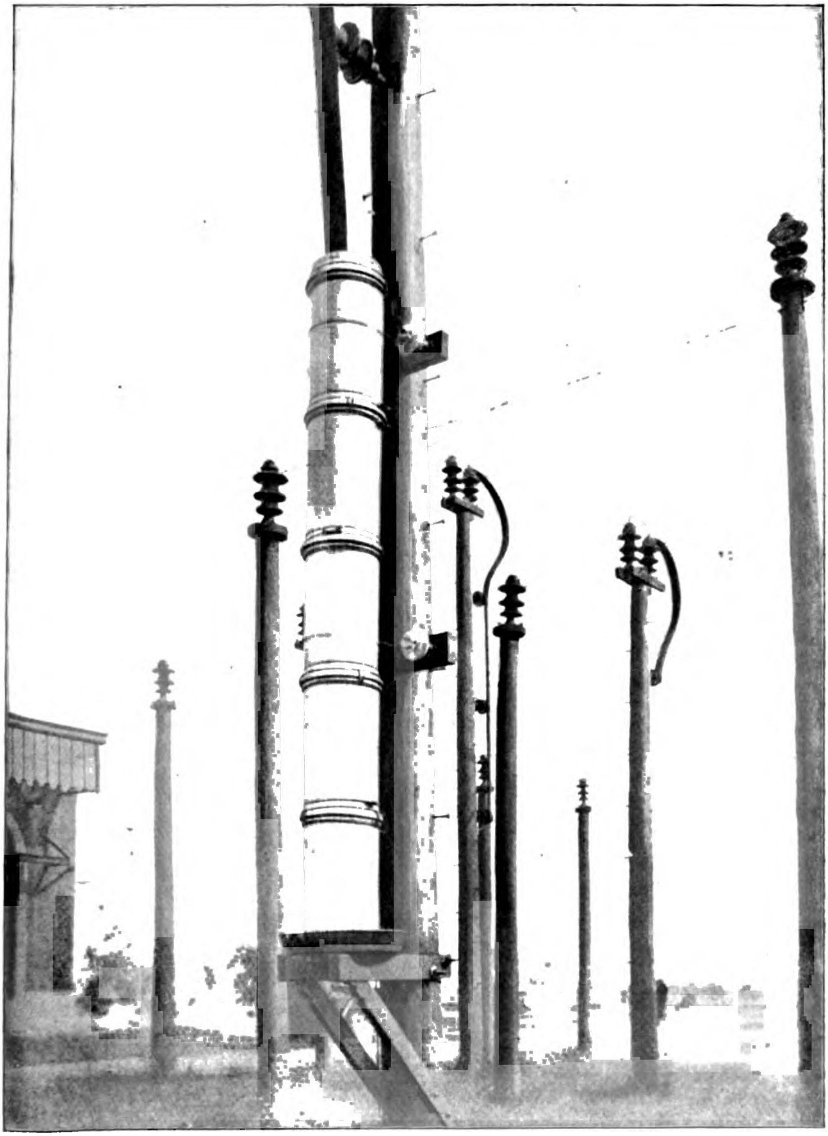


Fig. 19.—Electrolytic Arrester on 60,000-volt Circuit.

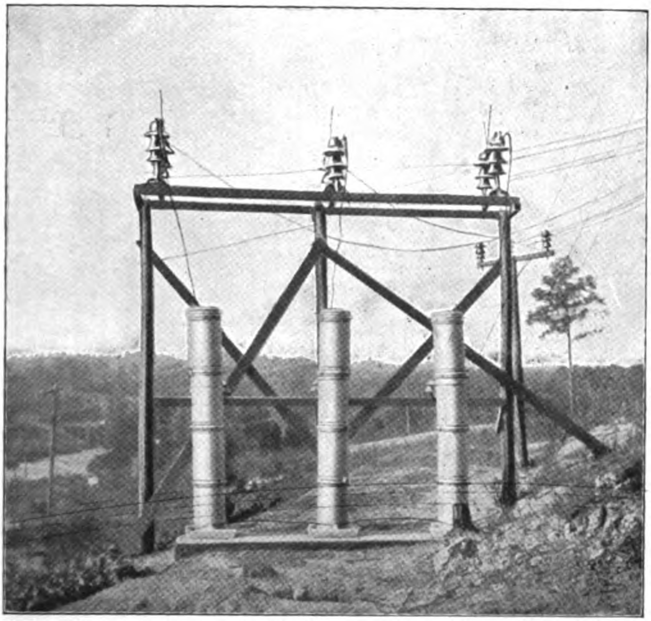


FIG. 20.—Electrolytic Arrester on 45,000-volt Circuit.

arcing metal cylinders is placed between the line and the electrolytic unit. For higher voltages the horn-type gap is used. The gap used must have sufficient power to suppress the arc which exists after the static disturbance has passed. The current in this arc is quite small. It is not advisable to use a horn-type gap on the lower voltages, as it must be set so close that the arc does not rise properly and extinguish itself.

The electrolytic arrester is manufactured for voltages from 4,000 to the highest in commercial use, and unless unforeseen difficulties arise it should practically solve the question of protection from lightning discharges.

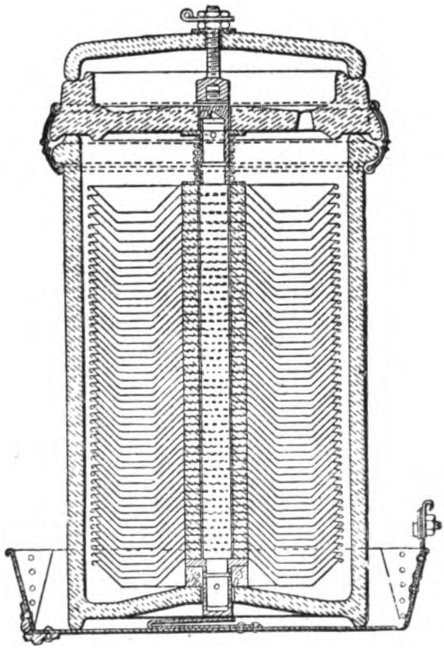


FIG. 18.

Illustrations of this arrester are shown in Figs. 18, 19, 20, 21, and oscillograph curves showing current and voltages on an electrolytic unit are shown in Fig. 22.

It is evident that no matter what type of lightning arrester is used, it must be set to discharge at a voltage considerably above the normal voltage of the line, so that before the arrester discharges the apparatus is subjected to a potential more or less above normal, depending upon the setting of the arrester. Thus the insulation of the apparatus should have a good factor of safety if satisfactory operation is to be secured. Fortunately solid dielectrics have the

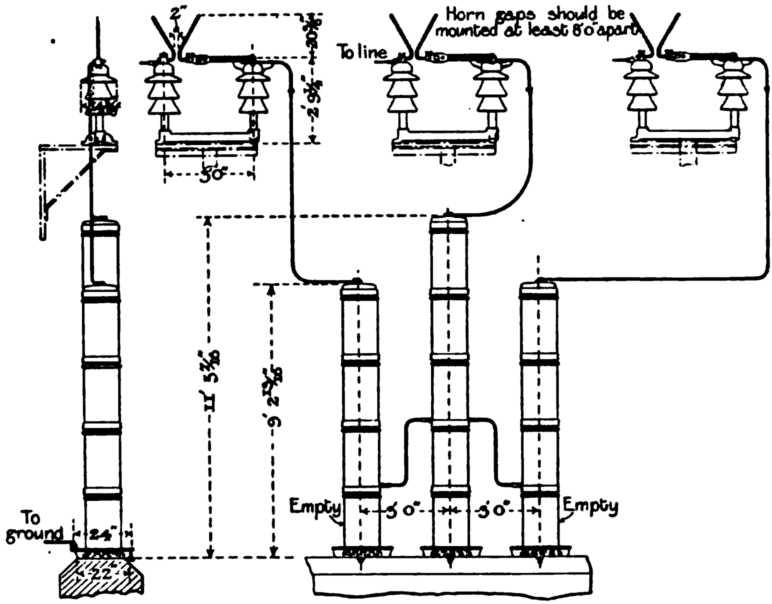


FIG. 21.—Outline of Electrolytic Lightning Arrester for 53,000 to 66,000-volt 3-phase Circuits with Underground Neutral.

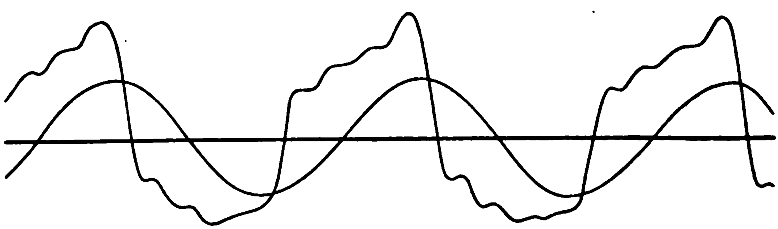


FIG. 22.—Oscillograph showing Current and Voltage on Electrolytic Unit, 12,500 Volts, 25 Cycles, 0·41 Ampere.

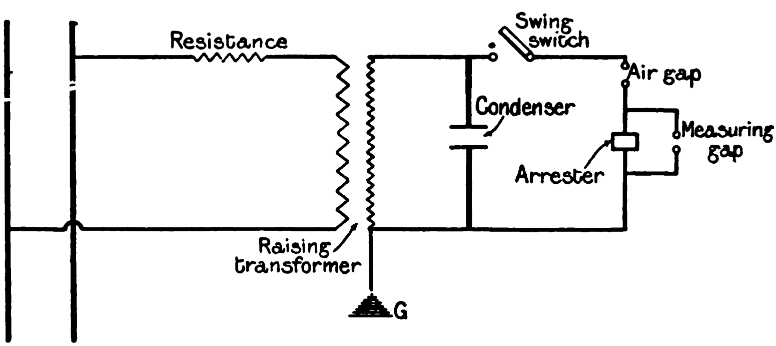


FIG. 23.

power of withstanding momentary strains when they would break down under much lower strains applied continuously. This fact probably explains why insulation troubles from static disturbances are not very much more frequent than they really are.

Ground Wire.—The concensus of opinion seems to be that one of the best methods of protecting a transmission line from lightning is to string a wire above the transmission line, this wire being grounded at frequent intervals. Three wires, one above and one at each side of the transmission circuit, are recommended as affording very complete protection. It is the general practice, however, to instal lightning arresters at the ends of the line, whether ground wires are used or not.

Testing Lightning Arresters.—Before concluding this paper, a few words should be said regarding the methods of testing the protective power of lightning arresters. Fig. 23 shows one system of connections which is used. A condenser is charged to a high potential from a raising transformer. This condenser is discharged by means of a swing switch through a spark-gap in series with the arrester. In shunt to the arrester is an adjustable spark-gap, which affords an alternative path for the discharge. The gap is adjusted until the great majority of the discharges, say 95 per cent., pass through the arrester instead of over the gap. The length of the gap under this condition is called the equivalent spark-gap of the arrester, and means an air-gap of such a length that a slightly higher pressure is required to break it down than is required to break down the arrester. Obviously, the lower the equivalent gap the greater is the protective power of the arrester, as the gap represents somewhat less than the minimum insulation strength which the arrester will protect.

In order to test the non-arcing power of the arrester, the adjustable spark-gap may be replaced by connections to the power circuit. When a discharge passes through the arrester, the arc which follows from the power circuit should be quickly suppressed.

SUMMARY.

Static strains may be produced by lightning, switching, grounding, or by any occurrence which causes an abrupt change in static potential.

Static strains may appear as high pressure to earth or between wires, or as a concentration of potential upon a portion of the circuit.

To relieve the high pressure between line and ground or between wires, lightning arresters are used. To protect apparatus from concentration of potential, choke coils are placed in series with the apparatus to be protected, or the turns nearest to the line are heavily insulated, or both choke coils and additional insulation may be employed.

Lightning arresters usually consist of an air-gap in series with a current limiting and an arc suppressing device. Four types of

lightning arresters are in general used—the horn, the multi-gap, the water-jet, and the electrolytic. The electrolytic is the last one to enter the commercial field, and while the time it has been in active service is short, the indications are that it will afford greater protection than any other arrester yet produced.

Discussion.

Dr. Kapp. Dr. G. Kapp: The method by which the author has endeavoured to explain complicated electromagnetic actions by means of simple and purely mechanical analogies is a particularly happy one. The railway train, where heavy cars are connected by springs, is an exact analogy of what actually is the condition of an electric circuit which is weighted with self-induction and capacity. On page 511 it is stated that the electrolytic lightning protector is charged with a "certain" electrolyte. Will the author in his reply say what precisely is the nature and chemical constitution of this liquid? Further, what voltage per tray can be allowed as the limit; and, finally, is the arrangement of this electrolytic lightning protector as represented on page 514 the usual arrangement? As shown there it seems that the electrolytic protector takes the place of an inductionless resistance sometimes used in combination with the lightning protector. It is not itself the protector; the protector in Fig. 17 seems to be the horn. It would be interesting to know whether the electrolytic protector can be used without the horn. The author mentions that where protectors of the tube-fuse pattern are used it is customary to have a good number all along the line, so that it does not matter if one has acted and becomes inactive afterwards. This applies to any other type also. The fact that not all of the fuses are put out of action at every stroke is easily explained; the waves of pressure and current produced by a discharge have nodes and anti-nodes. If a node happens to develop where a lightning protector has been placed, then this particular protector will remain inoperative. There is no protection because there is no rise of pressure at or near that particular place.

To get efficient protection we must distribute a sufficient number of protectors along the line, so as to make sure that an anti-node will hit a protector somewhere. There is an interesting point in connection with the rise of pressure which always accompanies a sudden reduction in current strength. Theoretically the rise of pressure on switching off a current is the current multiplied by the square root of the ratio of henrys upon farads. In most cases the capacity is small, and therefore the rise of pressure enormous; yet nothing happens. This might be explained on the assumption that the stress which causes insulation to be pierced depends not only on the electric pressure applied, but also on the time during which it is applied. A moderately high pressure applied for a few minutes may break down a certain kind of insulation, whilst one hundred times the pressure applied for the one thousandth part of a second may be quite harmless. The surge

on switching off has its own natural period of oscillation, and this is the shorter the smaller the capacity; so that although a small capacity makes the first blow very hard, it also makes it very short. The subsequent blows grow rapidly weaker owing to the enormous damping effect at high frequency. The damping is not only due to the ohmic resistance, but in a far greater measure to hysteretic and eddy-current losses. If it were not for this natural protection, the breaking down of direct-current circuits would be the rule and not the exception. As regards alternating-current circuits, the conditions are still more favourable. In an alternating line we always have the chance that, in switching, the arc which is drawn by the switch will last long enough for the current to pass through zero, and then it may go out. This seems to be the rule when switching under oil; the oil seems to have the property of extinguishing the arc exactly at the time when it goes through zero, and therefore no abnormal rise of pressure takes place as a rule. Still, the switch may not act so perfectly as here assumed, or there may be a sudden increase of current, or there may be a lightning stroke, so that there may be danger of excessive pressure, and the author has in an admirable way pointed out how this danger arises. The danger is the greater the greater the pressure. To provide against these elevations of pressure we have various lightning arresters, and amongst others the author has mentioned the horn type. I have brought here a part of a horn-type arrester, which was designed by Mr. Zapf, of the Land und See Kabelwerke, with a view to make its action more certain. The argument which led to this design, which is primarily intended for lines of moderate voltages, is this: In order to make the arrester go off at a voltage which is not too much above the normal, one must set the horns fairly close; in fact, so close, that any small foreign body, such as a fly or a spider, will cause the apparatus to act. This is inconvenient. When a discharge occurs and the horns are set close, there is an arc formed; a little metal is melted, forming a little globule; the globule protrudes, and the arrester has to be set again. To avoid this difficulty Mr. Zapf set to work to make an arrester (Fig. E) where even for moderate pressure the horns can be set at a suitable distance, say an inch apart, for as low a pressure as 5,000 volts. To make the arrester act with a moderate rise of pressure, something must be done to reduce the dielectric resistance of the air in the gap, and for this reason an auxiliary spark is provided, which acts like the hair trigger on a rifle. The diagram explains the connections. The auxiliary electrode has a platinum point with a screw, and is adjustable. It is set as close as required for the normal pressure. It may be said that the argument of the fly applies here also; no doubt a fly might set the auxiliary spark going, but the arc will soon die out, because it is in series with a high resistance. If, however, the spark is due to a rise of pressure, it will persist, and very quickly ionise the air in the neighbourhood, which will cause the horns to discharge. By this means Mr. Zapf is able to make a lightning arrester which can be set with great accuracy to go off at 25 per cent.

Dr. Kapp.

Dr. Kapp. above normal voltage, and yet not to go off uselessly by reason of something coming between the horns.

The author has mentioned the danger of "switching on," and has shown how, by the use of choking coils, this may be avoided. Fig. F

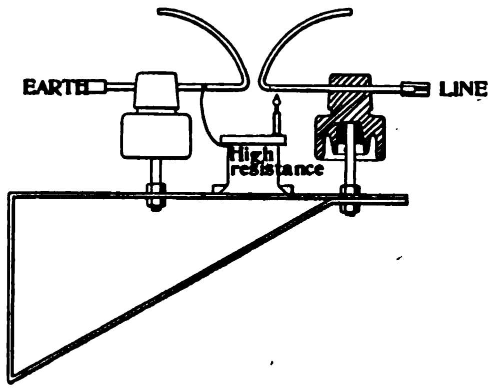


FIG. E.

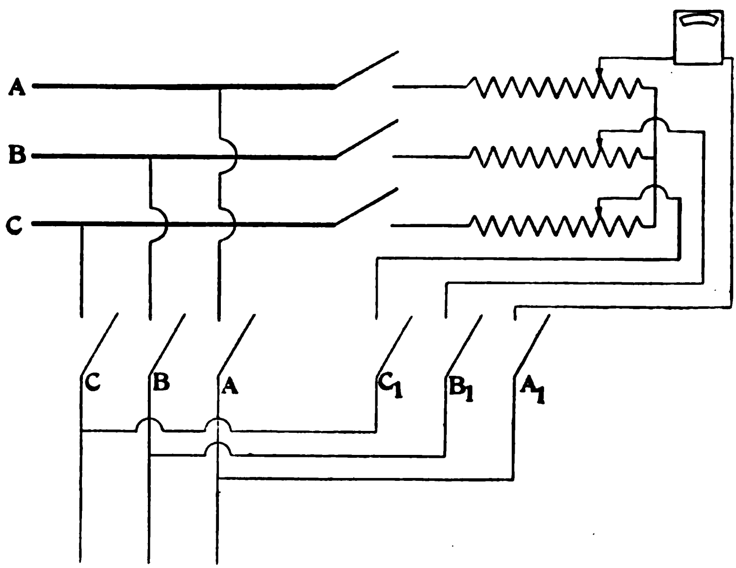


FIG. F.

shows another method by which we can avoid the danger of piling up pressure when a new cable or new machine is switched on to busbars. I have devised this method chiefly as a help to getting machines into parallel, but the application to transformers, cables, etc., is obvious.

The leading idea is to do the first switching in at low pressure, and then to raise the pressure gradually. Dr. Kapp.

The horizontal lines A, B, C represent the busbars, and the vertical lines A, B, C the connections to the cable, apparatus, or machine to be switched on. On the right is shown an auto-transformer with shifting secondary contacts. These are, of course, made double with resistance or inductance between each pair of contacts. The transformer being switched on to the busbars after C_1, B_1, A_1 has been closed, the incoming apparatus receives reduced pressure. The transformer itself is protected by reason of its secondary being closed. The shifting contacts are then moved to the left, so as to raise the pressure on the apparatus to the normal, when switches C, B, A may be closed and C_1, B_1, A_1 opened. Finally the transformer is switched off. In taking off the branch line or apparatus C, B, A, the operations are performed in the reverse order. I have tried this method in paralleling alternators, and have found it to work perfectly. The auto-transformer is set to give about quarter voltage, and the incoming machine is excited to give quarter voltage. There is no need to synchronise. On closing the switches C_1, B_1, A_1 there may be a rush of about double normal current into the new machine, but only for a moment. The machine is instantly gripped by this synchronising current and pulled into step. Its excitation is then increased to normal, and the contacts on the auto-transformer are simultaneously shifted to full voltage. The switches C, B, A are then closed, C_1, B_1, A_1 are opened, and this completes the process. It should be noted that the speed of the incoming machine need not be absolutely correct. I have found that if the speed is wrong by 1 or 1½ per cent. the switching in succeeds as well as if the speed is more nearly correct.

Mr. W. B. Esson: I can fully endorse what the author says about the coils giving way close to the line. In my early transmission days that was a thing that always happened. We had no step-up transformers then, because the pressure was not high enough; but I quite recollect that the alternator coils nearest the line were always the ones which gave way, and we put that down to the fact that the winding simply acted as choking coils, and the current jumped between the turns instead of going right through the winding to find the earthed point of the machine. The cure for that we very soon discovered; it was that if any choking was to be done, it should be done outside the alternator instead of inside. Mr. Peck has dealt with the subject of switching the current on to the apparatus, and Professor Kapp has described a very ingenious method of doing this gradually. Such devices have been used for a great many years, but it is not the every-day kind of occurrence for which provision is made that troubles the power engineer; it is the sort of thing that happens unexpectedly, and generally in a way that is not pleasant. As a matter of fact, the surges in the line caused by switching on are nothing like those due to a short circuit in the line if suddenly broken. This latter produces the greatest stress to which a line Mr. Esson.

Mr. Esson. can be put, because the effect of a short circuit is to make a very much larger current flow through the line, and consequently a much greater surging effect is produced when that current is broken. Mr. Baum, who controls in America the greatest power transmission system in the world, some 700 miles of line working at 40,000 to 50,000 volts, has given this matter a great deal of attention, and in a paper read before the St. Louis Congress in 1904 he went fully into the question of these surges. By taking certain usual values for the self-induction of the line per mile and for the capacity per mile—and these values for different lines do not, as shown by the formulæ, alter very much—Mr. Baum found that the surge pressure in volts due to breaking the circuit amounted to two hundred times the current in amperes which was interrupted. That is to say, with 100 amperes alternating current, giving a maximum of 141 amperes on the peak of the curve, the rise in pressure due to breaking that current at its maximum value would be approximately 28,200 volts. The curious thing that came out was that, as a first approximation, the rise in pressure was entirely independent of the working pressure of the line, and entirely independent of the length of the line, but dependent simply on the current which was broken. Mr. Baum drew some very important conclusions from that. He found as a matter of experience that it was much easier to work at a pressure of from 50,000 to 60,000 volts than at a pressure of from 25,000 to 30,000 volts. The reason was because the current being only half the magnitude in the case of the higher pressure, the surge pressure was so very much less. The figures can be easily worked out, but I made a note of some results this morning which I will give. At 60,000 volts pressure 3-phase there is between the neutral earthed point and either of the 3-wires about 48,000 volts maximum. With 30,000 volts there is half of that—24,000 volts. But supposing there is a current of 100 amperes flowing in the 30,000-volt line and 50 amperes in the 60,000-volt, and that by means of a short circuit these currents are doubled, then it will be found that for the 30,000-volt line the surge pressure may be as high as 56,400 volts, while for the 60,000-volt line it may be only 28,200 volts. This surge pressure is superposed on the maximum working pressure, so that the instantaneous possible pressure to be provided against in the case of the 30,000-volt line may be under certain circumstances greater than that to be provided against in the 60,000-volt line. Coming to the arresters, I have used both the multi-gap arrester and the horn-type, but only for medium pressures. It is when dealing with high pressures that difficulties with the arresters are met with. Mr. Baum, to whom I have referred, pins his faith to the horn-type arrester, and I do not think it is too much to say that he has tried multi-gap and other forms of arrester by the hundred. He finds, however, he cannot keep them on the line; they burn up and are of no use at all. In fact, he concludes that any form of arrester other than the horn (I do not suppose he has tried the electrolytic, which appears to be quite new) is, so far as his

experience goes, like some other things—only of use when there is no use for it, that is to say, when there is no lightning. He made some experiments on his lines by placing horn arresters with 4½ in. gaps at the power house and short-circuiting his line 100 miles away. In this way he got surges which jumped the 4½ in. gaps at the power house. It is an open question whether with pressures between 50,000 to 60,000 volts lightning arresters are of much good. Just consider what the condition of things is. We have an overhead line supported on porcelain insulators and terminating at each end in an oil-insulated transformer. In the very fact that the line is consistently insulated for 60,000 volts throughout all the minor troubles due to lightning are provided against, and it is unlikely that lightning surges can produce greater effects than are produced by breaking heavy shorts or than would be withstood by these lines insulated for 60,000 volts throughout, with a safety factor allowance of 2 or 3. The impression formed by many engineers is that with high pressures lightning arresters might without much risk be dispensed with. In the case of Mr. Baum's lines on the system of the California Gas and Electric Corporation, these horn arresters with 4½ in. gaps only go about once in a year, and I believe Mr. Baum only keeps them on the line for conscience' sake, and because he likes to take all precautions. If they do no good, at any rate they do no harm. Mr. Esson.

Mr. A. RUSSELL: In the earlier part of the paper there are one or two points which call for discussion. Mr. Peck says, for instance, that every line has a "natural period of oscillation." He also compares a transmission line to reactances and condensers in series. Both of these statements, I think, need to be amended. Let us consider the mechanical analogy he gives. One wagon and a spiral spring will have a perfectly definite period of vibration. But when we have two wagons and two springs there are two free vibrations, and mechanical resonance will ensue if the period of the forced oscillation is the same as either of these periods. Similarly when we have n wagons there will be n free vibrations. In a power transmission line there is an infinite number of free oscillations. In the particular case considered by Mr. Peck, namely, when the resistance of the line is negligibly small, the frequency of the free oscillations can be written down at once when we know the length of the line. The value of the diameters of the wires and their distance apart has no effect on the periods of the free oscillations. Let us consider a single-phase line of length l miles. When the alternator is switched into the circuit electric pulses of opposite sign travel along the lines with the velocity of light and get reflected at the far end. If they complete the return journey in half the period of the applied E.M.F. the reflected waves will be in phase with the generator waves, and hence the voltage and the current will go on building up until there is a breakdown. The frequency of the fundamental type of free oscillation is thus the velocity of light divided by four times the length of the line. In the case of a 20-mile transmission line, therefore, it will be 186,000/80, that is, 2,325. Any odd multiple of this number Mr. Russell.

Mr. Russell.

will also give a frequency at which resonance will occur. The formula given by Mr. Peck only applies to condensers and reactance coils in series. In the actual case we have distributed inductance uniformly shunted by capacity.

Mr. Peck rightly insists that one of the main functions of lightning arresters is to get rid of static charges which accumulate on overhead lines owing to atmospheric electricity. A knowledge of the laws of atmospheric electricity is an essential preliminary to the study of lightning arresters. I think we may assume that the potential gradient in the air is due to a negative charge on the earth. The gradient in this country near the ground in foggy weather is sometimes as much as 1,000 volts per metre. Even on an ordinary day it is about 200 volts per metre. It diminishes as we ascend, but the potential of a stratum of air 4,000 metres above the level of the ground is at least 100 kilovolts above the potential of the earth. It is easily seen, therefore, that in mountainous districts the potential differences between the overhead transmission lines and the earth will be variable quantities. It is also easy to see that when a spark-gap is 8 or 10 metres above the ground the potential gradient at the electrodes of the gap may be very great, leading to excessive ionisation of the air in the gap. Hence its action will be very uncertain. The fact that spark-gap arresters are sometimes almost continually in action during certain states of the atmosphere is often quoted as a proof of their efficiency. It may merely mean, however, that the spark-gap is continually breaking down at the normal voltage, and so, that the device is a source of weakness to the whole system.

Continuous arresters, like the water-jet arresters described in this paper, seem to me admirably adapted for getting rid of the static charge, but I think that their resistance would be too high for them to be of much use to limit sudden rises in the value of the potential difference between the lines.

Batteries of electrolytic cells seem to act as excellent safety valves, as their resistance rapidly diminishes at pressures above the normal. It would be of interest to know what electrolyte is used in the cells described by the author. A critical pressure as high as 400 volts is exceedingly satisfactory, and I do not know any electrolyte that gives a value approaching this. But even taking 400 volts, this would mean, with alternating pressures, about 4 cells per kilovolt—and this would be expensive in high-pressure lines. Besides, owing to heating effects and consequent evaporation of the electrolyte, etc., it is impracticable to connect electrolytic cells directly between the circuit and earth, and the use of a spark-gap considerably discounts their utility.

I think that the electric safety valve of the future will be of the coherer type. A powder, for instance, may be found which, when contained in a suitable tube, offers a very high resistance to currents at normal voltages but practically none to currents at higher voltages. An arrangement of this type would form a perfect safety valve. There have been numerous references recently in the French technical papers

to Thury arresters of the coherer type. A description of these safety valves would be of great interest. Mr. Russell.

Mr. W. H. PATCHELL: I think the author is too modest when he starts off with an apology for want of novelty in his paper. His name has been well known on the other side of the water for many years in connection with all the reports published by the High Tension Committee of the sister American institution, and one has only to refer to the two important volumes which have been published by the McGraw Company—the papers which were buried in the Minutes of the Proceedings are now available to everybody—to see the part the author has played in the development of this important class of apparatus. The only blemish I have found in those books is that they begin in 1903, and that the classical paper by Mr. Thomas, which the author refers to, is unfortunately not included, so that people who pick up the book cannot, as it were, begin at the beginning. Then the author puts aside his modesty and tells us at the bottom of the first page that the devices have been carried to a high state of perfection. That is where the commercial gentleman comes in; we are not to wait; we are to buy what is now available. "We have developed, and we are quite ready to try it on you!" But on turning to the last lines of the paper we see the author states his belief that although "the electrolytic is the last to enter the commercial field, the indications are that it will afford greater protection than any other arrester yet produced." We there get an inkling that we are not yet at finality, and that is the fact. Particularly on the other side of the water, where this class of apparatus has been very largely used, it is still in a transition state. There is no "best." Some men swear by the multi-gap type, some by the horn type—there are various forms of horn arresters—and some by other types. It is a fact that what will suit one condition will not suit another. The science has not yet developed and been reduced to well-known rules, and many of us have practically blundered along and done the best according to our experience. The concentration of potential at the end coils of a machine is a detail that we had to find out for ourselves as soon as we started the Bow plant. In my paper* describing those works, I went, as far as I could in the time at my disposal, into the question of surges, and ventured to hope that somebody else would take the matter up. I am sorry we have had to wait for two years, but what we have now has been worth waiting for. On the question of surges, I would like to ask such a mathematician as Mr. Russell if the matter is quite as simple as he suggests it is. We may get a wave reflected at the velocity of light from the end of the line on a blackboard, theoretically; but in the lines that any of us have built in practice we get inductance and capacity and various other idiosyncrasies which we do not get on a blackboard. I would like to ask Mr. Russell to consider that. Mr. Patchell.

Mr. A. RUSSELL: I forgot to mention that in any line the product of the capacity and inductance is always equal to l^2/v^2 where l is the length Mr. Russell.

* *Journal, Institution of Electrical Engineers*, vol. 36, p. 99, 1905.

Mr. Russell.

of the line in miles and v is its velocity in miles per second. Taking Mr. Peck's figures it will be found that they satisfy this relation. [The proof of this is given at length in Russell's "Alternating Currents," vol. i. p. 137.] It is shown, for instance, that the shapes of the cross-sections of the transmission lines and their distance apart has no effect on the value of the product, capacity × inductance. When the resistance of the line is taken into account the calculation of the resonating frequencies is more difficult. With the help of a little trigonometry, however, they can be readily computed from formula (49), vol. ii. p. 471, of the work above mentioned.

Mr. Patchell.

Mr. W. H. PATCHELL: On page 506 various forms of protection are mentioned by the author. The insulating of the end winding inside the machine, or choking coils outside the machine, are now commonly used. In class (*b*) the author mentions switching through a resistance. I believe the first example of that was due to Ferranti, who had a liquid switch and a tripping coil in series with it. When the switch was closed the coil was tripped; a flexible conductor was unwound which let a plate down at the bottom of the pot, and so a gradual short circuit was produced. The same apparatus was used by Ferranti for charging cables. Mr. Partridge had a good deal to do with the development of that class of apparatus. Then we come to the question of the various types of arrester used. We have first of all the ordinary horn type. What we might call the common type of horn arrester, generally spoken of as the Siemens type, is often not much better than useless. There are people who would have money in their pockets if they had not put the ordinary horn type of arrester on. With the horn type one very great trouble is the big flare which takes place when breaking. I am glad Professor Kapp has put before us the Land and Sea Cable Company's variation of the horn type. I have tried that not only in laboratory experiments but also practically, and the way in which that little trigger, as he called it, acts is simply marvellous. I mentioned in 1905 that facing one side of the horn with carbon got over the building up of metallic bubbles which upset the calibration, so that we could work more closely with the carbon and copper horn than with the copper horn only. I have tried the Land and Sea type up to 20,000 volts with a voltmeter in circuit, and the way it will act time after time at the same point on the voltmeter is remarkable. By disconnecting the tail of the resistance the arrester will act simply anywhere; by putting the resistance in circuit again—and one can repeat the experiment as often as desired—it will act regularly at the predetermined pressure. Practically one might almost calibrate the voltmeter by it! Of course, precaution must be taken against the changes of temperature, moisture, and that sort of thing for such accuracy as I am speaking of. The non-arcing multi-gap arrester is a particularly American idea. My difficulty in believing in it has always been my fear that a man would not turn the cylinders round. I have asked people who used this type whether that is so, and they say, "We have no trouble." I have then inspected the cylinders and find they have never arced,

Mr. Patchell.

and I have inspected the resistances in series with them and find there would not be much left if they did happen to act with some power behind the arc. The combination of the multi-gap arrester and the particular plant that it happened to be on was a happy one! There is in Fig. 14 a most marvellous combination of multi-gap arresters. One wonders how many gaps there are on that vertical stand. It is more like one of our friend Mr. Gill's telephone boards than a lightning arrester! I do not know how long it would take a man to look over it for the places that have arced and turn the cylinders round. I think it is thirty-eight rows high, but how many cylinders wide it is I do not know. On page 508 the author mentions the necessity of limiting the current. That is the crux of the whole question; the resistance to be used in series is all-important. The horn arrester or multi-gap arrester without a good resistance is just like the best haulage motor without a good controller—one cannot work without it. It is not so much the trouble that we get when the arrester goes, but the trouble we get when the arrester stops going—that causes anxiety. The flow of current is then what does the damage. At Charing Cross we tried dry resistances, liquid resistances, and also dust resistances, but as my friend Mr. Brazil is more acquainted with that development, I hope he will tell us in his own words how far he has now got with them. I gather that since I left the Charing Cross Company he has been going on with the same work, and has developed it now into something which is worth looking into. Mr. Mershon told me lately that the best resistance he had got for 60,000 volts was a concrete pillar. I asked him if it made much difference what material that concrete was made with —flint or rock with more or less mineral in it—but he said that he had not found that the class of ordinary rock used affected the resistance of the pillar so much as the size of the material that the concrete was gauged with. The water-jet arrester, as has been mentioned, is largely used in Southern France and Northern Italy, but the author has not told us that the common practice there is to turn the water nearly off when they do not expect any lightning, and to turn it on when they do! That is rather an objection to the use of that form of arrester. Another objection is that in many places where lightning occurs there is no water! The first arresters in parallel I ever saw were outside the Ontario Company's power house at Niagara. Mr. Mershon described the arrangement most fully in his paper* on the Niagara, Lockport, and Ontario Power Company's distribution system. The author says quite truly on page 513 that a few words ought to be said regarding the methods of testing the protective power of lightning arresters. I should like to supplement that by saying: Do not test them in a laboratory; get a big machine behind them and then you begin to know something about them! The arrangement of the electrolytic cell in Fig. 18 reminds one of Sellon's dish battery, which I had something to do with twenty years ago. We gave it up after a short experience because the peroxide bubbled up—due to the liberation of the gas

* *American Institute of Electrical Engineers*, Niagara Falls Convention, June, 1907.

Mr. Patchell. —and short-circuited. In appearance the general arrangement was very similar. The author does not mention a very important matter—the expense of protecting the protective apparatus. It does not matter what apparatus is used as protective, if one does not consider the protection of it one gets very far astray. Certain apparatus can only be used indoors; for instance, the multi-cylinders and the horn type of arrester for low tension, which are impossible out-of-doors. So that we have to consider whether the arresters are going to be put on the top of a mast or in connection with the switchboard, where they can be put well under cover.

Mr. Brazil. Mr. H. BRAZIL: The remarks I have to make apply exclusively to those systems in which underground cables are used—that is to say, those systems which are not subject to lightning disturbances. The author does not appear to think very highly of horn arresters, but as I have had a very considerable experience with them, I should like to say a few words in their favour. Mr. Peck, in talking of horn arresters, is probably thinking only of those illustrated in his paper, which consist of two bent pieces of wire going away from each other at a sharp angle. I should like, however, to refer to another type which Mr. Patchell has mentioned this evening, and which he described in his paper in 1905. This arrester has two nearly parallel bars, one of copper and the other of carbon, arranged vertically, slightly diverging towards the top, and terminating in widely diverging copper horns. An insulated box with glass front is built up round the vertical portions, and the arc which strikes at the bottom is assisted in rising by the draught that it causes. These arresters can be set with very great accuracy, and it is astonishing how long this accuracy is maintained in spite of the large number of discharges that have passed in the meanwhile. Tests taken at six months' intervals show that year after year the sparking voltage has remained the same.

Mr. Peck, on page 507, says that one of his objections to the horn arresters is, that the arc takes a long time to break. I do not quite know what he means by "a long time," but I do not consider that from 2 to 4 seconds—which is the figure with these arresters—should be considered too long. I would further mention that I have never found these arresters to fail in breaking the arc. The author also states that they form a short circuit on the system, but to my mind it would be foolish to put them in circuit without a very considerable resistance; in fact, I consider the resistance, and a pretty high one, absolutely necessary. Mr. Peck then mentions that having this resistance in circuit retards the static discharge. From what he says at the beginning of the paper about the word "static," I presume he means that as the resistance is increased the sparking voltage is increased also. This I have not found to be the case, testing with a considerable variation of resistance in circuit with the horns.

The author goes on in his paper to say that there is a lack of

interest in this country on the subject of lightning arresters, and I think this is largely due to the fact that it is so difficult to know whether they are working or not. On asking several engineers, who have the multi-gap type in their stations, as to how they work, I have been told that they have had no trouble with them at all. I think Mr. Patchell hit the nail on the head when he said that they had no trouble because they had never worked. An examination of the rollers used in this multi-gap type will generally reveal no signs of any current passing; and further, the resistance, which often consists of a small carbon rod, is, to my mind, utterly useless in dealing with any surge in a manner which would be of value in reducing the strain on the system. This brings me to the point that I think an indicator of some sort is what is required. Mr. Patchell, in his paper in 1905, described such a piece of apparatus which I had the pleasure of getting out for him. As it is some time since this paper was read, I may perhaps be permitted briefly to describe it again. It consists of a small transformer, the primary of which is connected between the resistances in circuit with the horn arresters and earth, the earth end of the winding being connected to the iron of the transformer. The secondary of this transformer is connected to a shutter-type relay, the shutter of which in falling makes a local circuit, thus causing an electric bell to ring, which bell continues ringing until the shutter is replaced.* This indicator has now been in use from 3½ to 4 years in the stations on the Charing Cross Company's system, and some hundreds of discharges have been recorded. At the time of the discharge, particulars of the number of cables in use and the machines switched on or off are noted, and when the records are examined the results are found to be very interesting. Take one case for an example: we find that during the summer, which is the time of light load, a very much larger number of discharges are recorded. This, I take it, is due to the fact that the cables being lightly loaded are more nearly in the open-circuit condition, which, as Mr. Peck points out, is the condition which is likely to produce surges. **Mr. Brazil.**

This indicator has also detected bad paralleling, faulty insulators and transformers, and, further, has made it clear that when a short circuit on a cable occurs, there is almost bound to be a rise of pressure on the whole system. Many engineers would be surprised if they knew the number of times the pressure on their systems rises to 50 per cent. above the normal, which is the figure at which I think arresters should be set.

As mentioned before, some hundreds of discharges have been noted, and I think the fact that they have occurred has very probably saved breakdowns. If multi-gap type arresters had been used, I do not think they would have safeguarded the system to anything like the same extent, chiefly because of the difficulty in setting them to anything like the accuracy with which the arresters above described

* The transformer is designed to absorb less than 10 volts, and therefore does not destroy the non-inductive character of the resistance.

Mr. Brazil. can be set. Further, with multi-gap arresters without a resistance or with a small rod of carbon in circuit, I am strongly of the opinion that, as it is almost impossible to say what current flows when a discharge occurs, this current may in itself be so excessive as to cause severe surges.

With regard to non-arcing cylinders, I have examined some of these in very large stations, and think that "non-arcing" is hardly the proper term to apply to them, as there were very distinct signs of arcing, and I should imagine that the current had been very excessive.

In conclusion, I should like to say that in my opinion in a system such as I have described, that is to say, one using underground cables, and therefore not subject to lightning strokes, the best arrangement is the horn arrester of the type I have described with a high resistance in circuit, limiting the current to, say, 1½ to 2 amperes, and fitted with indicators to show when a discharge occurs. These arresters should be placed in suitable positions all over the system, and as Professor Kapp points out, in order to obtain the greatest protection the number of these should be as large as possible.

Mr. Mordey. Mr. W. M. MORDEY: I note with interest on page 515 that "the concensus of opinion seems to be that one of the best methods of protecting a transmission line from lightning is to string a wire above the transmission line." Can the author say whether there is any advantage in using a barbed wire for that purpose, to get a large number of points, on the principle, of course, that prevention is better than cure? In the case of long transmission lines there should be a distinct advantage in making all the poles as far as possible lightning dischargers, or lightning protectors, by using well-earthed pointed conductors to discharge the atmospheric electricity—not merely to receive the discharge in the event of a stroke of lightning. That kind of protection is simple and costs very little. Those who have not forgotten their experiments in "Frictional Electricity" will know how very great is the effect of earthed points in preventing accumulations of charge, and how much may be done in that way to avert an actual "stroke." I have done what I could to give effect to this principle in work where lightning was frequent and severe, and I think with good results. We rely on earthed point protectors for buildings for the double purpose of preventing or lessening strokes, and for receiving them when they cannot be wholly prevented. If this plan is effective for this purpose and applied in this very partial way, much more should it be effective if applied on every post of a transmission line. I venture to think this is an aspect of lightning prevention that might usefully receive more attention.

Mr. Andrews. Mr. LEONARD ANDREWS: Several speakers have referred to the electrolytic arrester described at the end of the paper, and have spoken of it as if it were something quite new. This arrester. was described in a paper by Mr. R. P. Jackson* on Lightning

* *Transactions of the American Institute of Electrical Engineers*, vol. 25, p. 881, 1906

Mr. Andrews.

Protective Apparatus in 1906. It had then been in use, I believe, about two years, and judging from the discussion on that paper, it had up to that time given very satisfactory results. I am pleased to see the author looks upon it as being one of the most promising devices. I think it was Mr. Russell who said that what is really required is an arrester that partakes of the nature of a coherer. It seems to me the electrolytic arrester very nearly approaches this idea. It is an insulator for normal pressures, and does not allow current to pass until the pressure exceeds the normal by a definite percentage, when the insulation breaks down. The insulation is automatically restored immediately the potential drops to normal. I must protest against Mr. Peck's explanation of the theory of the horn break static discharger, the action of which he describes as being due to heated air, "the air being heated in rising until it (the arc) is drawn out so long that it is ruptured." This used, I know, to be the accepted

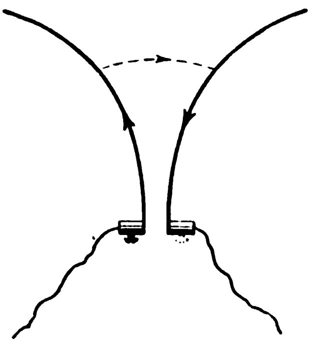


FIG. G.

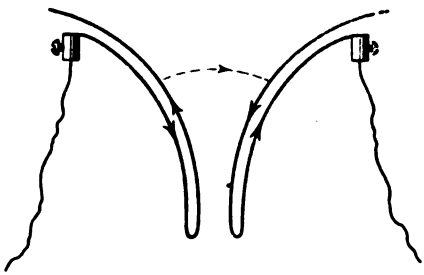


FIG. H.

theory for the action of this type of break. Some years ago I carried out a lot of experiments with this type of interrupter, and I found it worked almost equally well on its side, or even upside down. In the latter case the arc was blown down, which obviously shows that the effect could not be chiefly due to the rising of the heated air, though this action may assist to a slight extent.

The rising of the arc is, I think, mainly due to the magnetic repulsion between the current in the horns and the current in the arc, the direction of the flow of the current in the horns being opposite to that of the current in the arc as shown by Fig. G; in other words, it is due to the well-known principle that a looped conductor carrying current tends to enlarge itself. As a proof of this theory I constructed double horns as shown at Fig. H. It will be seen that in this construction the flow of current through the double circuit of the respective horns is equal and opposite. The repelling effect will therefore be neutralised. As I anticipated, horns constructed in this manner entirely failed to break the circuit. It is obvious that if the break had been

Mr. Andrews. due to the rising of the heated air, the altered construction of the horns would have had no effect upon its behaviour. It is, I think, important that this principle should be thoroughly understood, as a horn break discharger may be rendered ineffective by inadvertently carrying the connections to the discharger parallel and adjacent to the horns.

Mr. Fynn. Mr. VAL. A. FYNN: That a concentration of potential does occur at times admits of no possible doubt, but I was not aware that it could be gauged as nicely as the author's curve Fig. 6 would indicate. Some three or four years ago I had the opportunity of looking into the case of a 40,000-volt 3-phase transformer of some 400-k.w. capacity, which had broken down repeatedly without any apparent cause. The trouble was finally traced to concentration of potential and cured by a combination of heavier insulation and the use of chokers. Experiments carried out at the time showed a distinct rise of potential between adjacent turns up to about fifteen times the normal value, but no definite figures could be obtained. A spark-gap was used for estimating the potential between layers. One terminal of one phase only was at first connected to the mains, then this first terminal being left connected up the circuit of that phase was closed and finally a double-pole switch was used, thus connecting both terminals of that phase to the circuit simultaneously, or very nearly so. The highest rise of potential was observed in the second and third cases, but appeared much more regularly during the second experiment. No doubt the two blades of the double-pole oil switch did not always make contact simultaneously, and in all three sets of experiments the results seemed to vary greatly with the instantaneous value of the generator E.M.F. at the actual instant of closing the switch. Perhaps the author will say how he obtained his very regular curve, and also state the exact conditions under which the tests were carried out.

With reference to non-arcing multiple-gap arresters, when residing in Switzerland I devoted a good deal of attention to the protection of high-voltage overhead transmission lines, and had ample opportunities of putting all sorts of weird devices to a practical test. The Wurtz arresters came as a great boon, and their mode of operation greatly occupied our attention. I am inclined to think that there is no one metal which possesses the property of suppressing alternating arcs to a greater extent than any other similar metal. In constructing such an arrester I believe it is only necessary to select a very good conductor of heat, to give it some rounded shape, cylinder or ball, to see that its surface remains clean, and to make the volume of each element large. Any such arrester will have a limited capacity of discharge; the limit will be reached when the rate at which heat is generated when an arc passes from one element to another exceeds by a certain amount the rate at which this heat can be conducted away by the mass of the element. In other words, as soon as the temperature of the elements in the immediate neighbourhood of the arc is raised beyond a certain limit, then the arc persists. It is clear that it soon becomes useless to increase the volume of each

element in order to increase the capacity of discharge, for the heat cannot be conducted away at such a rate as to keep pace indefinitely with a constant increase in volume of the elements. In order to preserve the non-arcing quality of such an apparatus, it is therefore necessary to limit the maximum machine voltage per unit gap. Now, although a 3,000-volt circuit may be sufficiently protected if the arrester is set to go at 6,000 or even 9,000 volts, yet a 20,000-volt circuit, for example, would *not* be sufficiently protected if the arrester were set to go at 60,000 or even 40,000 volts, with the result that the maximum machine voltage per unit gap must increase with increasing line voltage. The heat of the arc will therefore also increase with increasing line voltage, and a point is soon reached when the elements cannot be sufficiently cooled in the Wurtz arrester. As soon as this is the case the arc persists. The arc is also much more likely to persist when backed by a very powerful station. I suggest that these are the reasons why these arresters do not give satisfaction on very high-voltage circuits. Mr. Fynn.

We made many tests with other metals, the results being all satisfactory; brass is, however, preferable, for it can be easily cast into the required shape, and retains a clean surface, so that the sensitiveness of the apparatus does not vary. It would be interesting to hear if the author knows of any one metal which does not possess the property of suppressing an alternating arc under the above conditions, and if he does not agree with the explanation of this non-arcing property which I have suggested, perhaps he will give us his own views as to the exact manner in which such arcs are suppressed.

We have repeatedly used both plain and barbed ground wires in Switzerland, without ever being able to say with certainty that they were useful, although some consulting engineers always called for them and considered them to be indispensable. It is, I think, reasonable to suppose that if very carefully grounded they do reduce the maximum possible potential of the line. They are probably of distinct use when lightning strikes the line directly. If a line is struck some distance from the station, then as a rule the line only is damaged; but if it is struck close to the station, the probabilities are that both line and station will be badly damaged, and I do not think anything can afford protection in such a case. Cases of direct stroke are extremely rare on the Continent, but probably more frequent in America.

The author has said nothing as to the distribution of the arresters. As long as the discharging capacity of each arrester is small, and so far we have had no others, excepting perhaps the horn type, I always prefer distributing the arresters in bunches along the line. This is a difficult and tedious business, but gives, I think, greater safety in most cases. It is necessary to find the points where the maximum potential usually occurs, and to place the arresters in that neighbourhood. The position of these points apparently does not only depend on the constants of the line itself, but on the nature of the surrounding

Mr. Fynn. country as well; they can only be found by trial and error. I frequently examined the arresters and never hesitated to change their position as many times as seemed necessary.

The question of a proper earth is of vital importance, as a bad earth is not only dangerous to the apparatus, but also to human beings. I should like to ask if any new developments have taken place in that direction of late.

The electrolytic arrester described by the author is not only a most interesting, but also a very promising piece of apparatus. I only wish it had been available at the time I was in daily touch with this particular proposition. I have had no experience of this new apparatus, but it does seem to me to possess a great many of the properties which one must ask for in a lightning arrester. One is naturally led to look for the weak spots in any new departure. The points that suggest themselves to me are briefly as follows. It is just possible that the insulation between the central metal rod and the cells will be liable to break down, the more so as it is constantly immersed in the electrolyte, which I presume, is a solution of soda. If this insulation does break down at two points, a part or the whole of the cells will be short-circuited. A fault of this nature will make the arrester more sensitive, and will not therefore endanger the apparatus or the line, but might lead to the destruction of the arrester itself.

The electrolyte is liable to freeze or to evaporate. The freezing is not so very serious, because storms rarely occur in such cold weather. It is proposed to prevent evaporation by sealing the cells with oil. It occurs to me that there is a certain amount of danger in this remedy. It is possible for a thin layer of oil to coat the effective surface of the cells, thus preventing that action from taking place on which the operation of this arrester depends. Such an oil film would at any rate greatly reduce the sensitiveness of the arrester, and thus endanger the machinery.

One great advantage of this arrester is its great discharging capacity. I should think that electrolytic arresters will give sufficient protection if placed near stations only. In such positions they can be well looked after, and any extra expense thus incurred will be good economy. I sincerely hope that this ingenious apparatus will really fill the gap which at present does exist, and is seriously felt.

The President. The PRESIDENT (Colonel R. E. Crompton, C.B.): Unfortunately the President rarely has a chance of taking part in the discussion, but in this case the paper is of interest to me, as my firm had considerable trouble in India in protecting air-lines from lightning discharges, and I should have liked to put to the author some interesting questions on the best method of dealing with the difficulties we met with where the atmospheric conditions were somewhat abnormal. At Darjeeling we had a case where the overhead was 3 miles in length. The difference of level was 3,500 ft. In addition it was probable that there were unusual differences in the moisture present and the temperature of the air at the highest and lowest points of the line. Mr. Moncrie-

was the engineer then in charge of that line. He believes that the extraordinarily rapid discharges noted at the foot of the line were not due to direct lightning discharges, but were probably due to rearrangements of the earth's potential following the lightning discharges which took place near the line. Under these conditions he found that the horn arresters quickly became unserviceable, the horns being burnt away. The Wurtz dischargers were used, but never had a fair chance on account of the difficulty of getting good earths for them. An earthed guard-wire was used on the poles above our conductors, but with no apparent benefit. As we did not wish to impede the circuit we tried to work without kicking coils. We then found that the discharges, which took place with extreme rapidity—sometimes 20 a minute—occurred at the revolving armature of the alternating generator, between the thimbles and one side of the slip-ring. At this point it was continuously blown out by the air draught of the revolving armature and never did any damage, so that it appears that a mechanical arrester might be made on this principle. The President.

Mr. Moncrieff himself devised an arrester which contained 30 gaps. These were all of the horn pattern, arranged in parallel on two concentric rings, the inner one connected to the line and the outer one to the earth. This construction of arresters seemed to be more durable and more effective than any of the other arresters that were tried.

Mr. J. H. M. WAKEFIELD (*communicated*): In connection with these protective devices, might I ask Mr. Peck to include some details of the cost of lightning protectors per mile per wire. Lightning damage in Great Britain must be taken into account, but the commercial aspect of the question is most important—that is, the ratio of the cost of lightning protectors to the cost of plant which it is protecting. It is a case of compounding an insurance premium as a safeguard against lightning. Air-gap arresters have been used by the Post Office for many years and have been found to meet all requirements. I will not go so far as some of the speakers in saying that if the protectors had not been fitted the results would have been the same. It would be useful to know the earth resistance which Mr. Peck recommends for a lightning protector earth. The standard adopted by the Post Office is a maximum of 10 ohms. The Post Office does not use a ground wire above the working wires. The expense does not appear to be justified, especially on a long-distance line. Each Post Office wood pole and arm is fitted with an earth wire. Mr. Wakefield.

Mr. P. LOESCHER (*communicated*): The author has chosen a most interesting subject, which he treats with skill, particularly as regards the mechanical analogies introduced to explain the phenomena to be dealt with, which are certainly original. But in other respects Mr. Peck was decidedly one-sided. He does not mention a single name not American in conjunction with research and original work done in this field. In Fig. 17 he illustrates a water-jet arrester, but he neglects to mention the inventor of this device—Mr. Brown, of Brown-Boveri & Co.—neither does he mention Siemens in connection with the horn- Mr. Loescher.

Mr. Loescher.

type lightning arrester. In fact, he overlooks altogether the extensive work done on the Continent, and particularly in the classical countries of high-tension transmission—Switzerland and Northern Italy.

The paper left one with the uncomfortable conviction that its ultimate purpose is to advertise a certain device—the electrolytic arrester. This device is, as Mr. Field pointed out, the invention of Mr. Ferranti, but Mr. Peck, although he admitted this, did not mention it in his paper. As to the value of the electrolytic arrester itself I have my doubts, and it will certainly be interesting to know how the device will be affected by climatic influences—low temperatures in northern parts, and humidity and heat in the tropics; further, whether the apparatus will withstand very heavy atmospheric discharges, representing very great masses of energy. In conclusion, I may say that there remains a great deal to be said on this subject which is not only interesting but of the utmost importance, and it would be most desirable if another paper treating it impartially could be prepared to supplement the present paper.

Mr. Watson.

Mr. E. A. WATSON (*communicated*): The immense number of gaps required in connection with the arresters of the multi-cylinder

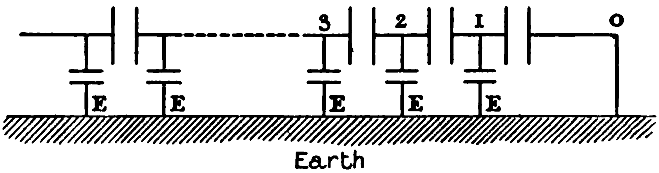


FIG. I.

type and the effect of the neighbourhood of the earth upon the number required may at first sight appear strange, but a little approximate calculation will show what a great effect the capacity of the cylinders relative to the earth has upon the distribution of potential between them. The multi-gap arrester may, for the sake of calculation, be considered as consisting of a number of condensers in series, the one end of the series being connected to earth and the other end to the line, while the junction of each pair of condensers is connected to earth by means of another condenser. Thus in Fig. I, 0–1, 1–2, 2–3, etc., represent the condensers formed by the cylinders themselves, while 1–E, 2–E, 3–E represent the condensers which the cylinders form with the earth.

For the sake of argument let us take the capacities of the condensers 0–1, 1–2, 2–3, etc., all equal and represented by C, and let us also take 1–E, 2–E, 3–E, etc., also equal and represented by λ C, where λ is some constant, which is generally less than unity. The capacity of the condenser formed by two cylinders is equal to $\dfrac{l}{9{\cdot}2 \log_{10} \frac{d}{r}}$ where $l =$ length of cylinders, $d =$ distance apart of centres, $r =$ the radius.

The capacity of an infinitely long cylinder relative to the earth is $\frac{l}{4{\cdot}6 \log_{10} \frac{2\,H}{r}}$ where H = distance from earth. As, however, our cylinders are not infinitely long the capacity will be greater than that given by the formula. Mr. Watson.

The minimum value of (1) is when the cylinders nearly touch—that is, when $d = 2\,r$ and $C = \frac{l}{9{\cdot}2 \log_{10} 2}$.

The maximum value of $\frac{H}{r}$ may certainly be taken as equal to 100, and is probably very much less. Taking it as 100, (2) becomes—

$$\frac{l}{4{\cdot}6 \log_{10} 200}.$$

The ratio λ will therefore be $\frac{l}{4{\cdot}6 \log_{10} 200} \times \frac{9{\cdot}2 \log 2}{l}$, which works out to about 0·26.

Thus we see that the "earth capacity" of a cylinder is of quite a large order even when the cylinder is at a considerable distance from surrounding objects. Knowing the value of λ, it is possible by a step-by-step process of calculation to arrive at the potential across each gap in terms of that across 0–1.

Let us take λ as 0·1 as a very extreme case indeed. Then the capacity of the point 1 relative to earth will be the sum of 0–1 and 1–E —that is, C (1 + λ) or 1·1 C. The potential across 1–2 will therefore be $\frac{1{\cdot}1\,C}{C}$ × potential across 0–1—*i.e.*, 1·1 *v*.

The potential between 2 and earth will therefore be 2·1 *v*, the capacity of 2 relative to earth will be $\frac{C \times 1{\cdot}1\,C}{C + 1{\cdot}1\,C} + 0{\cdot}1\,C = 0{\cdot}624\,C$, and the potential across 2–3 will be $0{\cdot}624 \times 2{\cdot}1 = 1{\cdot}31\,v$.

Proceeding in this way we can obtain the potentials across the various gaps, when it will be seen that this rises at a steadily increasing rate, as shown in the curve (Fig. J), being 1·65 for gap 3–4, 2·16 for gap 4–5, 9·88 for gap 9–10, 13·5 for gap 10–11, and so on, mounting up very rapidly.

It will, moreover, be noted that we have taken a much more favourable case than would ever actually occur in practice, *i.e.*, we have used the formula for an infinitely long cylinder, which gives too low a result, and we have taken λ as only 0·1, whereas 0·25 is nearer the mark.

This, then, is the reason why the multi-gap arrester on high voltages requires such an abnormal number of gaps if the gaps nearest the line are not to break down; placing shields near the gap and connecting to the line would no doubt be beneficial provided that the shields do not come too near the earthed end of the arrester, in which case the trouble would only be moved from one end of the arrester to the other. The correct way to treat the case would be to place the cylinders of the

Mr. Watson.

arrester midway between two shields, one of which is connected to the line and the other to the earth. This is then equivalent to placing the arrester in free space and completely cancels the injurious effects of the "earth capacity" of the cylinders, but I am not aware of its having been tried at all.

Leaving the subject of multi-gap arresters, there is a method of protecting high-tension lines which is, I believe, employed to some extent on the Continent, but is not mentioned in the paper. This consists in the installation of condensers suitably constructed to

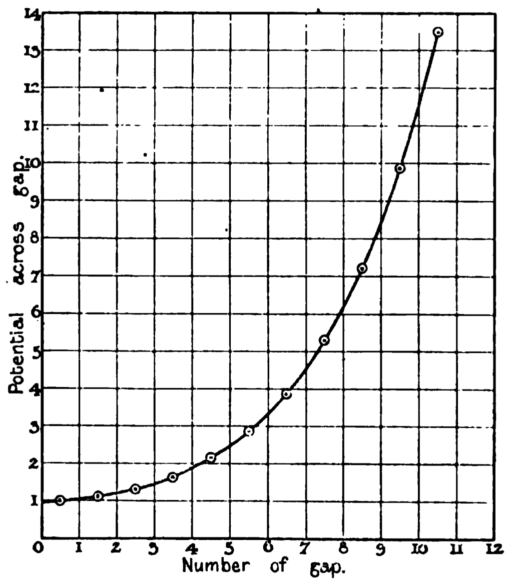


FIG. J.

withstand the voltage at the entrance to the power station and sub-stations. These condensers, while only allowing a small current to pass at the ordinary frequency of the supply, act practically as a short circuit to any high frequency discharge, such as that due to lightning, and prevent an excessive rise of pressure on the line. The idea seems good, but I am not aware to what extent it is carried into practice, and I should be glad of some further information on this point. In conclusion, I can only say that the paper has come at a very opportune time now that overhead work is coming into use in this country, and fills a very useful purpose in giving us some data on a subject to which British electrical engineers give but scant attention.

MANCHESTER LOCAL SECTION.

DISCUSSION, *February* 4, 1908.

Mr. C. D. TAITE: Mr. Peck has given us to-night a most interesting paper on a subject about which very little is known in this country, and upon which there is absolutely no standard practice whatever. I have recently had occasion to write round to several engineers in England who are dealing with overhead work, and I asked them what their practice was with regard to lightning arresters, and the gist of the replies that I received was to the effect that they had tried most arresters, they did not believe in any of them, and that they simply used them for the sake of convention. I think the reason why so little is known is not very far to seek. In the first place, very little overhead work has been carried out in this country at all, and in the second place, I do not think our thunderstorms are so severe as they are in America or on the Continent. Those lines which we have are very difficult indeed to protect, because we generally have to adopt a combination of overhead and underground. In a town, or even a country district, in which there are a large number of houses, the mains have to be taken underground, and then emerge overhead again, and when one comes even to a by-road the cables have to go underneath the road and up again the other side. To protect the lines properly, as cable makers say they should be protected, will be a very costly matter, and it becomes a very heavy burden on overhead construction. In the case of the company with which I am connected, we have a certain amount of overhead work, and I may say at once that we have never had the slightest trouble from lightning discharges. Our lines cannot be said to be protected in a very scientific manner: on one we have horn-break arresters, which have undoubtedly acted on frequent occasions at about 25,000 volts. We found plenty of evidence that sparks have passed across the horns when set at a distance requiring 25,000 volts to jump. On another line we have a multi-gap arrester, and I am afraid we do not know in what condition it is, being in a box out-of-doors and difficult to examine. On another line we have no arresters at all. Now the line on which we have had the least trouble is that on which we have the most overhead work; that is the one on which we have the horn-break arresters, and whether the freedom from trouble is due to the fact that it usually rains when there is lightning about, and that the discharge passes across the insulators or not, I cannot say, but there is not the slightest doubt as to my statement; our plans and records show clearly that the line with the greatest amount of overhead work is the one which is most free from all trouble. The line which is most affected by faults is the one on which there is least overhead work. On that we have had faults from time to time, and they always occur at the very end of the line underground. That line is the one protected by a multi-gap arrester where the cables go underground, but at the end of the line there was no path provided for lightning

Mr. Taite. or surges to find their way to earth; that has now been remedied by providing another multi-gap arrester there, and since then we have not had any faults at all.

Mr. Peck has given us some interesting information about this new arrester—the electrolytic—and I shall be glad if he will tell us whether it is a cheap arrester to manufacture. I should imagine from its construction it would cost very little, and might be of great use to us.

Dr. Rosenberg. Dr. E. ROSENBERG: I have to take only slight exception to the analogy given for resonance on page 504 of Mr. Peck's paper. It is stated that resonance occurs if the push comes every time towards the right when the wagon moves in the same direction. This is a little inaccurate. The oscillating force is always directed to the left when the displacement of the moving body is towards the right, and therefore resonance occurs if also an external force directed to the left is impressed for a displacement of the body towards the right. If a boat in its movement leans over to the starboard side, and one presses it further in the same direction, then the oscillation is not increased but mitigated.

As to the oscillograph in Fig. 22 showing the current through an electrolytic valve, I see that there is only a slight difference between the positive and negative halves. I should expect that the current in one direction is nearly suppressed, so that the difference in the two halves is much greater than that shown. I would ask Mr. Peck kindly to explain this oscillograph.

Mr. Chamen. Mr. W. A. CHAMEN: In South Wales we have very little indeed of overhead work at present. We have only one line over a mountain top, about 2 miles in length. Whether it has actually been struck by lightning or not we do not know, but we had a transformer broken down at one end after a thunderstorm, which looks rather suspicious; we had arresters on one end of the line only. The arresters we use are mostly of the horn type with water resistances; there is no further complication about them than that. Some sets are placed in the generating station, of course, and there are also other sets at two of the sub-stations at considerable distances. I am quite satisfied that they have saved us a great deal of trouble; but these horn arresters are in no case fixed on overhead lines, or out-of-doors; they are all under cover, and we have yet to learn whether we require anything more. We have had extremely little trouble up to now.

Mr. Cramp. Mr. W. CRAMP: I should like first of all to refer to page 498, where reasons are given why lightning arresters have not been worked out in this country. The second of these reasons, namely, that the great majority of transmission systems are laid underground and are therefore not subject to lightning disturbances, is hardly a good one, I think. For surely there is no transmission system, however far underground, that is not, somewhere or other, connected to an extensive surface system, and is therefore liable to be exposed to some sudden lightning strain that may be conveyed to any part of the underground system,

while the large capacity of such a system one would think would tend to accentuate the effects. Of course the earthing of the armouring of 3-core cables would tend to make any flash simply go across to the armouring, and possibly the puncture may tend to seal itself, so that no further trouble would ensue. Mr. Cramp.

Secondly, on page 507 Mr. Peck has made a remark about the insertion of series resistance which does not seem to me to be quite correct. At the bottom of page 507 he says, "It is now customary to instal a high resistance in series with this arrester in order to limit the current which flows on short circuit, but this resistance retards the static discharge." Surely the reactance of the discharge circuit due to a frequency which Mr. Peck admits is of the order of at least 100,000 a second, would be far in excess of any resistance placed in series, and one would think such resistance would be absolutely negligible, just as it is in the case of a lightning conductor.

Thirdly, there is a very curious remark on the effect of wind on overhead lines, namely, that the wind blowing across overhead lines tends to raise the potential of the lines as regards earth. Is this due to charged ions from the atmosphere being attracted to the wires, and is this effect found to be more marked on continuous-current systems than on alternating-current systems?

Next there is the question of the protection of lightning arresters. Lightning arresters are uncertain in action, and the reason so few of them are at work in this country is partly due to this uncertainty. Lines seem to be as well protected without them as with them; they do not seem to make much difference, and as long as that is the case engineers will not trouble to put them up. The reason seems to be that lightning arresters are too coarsely adjusted. The air-gap over which the spark has to take place to allow the discharge to pass to earth is so large that only very large rises of potential are at all allowed for. We are told, for instance, in one part of the paper that when very high potential differences occur, the necessity of using a vast number of air-gaps is such as to put the multi-gap type out of the question. Now we know that there is continuously a current passing between all the arrester cylinders to earth due to ionised particles; the result of this is that should there be any slight rise of pressure, a breakdown might occur at a much lower voltage than that for which the gap is set. The only way to get over this would seem to be to place the arrester itself in some kind of filled protection box, so as to minimise the effects of ionisation. I should like to know if such protected arresters have been used.

Lastly, I would say that Mr. Peck has very justly called attention to the difficulty of terminology in this subject, but he does not make it any better when he uses the word "static" as a noun, and talks about the "static escaping," as though "the static" were a sort of animal. The use of the term "concentrated potential" seems also open to question. I would plead for the sake of those who have to teach these subjects that such use of terms should be avoided as far as possible.

The Chairman.

The CHAIRMAN (Mr. M. B. Field): The potential strains generated by the various effects that Mr. Peck has dealt with in his paper, such as switching, grounding, etc., may be classified broadly under two heads: (1) Effects which produce a limited rise of potential, and (2) effects which produce an unlimited rise of potential. Under the first heading fall such effects as the switching of a cable on to a generator, the effects of reflected waves and such like, the rise of potential in these cases being limited to twice the normal potential.

Now, these rises of potential are of an extremely transitory nature, and we know that whether a high potential difference will break down any insulation depends very much upon the time during which it is applied. Thus some insulation will stand, say, 20,000 volts if applied for only $\frac{1}{50}$th part of a second, whereas it might break down with 5,000 volts applied for a sustained period.

Considering, therefore, the very transitory nature of these high potential effects, it is quite possible that where the rise of potential is limited, they do not seriously strain the insulation of the system, and this, to my mind, is a very consoling reflection.

The other effects, such as breaking inductive circuits, power surges, atmospheric effects, etc., produce rises of potential which are more or less unlimited. We are all acquainted with the rises of potential which are produced on suddenly breaking an inductive circuit through which a heavy current is flowing—breaking the shunt circuit of a dynamo is a very good instance of this—but in alternating current work we are very fortunate in having an automatic protection in this respect in the oil circuit breaker. The properly constructed oil circuit breaker has the peculiar characteristic of interrupting the circuit at or near zero current, and for this reason no difficulty is experienced in practical work in interrupting heavy inductive loads on alternating-current systems by means of oil circuit breakers.

In my opinion, therefore, the effects of power surging, atmospheric effects and lightning are the most important ones of large systems which are to be guarded against. The whole subject of power surges is very difficult, and I do not propose to touch upon it now. A remarkable instance was described in the *Transactions of the American Institute of Electrical Engineers*, in 1901, vol. 24, p. 579, in connection with the Manhattan System. This gives one some insight into what can happen on a large system in the way of rises of potential when something occurs to bring a heavy power surge into existence.

Mr. Cramp has already referred to atmospheric effects on overhead lines. I have also noticed such effects, and perhaps I can partly answer his question. I have noticed in water-power stations from which a considerable number of overhead lines emerged, on some evenings when the sky was apparently clear and no trace of lightning visible, that the lightning arresters would spark over. This was by no means an uncommon experience, and I put it down to the atmosphere becoming suddenly charged, due to rapid cooling. I do not know if this be the right explanation or not, but I suggest it as within the

realm of possibility that a warm current of air blowing across the river on which the power station is situated becomes suddenly chilled, and the result of the internal commotion brought about by contraction is to electrify the atmosphere, and the overhead wires collect the charge from the atmosphere, thus raising the system to a sufficiently high potential to set the lightning arresters in operation. The Chairman.

I would like to say a few words with regard to the concentration of potential difference over the first turns of transformers and motors. Mr. Peck has explained this action in a very simple and pretty way, and his explanation seems perfectly clear; but it is not altogether clear to me why the action should stop at the first few turns—why, in fact, the abrupt potential wave does not travel from one end to the other of the winding, thus straining all portions to the same extent. The curves which Mr. Peck publishes certainly show that this is not the case, and I should like to ask him if he has any explanation of the matter. Mr. Peck has explained the concentration of potential difference as being due to sideways capacity between the turns and earth or the other pole of the system, and he suggests to do the equivalent of putting these turns outside the transformer altogether; that is, to put in series with the transformer a choking coil, so that the concentration of potential difference will occur in these external turns. If, however, we look at Fig. 7 we see that the choking coil proposed does not correspond to the initial turns of the transformer as regards sideways capacity. There is, it is true, capacity between turn and turn, but I do not see that any provision has been made to imitate the effect of capacity between each turn and earth, and I would ask whether the shape of choking coil shown in Fig. 7 is the best shape for the purpose of curing the evil—whether, in fact, an improvement would not be brought about by introducing a plate either connected to earth or to the other pole of the system in the neighbourhood of the choking coil and parallel to its plane, so that a considerable sideways capacity effect would be introduced.

About twelve years ago I had considerable trouble with the breaking down of the windings of high-tension motors in this way, and I inserted flat disc choking coils just before the motor winding, in the way Mr. Peck suggests. These choking coils consisted of a flat copper ribbon wound up spirally with a strip of paper between turns. The copper strip was exactly of the same width as the paper strip. Thus there was every opportunity of sparking across the edge of the paper from turn to turn in the choking coil. These choking coils, however, did not appear to be in any way effective, and at a later date, when I began (as I thought) to understand the phenomenon better, I attributed it to the want of sideways capacity, as explained. At the same time, I am by no means sure that this is the explanation, and it would appear to be somewhat negatived by the fact that the choking coils Mr. Peck illustrates, which also have very little sideways capacity, apparently are beneficial in eliminating these concentration effects.

Mr. H. W. WILSON: Several speakers have said to-night the Mr. Wilson.

Mr. Wilson. amount of acquired ignorance on the subject of lightning arresters in this country was very great, and the only station I know of where they have any considerable number of them is the North Wales Power Scheme, where they have spark-gap arresters on the lines themselves and horn-break arresters in the station, and I should think they probably get more thunderstorms per square mile there than anywhere in Great Britain. The station superintendent there told me that their lightning arresters were in operation fairly frequently. Mr. Peck has to-night detailed very clearly the different types of lightning arresters, but has carefully refrained from saying which is the best, and I think it would be of distinct interest if he would say which he really considers to be the most satisfactory for use in this country. I should also be glad of some information as to the voltages at which the various types can be used. The only statement Mr. Peck makes is that the multi-gap arrester is in use on circuits of 2,000 volts and at voltages as high as 15,000 to 20,000. He also says the other type has been used for as high as 66,000 volts. I should like to know whether there is any definite information available as to the best type of lightning arrester for the voltages which we expect to get in this country—something under 10,000. Another point upon which there is a very considerable difference of opinion is the frequency with which these lightning arresters ought to occur on the system. I should be glad to learn whether there is any accepted practice as to the length of line which one lightning arrester is supposed effectively to protect.

Mr. Watson. Mr. E. A. Watson: I should like to ask Mr. Peck with regard to the case mentioned where a static charge became accumulated by the action of the wind. I suppose that only applies to the case of a line disconnected at both ends at the same time, or else to a system where the neutral point is not earthed. In the ordinary case, with an earthed neutral, for a charge of such a low frequency the resistance of the machines or transformers will allow the charge to escape to earth without excessive voltage rise, and the inductance of the machines is not liable to cause trouble by breaking down the end windings, as in the case of a high-frequency disturbance. With regard to the water-jet arresters, I should like to ask whether there is any action analogous to that of an ordinary lightning arrester, where the resistance drops greatly when the line is struck; if they act simply as an ordinary non-inductive resistance, the waste of current which would be incurred, were this made low enough to ensure efficient protection, must be very large indeed.

Mr. Slacke. Mr. R. B. Slacke: With reference to the water-column arrester on page 511, the overflow pipes do not seem to be required. Surely if the thing was once filled it would last some considerable time, and if there was an air-gap in series with the arrester the water would act as a sort of resistance and serve its purpose without a continuous supply of water. Is there supposed to be a head of water in that case which keeps the level always above the overflow pipe?

Referring to the gap arresters, I should like to know how the number of gaps is calculated for any given voltage.

DUBLIN LOCAL SECTION.

DISCUSSION, *February* 13, 1908.

The CHAIRMAN (Mr. T. Tomlinson): The lightning arresters described in the paper are really a kind of safety valve, and one may cite as an analogous case a certain form of mechanical stoker, which has at times been known to jamb, with the result that owing to the large amount of power behind, something had to go, and the makers therefore inserted an intentionally weak pin in one of the pawls, which, whenever the machine jambed, broke, preventing greater damage. The Chairman.

Mr. W. TATLOW: The author has given an exceedingly lucid explanation regarding the rise of potential between adjacent turns of transformer coils at the instant of switching on. I would like to ask whether the overhead earthed line which is usually erected is being superseded in favour of arresters. I should also be glad to know whether the choke coil inserted between the line and transformer is kept permanently in circuit, or only is used for switching operations; if the former, I imagine there would be entailed a certain amount of loss. I should further like to know whether the horn type of arrester is not liable to set up oscillations when it operates. It appears that the Wurtz type arrester is only applicable to alternating circuits, therefore I presume that it is necessary in the case of direct-current circuits to use either the horn or electrolytic type. It would be interesting to know what is the solution used in electrolytic arresters, because I have found that with the Nodon valve, in which phosphate of ammonia is used, the aluminium plates are attacked; I have found that with tungstate of soda the film is capable of withstanding a high pressure. Mr. Tatlow.

Mr. A. T. KINSEY: I think that telegraph and telephone engineers in adopting the devices described by the author often create more troubles than they are trying to avoid. It is now standard practice to earth every pole and arm, so that if a rise of pressure should occur on any section of the line, the current can get to earth immediately, without traversing any length of line, which helps them very considerably. I have known a case where cattle have been killed at the foot of the pole while the wire remained intact. I am rather surprised that the author has not said one word about the fuses used in connection with spark-gap arresters; often after lightning these fuses scatter over miles, and miles of line are blown, causing serious inconvenience. I have sometimes found that in the case of the little spark-gaps, consisting of glass tubes with platinum points, when a spark passes the platinum gets deposited on the glass. Regarding transformer breakdowns, we use small transformers in connection with superimposed telephone circuits, and I have had many breakdowns between windings. Further, I can fully endorse the author's statement as to the troubles likely to follow the use of circuits, partly overhead and partly underground, at the points where the underground joins the overhead system. Mr. Kinsey.

Mr. Harriss.

Mr. G. MARSHALL HARRISS: The necessity for the use of such devices has not been brought home to us in this country. I have, however, had some little trouble due to lightning, and am pleased to say how well the Wurtz arrester worked; I have stood and watched it act during a thunderstorm, when it responded to almost every flash. I am of opinion that there is no apparatus able to deal with a direct stroke of lightning; if a pole is struck it immediately splinters into matchwood, and no device can prevent that. Mr. Peck has mentioned that this difficulty is due to the great suddenness with which a direct stroke acts. I would rather say it is due to the enormous potential and high frequency. Professor Forbes once suggested to me that if the line was taken one complete turn round the building it would probably (owing to the choking effect of even one turn at so high a frequency) prevent the charge from entering the buildings and reaching the transformers or machines.

Mr. Tweedy.

Mr. R. N. TWEEDY: In tramway work it is generally the custom to insert a choking coil between the bus-bars and the incoming line, and I consider that the freedom from damage due to lightning is largely due to chokers and the arresters also installed. From my experience any arresters having moving parts are entirely useless under severe conditions. I would like the author to state what steps are taken in America to safeguard pedestrians walking under the high-tension transmission lines; judging from the illustrations shown by the author, there is a distinct element of danger. In England the Board of Trade insists upon elaborate earthed cradles.

Mr. Kettle.

Mr. KETTLE: Many engineers have had painful experience of the destructive effects of pressure surges which occur on switching in high-tension induction motors and transformers. The resultant breakdowns invariably take place between the individual wires of some of the terminal coils. These individual wires are, of course, only very lightly insulated from each other, although the coils are heavily insulated from the core. With regard to Mr. Tatlow's remarks about the inability of the Wurtz multi-gap arrester to deal with direct-current lines, Wurtz has also invented a direct-current arrester, consisting of two metal electrodes embedded in a marble slab, the gap between them being bridged by the charred surface of a piece of lignum vitæ, which is in turn covered by another marble slab. The gap between the electrodes holds back the line pressure without appreciable leakage, but breaks down under pressure surges. I should like to know from Mr. Peck whether the electrolytic arrester is effective for direct current. In his paper Mr. Peck has dismissed in a few words the water-jet arrester, which is used largely on the Continent in conjunction with a combination of horn arrester and series water resistance. This arrangement has been found very effective, but Mr. Peck condemns it on account of the leakage and the need of water pressure. He has also in his remarks cited the fact that the water-jet arrester always has a horn arrester in parallel with it as evidence that its users have no confidence in it. This is not exactly fair criticism. The electrolytic arrester which Mr. Peck

advocates needs, according to him, another arrester in series with it. In fact, it seems as if the electrolytic arrester performs the functions of a series resistance rather than that of an arrester proper, so that in that respect, at any rate, it does not score very much over the water-jet arrester. Besides, the water-jet arrester and the combined horn arrester and series resistance perform different functions, the water-jet smoothing down the pressure surges, which may not be of sufficient magnitude to jump the horn gap. The two are thus something like the ordinary and the emergency governors of an engine. The power needed to supply water for the water-jet arrester is a small matter at generating or sub-stations, as where transformers are in use water would probably be used for cooling these. Whilst on the subject of leakage I should like to know whether Mr. Peck has any figures showing the value of the leakage on the water-jet, and also the comparative leakage on the electrolytic arrester, if it were at all practicable to use it alone. Mr. Peck has mentioned as the special feature of the electrolytic arrester that when it comes into action it loses nearly all its resistance, and becomes practically a short circuit, thus affording a free passage for the discharge. This certainly makes it a very efficient substitute for the ordinary series resistance, but what would be the effect of two of these arresters discharging simultaneously? Would it not constitute a short circuit on the line, and produce effects similar to two horn arresters without series resistances? Mr. Kettle.

Mr. PECK (*in reply*): The composition of the electrolyte is somewhat in the nature of a trade secret, and consequently the company which has brought it out is not very anxious that the information should be made public. I am sorry, therefore, that I am unable to state what the electrolyte is, nor what is the method of treating the plates to insure the continuity of the film. Mr. Peck.

The volts per tray vary from 380 to 400, but it takes some little time to treat the plates. At first they will stand only about 100 volts, then, as the process of treating is continued, this is increased to 380 or 400. The electrolytic arrester can be used without a gap, I believe, but it takes a charging current. The film is about the thickness of a wave-length of light, so that although there may be a large number of films in series there is still a thin dielectric, so that the charging current is quite heavy, and it is to prevent the charging current from heating the arrester that the gap is used. The particular type of horn arrester mentioned by Dr. Kapp is new to me, but it looks as if it might work very well for fairly low voltages. With high voltages the difficulty is to get a long enough air-gap, and what is wanted is some device which will permit the air-gap to be reduced. Dr. Kapp has shown also a very nice method of charging a cable, but there are a great many difficulties in working from point to point with 60,000-volt transformers. Mr. Esson has referred to the voltage which it is possible to get when opening a short on a transmission circuit. Of course, the energy in an inductive circuit varies as the square of the current; therefore, although the voltage may go down if the current goes up,

Mr. Peck. there is a higher voltage on breaking. The maximum voltage rise is obtained upon breaking the current instantaneously at its maximum point. Different people hold different opinions with regard to the value of lightning arresters; many say they are no good, but few dare operate without them. I suppose an air-gap is an objection in any arrester, as a discharge through air is always a very uncertain thing, and an air-gap in any circuit introduces an element of uncertainty; but in the case of high-voltage circuits this is so small as to be almost negligible. Regarding the coherer type of arrester, there is an arrester on the market for direct or alternating current from 500- to 1,000-volt circuits. I have myself had a good deal to do with the development of this, but the original idea is due to Mr. Thomas, who, however, had no idea of the coherer action at the time. His idea was to take a great number of very small filings and to mix them up with cement or similar substance to make a solid block, with a plate at the top and the bottom, so as to get a discharge jumping from metal to metal. This would give a great number of minute air-gaps in series, so that it would withhold the low voltage and not permit an arc to continue. After working at it for some months and finding that the metal filings were not very satisfactory, he took ground coke. This worked very well, but after a number of discharges the resistance would change and the block break down. It was then found that by using carborundum a block $1\frac{3}{4}$-in. thick could be made, which would easily withstand 1,000 volts. It had an equivalent air-gap of about $\frac{1}{10}$-in., and showed very great protective power. With regard to electrolytic arresters, I believe Mr. Ferranti was the first to suggest the use of aluminium. Mr. Patchell has referred to testing arresters with large machines. Arresters are usually tested on very large condensers, and with the full voltage of the circuit on which they are to be used, so that when the condenser is discharged a very heavy static discharge passes through the arrester, and there is always plenty of power behind it. I do not think there is much value in the use of barbed wire over ordinary wire, as suggested by Mr. Mordey. Mr. Andrews has given some interesting information about the horn type of arrester. I knew that the action was partly magnetic, but I thought it was principally a heat action. In reply to Mr. Fynn, the curve in Fig. 1 was obtained by putting the spark-gap in shunt to the winding, and then operating a simple swing-closing switch probably several hundred times for each gap. The results varied considerably, but by plotting them and drawing a smooth curve, a very fair average was obtained.

Mr. Taite asked for information regarding the cost of the electrolytic arrester. I am not able to give figures as to the cost of this arrester, but from its general appearance should not expect it to be very much more expensive than the low equivalent arrester. The cost of an arrester of this type is not measured by the actual labour and material required for building it. The electrolytic arrester has been experimented with for about three years, and very heavy expenses

have been incurred in its development. It is customary to test apparatus of this kind in the laboratory until it is found to be as perfect as it is possible to make it, then to place sample arresters in actual service, and thus give the arrester a service test before it is put on the market. Mr. Peck.

Dr. Rosenberg referred to the curve on the electrolytic arrester. This curve is upside down, as the current should lead the voltage, as it is principally a charging current. The ripples on the current wave are apparently due to the current breaking down the film on account of the critical voltage being exceeded.

Mr. Cramp stated that underground systems were not entirely free from lightning effects, as they must come to the surface at some point, where they would be exposed to lightning disturbances. This is, of course, true to a very limited extent, but the exposed portion is usually so small, and as it is often protected by buildings, there is very little danger as compared with an overhead circuit which extends for miles through uninterrupted country. There is, of course, a certain amount of self-induction in the ground wire, but this inductance does not appear to be sufficient to retard the flow to earth to any great extent. Mr. R. P. Jackson* states that there is a critical resistance which should not be exceeded if a free passage to earth is desired. His argument appears to be sound. His reasoning is as follows: A static wave travels along a transmission line at approximately the velocity of light. The capacity of any transmission line can be calculated with some degree of accuracy. If therefore we assume that the line is charged by the static wave to a certain potential above earth, we know definitely the amount of current which it holds as a condenser and also the rate at which this current can arrive at the arrester. If the resistance is such that the voltage of the static discharge cannot force the current through the resistance as rapidly as it arrives at the arrester, there will be an increase in voltage at this point. It is therefore desirable that the resistance be kept so low that no increase in potential will occur.

Regarding the action of wind on transmission lines, I am not able to say how the results which are obtained are really produced.

It is, of course, necessary to set lighting arresters at a potential considerably in excess of the normal line potential, which means that the insulation must be able to withstand a considerable momentary rise in potential. As Mr. Field has pointed out, insulation will stand a momentary strain very much in excess of that which it will stand if applied continuously.

Mr. Field asks why this concentration of potential does not spread to the interior of the winding. The outer turns are raised quickly from zero to line potential, the charge passing into the interior of the coil, bringing up the whole coil to line potential. When voltage is first applied, the potential is concentrated upon a few layers, and as the charge advances toward the interior of the coil, the number of layers

* *Transactions o the American Institute of Electrical Engineers*, vol. 25, p. 881, 1906.

Mr. Peck. over which the potential is concentrated gradually increases with a consequent diminution in voltage between adjacent layers. When a choke coil is placed outside the apparatus, it acts as a buffer, so that the outer turns of the winding are not raised instantly to full potential on account of the time required for the charge to pass through the coil. In the same way, when there is no choke coil, the outside turns tend to retard the passage of the charge into the interior of the winding. The general design of the choke coils shown in Figs. 7 and 8 was determined after a very large number of experiments, and these coils appear to give a sufficient amount of protection for apparatus which is properly insulated.

Mr. Field's suggestion regarding the advisability of increasing the capacity of the choke coils was appreciated by Mr. Thomas, who invented what he called a "static interrupter," which consisted of a choke coil and a condenser, the condenser being placed between the choke coil and the apparatus which it protected. Any discharge not stopped by the choke coil would enter the condenser, and since time is required to charge the condenser, there would be no abrupt change of potential on the end windings of the apparatus; in other words, the condenser intensifies the effect of the choke coil without increasing the inductive drop in the circuit.

I know that Mr. Ferranti was one of the first to experiment with the aluminium cell, but I am informed that he did not attempt to use it as a lightning arrester, but rather as a device for suppressing arcs on switches.

Mr. Wilson asked me as to the best type of arrester for use in this country. If I wished the best possible protection, I think I should instal the electrolytic arrester. Next to this, the low equivalent type of arrester has probably greater protective power than any other.

Regarding the number of arresters per mile, this depends upon the extent to which the line is exposed and upon the number of points at which apparatus is connected to the line. For long transmission lines it is customary to instal arresters only at generating and sub-stations, but for single-phase railways or for direct-current railways the arresters should be scattered thickly over the whole system, say 4 or 5 per mile.

With regard to water-jet arresters, it is necessary to have running water, as a simple tub of water would heat and probably give trouble from various causes.

Regarding the determination of the number of gaps in a multi-gap arrester, this is largely a matter of experience. The first arrester for 1,000 and 2,000 volts had a certain number of gaps. When the voltage was doubled the number of gaps was doubled, and if this was not enough to hold back the line voltage, more were added. A very great amount of experimental work, both in the laboratory and in actual service, has been done in connection with this type of arrester.

I agree with Mr. Tomlinson's simile regarding the function of the lightning arrester, which is, as he has said, really a safety-valve to the system to which it is connected. In reply to Mr. Tatlow, the ground

wires alluded to give trouble owing to rusting at the joints, but now much greater care is taken in their erection than formerly. The choke coil is always in circuit with the transformer or other apparatus which it is designed to protect, because disturbances, such as earths, short circuits, etc., may occur at any time, and by keeping the choke coil in circuit continuously, the apparatus is always protected. I agree that the horn-type arrester may in some cases cause oscillations in the circuit. The Wurtz arrester is only suitable for alternating circuits. Regarding the permanency of the electrolytic arrester, I know cases where they have been in service over one year, and no change whatever in the film has been found. The horn type of arrester is not, in my opinion, very satisfactory on circuits of 15,000 volts or less, as the arc does not rise quickly enough. Many engineers have expressed an opinion similar to that of Mr. Kinsey, but at the same time trouble due to lightning, etc., is so serious, and the cost of protective apparatus so small, that few engineers cared to operate without them. The efficacy of the multi-gap arrester has been tested frequently by inserting pieces of paper between the spark-gaps and examining them from time to time. They are usually found punctured after a storm. Regarding the transformer breakdowns mentioned by Mr. Kinsey, I rather think that many telegraph engineers do not appreciate the necessity of thorough insulation for high voltages. I remember one case in particular in which transformers and other apparatus are repeatedly being damaged. The complaint is that the arresters are useless for their purpose. Subsequent investigations have shown that the insulation of the apparatus is at fault, and on its being increased the arresters have acted perfectly, it being, of course, essential that arresters should be set to act at a pressure considerably above that of the line. Regarding overhead high-voltage transmission lines in America, these are usually run through the supply undertakers' own property, a sole right of way being acquired. Overhead transmission lines for very high voltages are not carried through city streets where it can be avoided. No precautions are taken on the undertakers' own right of way, as only employees should be there, and they are aware of the existing danger. The water supply for water arresters are usually turned off in good weather, and only turned on when storms are anticipated. Mr. Peck.

The PRESIDENT: Gentlemen, you have already showed in the usual manner your appreciation of the paper the author has given us, but I have to ask you to pass him a formal vote of thanks for the paper he has read. The President.

The resolution of thanks was carried by acclamation.

The meeting adjourned at 9.55 p.m.

MANCHESTER LOCAL SECTION.

THE RELUCTANCE OF THE AIR-GAP IN DYNAMO MACHINES.

By THOMAS F. WALL, M.Sc., B.Eng., Student.

(*Received from* MANCHESTER LOCAL SECTION, *November* 11, *and read at Manchester, December* 10, 1907, *and at Birmingham, February* 12, 1908.)

There appears to be, so far as I am aware, a scarcity of experimental data concerning the reluctance of the air-gap in dynamo machines with slotted armatures, and also as to the distribution of the flux over the surface of the pole shoes and on the surfaces of the teeth themselves. Although the estimation of the reluctance of the gap is of great importance in the design of machines, the only experiments which appear to have been made in this direction are those of Hele-Shaw, Hay, and Powell.* In the following pages the results of some measurements are described which were carried out this year in the Electrotechnical Institute, Karlsruhe.

These measurements have been employed to find the increase of the reluctance of the air-gap due to slotting the armature, and a comparison has been made of the values so found with those given by the methods generally used.

The apparatus was specially designed and made for these experiments, and is shown in Fig. 1. It was made in two parts, one part built up of iron laminations and containing the exciting coils, which will hereafter be referred to as the pole-piece, the other part being simply a rectangular piece of laminated iron slotted on one side and smooth on the opposite side. This part will be referred to as the armature in what follows. All the slots were of the same dimensions, namely, 1·5 cm., and the teeth were made so that two were 1 cm. wide, two 1·5 cm. and 1·53 cm. wide respectively, two 1·95 cm. and 1·96 cm. wide respectively, and one 3 cms. wide. These parts were used together in the following way: When it was desired to measure the flux distribution on the pole-piece when used with the slotted armature the latter was placed on the pole-piece with the slot opposite the smooth portion A B. The air-gap could be varied at will by inserting plates between the armature and pole-piece at C and D, each plate being 2 mm. thick. When the flux distribution on the smooth surface of the armature was to be found the pole-piece was turned over and the armature laid on it with

* *Journal of the Institution of Electrical Engineers*, vol. 34, p. 21, 1905.

the smooth side opposite the large polar projections. The air-gap could be varied as before.

In order to explore the field a search-coil was made of 2 mm. width and having two turns. The sides of this coil were laid in grooves in

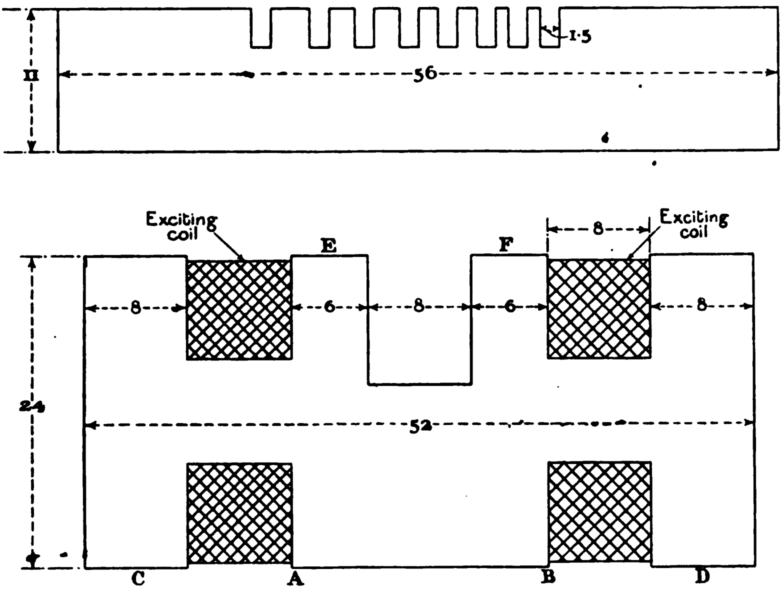


FIG. 1.—Experimental Model for the Determination of the Flux Distribution on the Flanks and Surfaces of the Teeth.

Dimensions in Centimetres.
Width of Apparatus perpendicular to Plane of Paper = 11 cms.

a strip of vulcanite, and flush with the surface (see Fig. 2). By this arrangement the coil could be placed on the surface of the pole-piece or the surface of the armature as desired, and by means of ballistic measurements the distribution of flux could be completely examined. The ballistic throw was measured when the circuit of the exciting coils

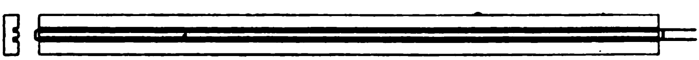


FIG. 2.—Search Coil.

was broken, this having been found to give the most satisfactory results. The galvanometer had a period of swing of about 35 seconds.

It was found that if the current in the exciting coils was large enough to cause much temperature rise in the iron, the change in permeability thus produced was sufficient to affect the throw, and

the measurements had to be taken in every case when the iron had assumed a steady temperature.

I.—Experiments on the Surface and Flanks of the Polar Projections.

Experiment 1.—This experiment was made to examine the flux distribution on the surface and flanks of the polar projections E F (Fig. 1), and also the corresponding flux distribution on the smooth surface of the armature. Although in practice pole-tips would never be so sharp as in this case, but would be more or less rounded off, yet these polar projections may be looked upon as large teeth, and the curves obtained will be similar to those for teeth of more normal

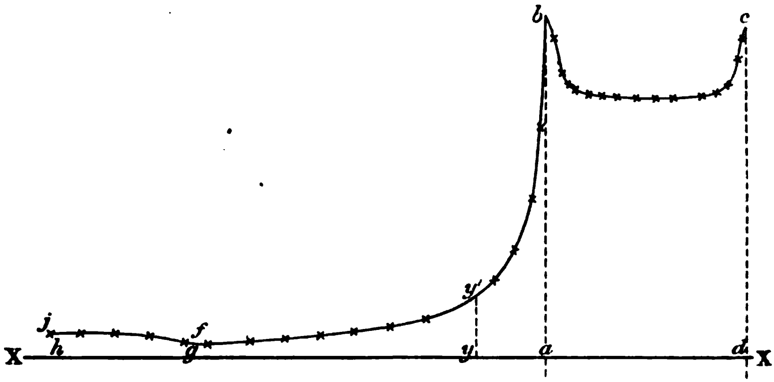


Fig. 3.—Curve showing Flux Distribution on Pole Face and Flank.

Air-gap = 1 cm. Width of Pole Face = 5·8 cms.
Length of Pole perpendicular to Plane of Paper = 11 cms.
Average Flux Density over Pole Surface = 16,000 lines per cm.².

dimensions, but having the same ratios of width of tooth to width of slot and width of tooth to length of gap.

In this experiment—

$$\text{Length of the air-gap} = 1 \text{ cm.}$$
$$\text{Width of polar projection} = 5{\cdot}8 \text{ cms.}$$
$$\text{Ratio } \frac{\text{width of pole}}{\text{length of air-gap}} = 5{\cdot}8 \text{ cms.}$$

Fig. 3 shows the flux distribution curve on the surface and flank of one of the polar projections. The characteristic feature of this curve is the large peak at each pole-tip. This indicates, of course, that the flux is much denser at the tips than at the middle of the pole surface. The explanation of the existence of these tips may be looked for in the fact that the flux tends to spread out on the armature surface in order to take up the path of least reluctance, and thus concentrates on the

pole surface at the tips. The case is, in fact, similar to that of electrified metallic plates separated by an air-gap, the density of the electric charge being greatest at the corners or edges. The peak on the tip adjacent to the existing coil is somewhat different to the peak at the other tip owing to the presence of this coil and the consequent dissymmetry of the arrangement.

Fig. 4 shows the curve of flux distribution on the smooth surface of the armature. This curve has the shape usually shown, and it is seen that the flux density at a point immediately opposite the pole-tip is much smaller than it is at a point under the middle of the pole. The point at which the flux density falls off rapidly appears to be at a distance equal to the air-gap, from the point just under the pole-tip.

The average flux density at the surface of the pole was about 16,000 lines per cm.², and the ratio of the maximum density at the pole-tip to

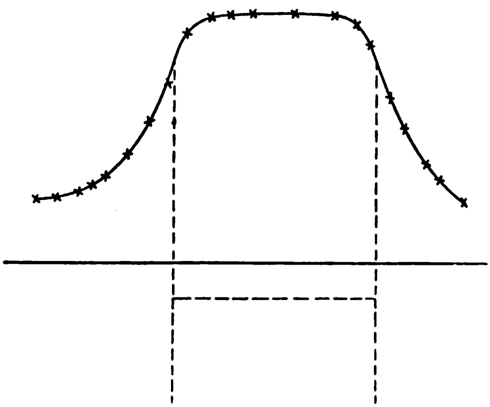


FIG. 4.—Curve showing Flux Distribution on Surface of Armature.

Air-gap = 1 cm. Length of Armature perpendicular to Plane of Paper = 11 cms.
Average Flux Density under the Pole ≏ 16,000 lines per cm.².

the density at the middle of the pole was found, by measuring the curve, to be 1·32.

Experiment 2.—This experiment was similar to the previous one, except that the air-gap was smaller and the flux density much less. In this case—

Air gap = 2·3 mm.

$$\text{Ratio}\ \frac{\text{width of pole}}{\text{length of air-gap}} = 25{\cdot}2.$$

The flux density under the pole was approximately 5,000 lines per cm.².

The curve for the distribution of flux on the polar face shows again the peaks at the pole-tips, although they are now very much smaller than was the case in Experiment 1, with a large air-gap. This

indicates that the peaks are dependent on the size of the gap, or, more correctly, on the ratio of slot width to gap.

The curve of flux distribution on the armature surface has the usual shape, and the point at which the flux density begins rapidly to fall off is at a distance equal to about twice the air-gap from the point on the armature just opposite a pole-tip.

It is interesting to note from these curves that with a large gap the maximum flux density on the pole-face is greater than the maximum

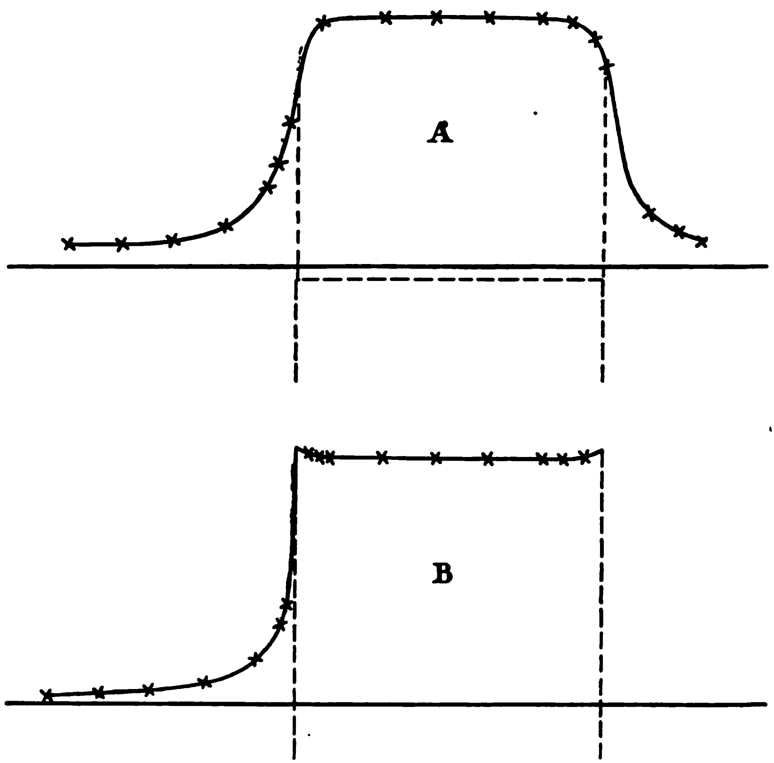


FIG. 5.

Air-gap = 2·3 mm.
Flux Density (average) at the Surface of the Pole about 5,000 lines per cm.².
Width of Pole Face = 5·8 cms. Length of Pole perpendicular to Plane of Paper = 11 cms.

flux density on the armature surface. With a small gap the maximum flux density on the pole-face is about equal to the maximum flux density on the armature surface. The curves also show how much flux leaks out of the flanks of the pole and also how much passes from the root of the gap between the poles.

In Fig. 3 the part *a b c d* gives the flux issuing from the polar surface, the part *a b f g* represents the flux issuing from the pole flank,

and the part $f\,g\,h\,j$ shows the flux issuing from half of the root of the gap between the polar projections. Thus, the ordinates of the curve $b\,j$ give the flux density at the various points of the flank measured from X X as abscissa. For example, at a point 2 cms. down the pole flank the flux density is measured by the ordinate $y\,y'$ where $a\,y = 2$ cms. This method of plotting the curves has been adopted throughout.

II.—Experiments with the Slotted Side of the Armature and Smooth Side of the Pole-piece.

In these experiments the flux distribution on the surface of the pole-piece due to the armature teeth was ascertained for various values of the air-gap, and also the complete flux distribution over the surfaces and flanks of the teeth. This was done, as in the previous experiment, by placing the search coil at various parts of the surfaces of the armature and pole-piece.

Experiment 1.—

Air-gap = 2·3 mm.

Ratio $\dfrac{\text{slot}}{\text{gap}} = 6{\cdot}5$.

Ratio $\dfrac{\text{tooth width}}{\text{gap}}$ for the various teeth = 4·35, 6·51, 6·66, 8·48, 8·52, 13·05.

Average flux density on the tooth surface; 5,000 lines per cm.^{2}

The curve in Fig. 6 shows the flux distribution on the pole surface. It is seen that the density varies very considerably at points under the teeth and under the slots. This really gives the case of the flux distribution on the surface of the pole in a direct-current machine when the slot and gap relations are as shown. As the teeth pass across the surface of the pole shoe this wave moves with them and corresponding eddy currents are set up. It is noticeable that if the tooth is too narrow relatively to the air-gap the flux wave never becomes flat under the tooth.

The points at which the curve of flux density on the pole-piece surface begins rapidly to diminish is approximately at distances from the tips of the teeth about equal to the width of the gap. The curves for the pole-piece surface have been measured, and the results are as follows :—

Curve I.—Area = 1,460; mean height = 57·1; maximum height = 88·5; minimum height = 31·5.

Curve II.—Area = 1,927; mean height = 66·3; maximum height = 90·5; minimum height = 31·8.

Curve III.—Area = 2,004; mean height = 64·5; maximum height = 90·2; minimum height = 32·0.

Curve IV.—Area = 2,356; mean height = 67·3; maximum height = 91·0; minimum height = 31·8.

In Fig. 7 the flux distribution on the surfaces and flanks of the several teeth has been plotted—the air-gap and flux density being the same as for the curves of Fig. 6. These curves in Fig. 7 have been plotted in precisely the same way as in Fig. 3, and the method has been already fully explained.

The characteristic feature of these curves is the high flux density at the tooth tips. Thus in Curve I., Fig. 7, the density at the tooth tip is greater than the density at the middle of the tooth surface by about 10 per cent. Moreover, the flux density at a point at the middle of the tooth surface is greater than at the point just opposite on the pole-piece, and the same features can be observed for all the teeth.

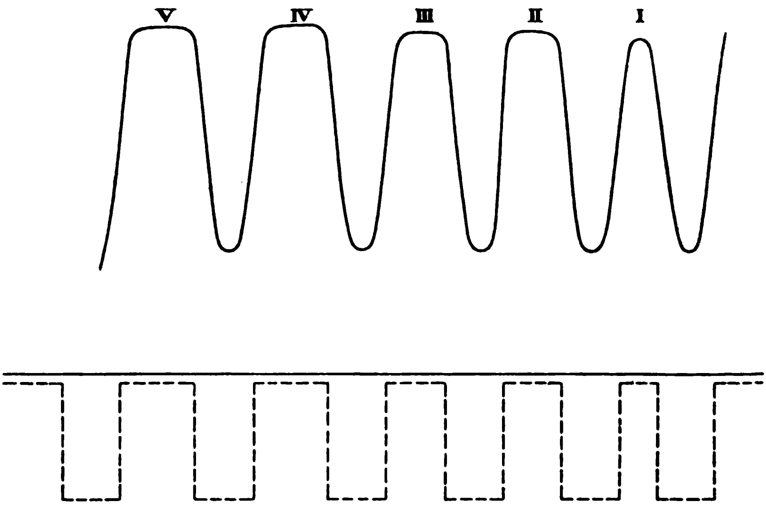


FIG. 6.—Flux Distribution Curve on the Smooth Surface of Pole-piece.

Air-gap = 2·3 mm. Average Flux Density under a Tooth ≏ 5,000 lines per cm.².

The curves also show to what extent the flux leaks out from the flanks of the teeth into the pole-piece.

Curve I.—Area = 1,535; ratio, flux density at tip to flux density at middle of tooth surface = 1·11; minimum height for the surface of the tooth = 90·6.

Curve II.—Area = 1,944; ratio, flux density at tip to flux density at middle of tooth surface = 1·08; minimum height for the surface of the tooth = 91·8.

Curve III.—Area = 2,019; ratio, flux density at tip to flux density at middle of tooth surface = 1·07; minimum height for the surface of the tooth = 91·8.

Curve IV.—Area = 2,366; ratio, flux density at tip to flux density at middle of tooth surface = 1·07; minimum height for the surface of the tooth = 90·2.

Experiment 2.—

Air-gap = 5·3 mm.

Ratio $\frac{\text{slot}}{\text{gap}} = 2{\cdot}83$.

Ratio $\frac{\text{tooth width}}{\text{gap}}$ for the various teeth = 1·89, 2·83, 2·89, 3·68, 3·7, 5·66.

Average flux density on the tooth surface = 6,600 lines per cm.².

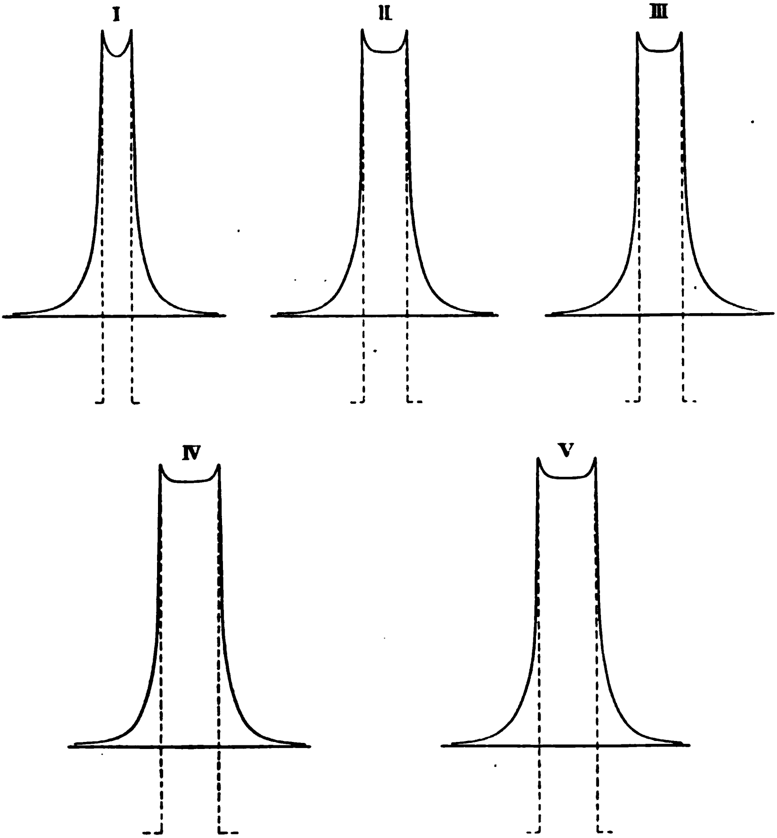


FIG. 7.—Flux Distribution on the Surfaces and Flanks of the Teeth.

Air-gap = 2·3 mm.

In Fig. 8 the flux distribution curves are given for the surface of the pole-piece under these conditions. It will be noticed that the flat tops of the curves have almost disappeared and the distribution takes a definite wave shape. Further, the fluctuations of the flux at any point

on the pole-piece surface are much less than was the case with a smaller air-gap.

The following gives the details of the several curves :—

Curve I.—Area = 1,820; mean height = 71·3; maximum height = 93·8; minimum height = 54·5.
Curve II.—Area = 2,218; mean height = 73·9; maximum height = 94·35; minimum height = 54·5.
Curve III.—Area = 2,278; mean height = 76·0; maximum height = 94·6; minimum height = 55·0.
Curve IV.—Area = 2,630; mean height = 76·3; maximum height = 94·5; minimum height = 55·0.

Fig. 9 shows the flux distribution on the surfaces and flanks of the various teeth for the same gap and flux density as for the curve in

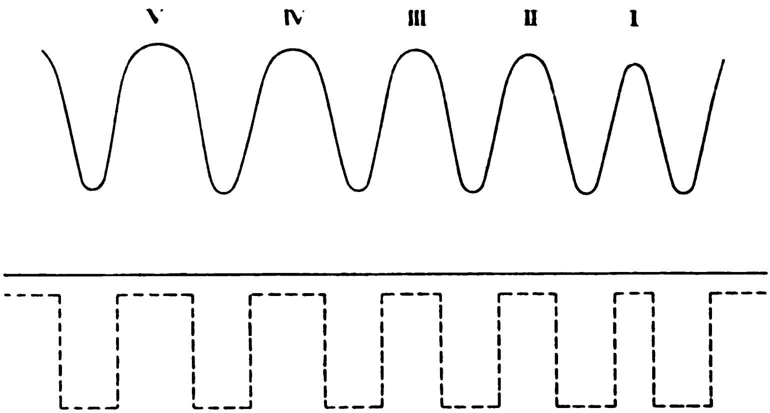


FIG. 8.—Flux Distribution Curve on the Smooth Surface of the Pole-piece.

Air-gap = 5·3 mm. Average Flux Density at Surface of a Tooth = 6,600 lines per cm.².

Fig. 8. In this case the peaks of flux concentration at the teeth tips are more marked than they were for the smaller air-gap.

Curve I.—Area = 1,783; ratio, flux density at tip to flux density at middle of tooth surface = 1·11; minimum height for the surface of the tooth = 98·8.
Curve II.—Area = 2,295; ratio, flux density at tip to flux density at middle of tooth surface = 1·15; minimum height for the surface of the tooth = 97·5.
Cuive III.—Area = 2,183; ratio, flux density at tip to flux density at middle of tooth surface = 1·16; minimum height for the surface of the tooth = 97·1.

Curve IV.—Area = 2,636; ratio, flux density at tip to flux density at middle of tooth surface = 1·18; minimum height for the surface of the tooth = 95·5.

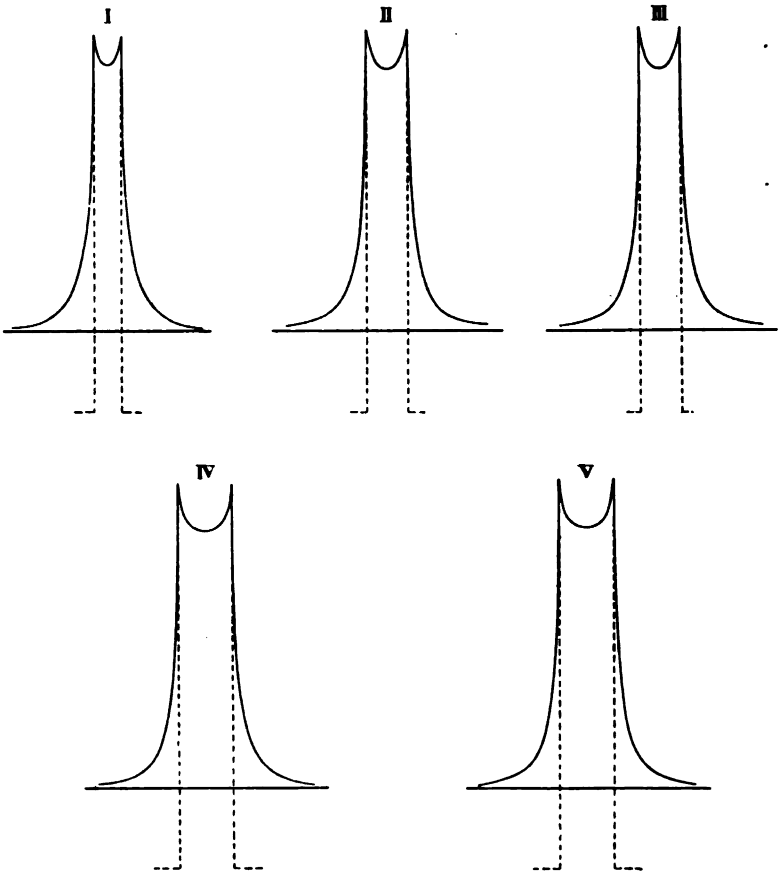


FIG. 9.—Flux Distribution on the Surfaces and Flanks of the Teeth.

Air-gap = 5·3 mm. Average Flux Density on Surface of a Tooth ≏ 6,600 lines per cm.².

Experiment 3.—

Air-gap = 7·3 mm.

Ratio $\frac{\text{slot}}{\text{gap}} = 2{\cdot}05$.

Ratio $\frac{\text{tooth width}}{\text{gap}}$ for the various teeth = 1·37, 2·05, 2·09, 2·67, 2·68, 4·1.

Average flux density on the tooth surface ≏ 5,000 lines per cm.².

Fig. 10 shows the curve of flux distribution on the pole-piece surface, and it is seen that the effect of increasing the air-gap has been again to diminish the fluctuation of the field.

The measurements of the several curves are as follows:—

Curve I.—Area = 1,525; mean height = 62·3; maximum height = 73·0; minimum height = 53·0.
Curve II.—Area = 1,846; mean height = 61·5; maximum height = 72·7; minimum height = 53·0.
Curve III.—Area = 1,973; mean height = 64·6; maximum height = 74·2; minimum height = 53·5.
Curve IV.—Area = 2,309; mean height, 67·0; maximum height = 74·5; minimum height = 54·0.

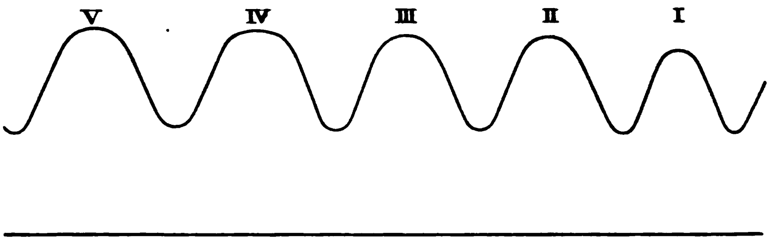


FIG. 10.—Curve showing Flux Distribution on Smooth Surface of Pole-piece.
Air-gap = 7·3 mm. Average Flux Density at Surface of a Tooth ≏ 5,000 lines per cm.².

The flux distribution on the surfaces and flanks of the teeth for the above air-gap and density is shown in Fig. 11. The effect of the larger gap is to increase further the relative flux density at the tips of the teeth.

Curve I.—Area = 1,439; ratio, flux density at tip to flux density at middle of tooth surface = 1·21; minimum height for the surface of the tooth = 76·0.
Curve II.—Area = 1,810; ratio, flux density at tip to flux density at middle of tooth surface = 1·29; minimum height over the surface of the tooth = 72·5.
Curve III.—Area = 1,879; ratio, flux density at tip to flux density at middle of tooth surface = 1·28; minimum height over the surface of the tooth = 75·0.
Curve IV.—Area = 2,232; ratio, flux density at tip to flux density at middle of tooth surface = 1·34; minimum height over the surface of the tooth = 73·3.

These curves also show that the leakage of flux from the flanks of the teeth to the surface of the pole-piece is increased with increased gap, as would of course be expected.

Experiment 4.—A second experiment was made with a gap of 5·3 mm., but with a much higher flux density than previously, namely,

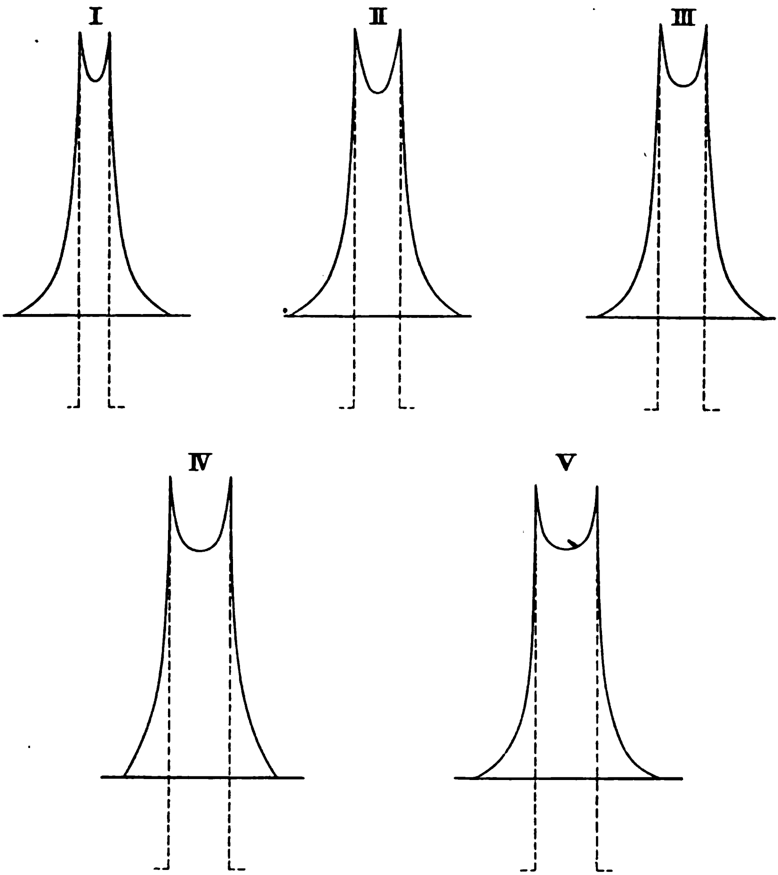


FIG. 11.—Flux Distribution on Surfaces and Flanks of the Teeth.
Air-gap = 7·3 mm. Average Flux Density at the Surface of a Tooth ≏ 5,000 lines per cm.².

13,500 lines per cm.² The curves so obtained are plotted in Fig. 12, and the numerical results are as follows:—

Curve I.—Area = 1,813; mean height = 73·5; maximum height = 92·7; minimum height = 57·0.

Curve II.—Area = 2,416; mean height = 80·5; maximum height = 99·7; minimum height = 60·0.

Curve III.—Area = 2,445; mean height = 81·5; maximum height = 99·8; minimum height = 60·0.

Curve IV.—Area = 2,905; mean height = 84·0; maximum height = 102·0; minimum height = 61·0.

The curves of Fig. 13 show the corresponding flux distribution on the surfaces and flanks of the teeth. The increase of flux density has apparently had the effect of increasing the relative magnitude of the flux peak at the tooth tips.

Curve I.—Area = 1,760; ratio, flux density at tip to flux density at middle of tooth surface = 1·14; minimum height for the surface of the tooth = 96·3.

Curve II.—Area = 2,408; ratio, flux density at tip to flux density at middle of tooth surface = 1·18; minimum height over the surface of the tooth = 102·0.

Curve III.—Area = 2,372; ratio, flux density at tip to flux density at middle of tooth surface = 1·18; minimum height over the surface of the tooth = 102·0.

Curve IV.—Area = 2,754; ratio, flux density at tip to flux density at middle of tooth surface = 1·18; minimum height over the surface of the tooth = 103·5.

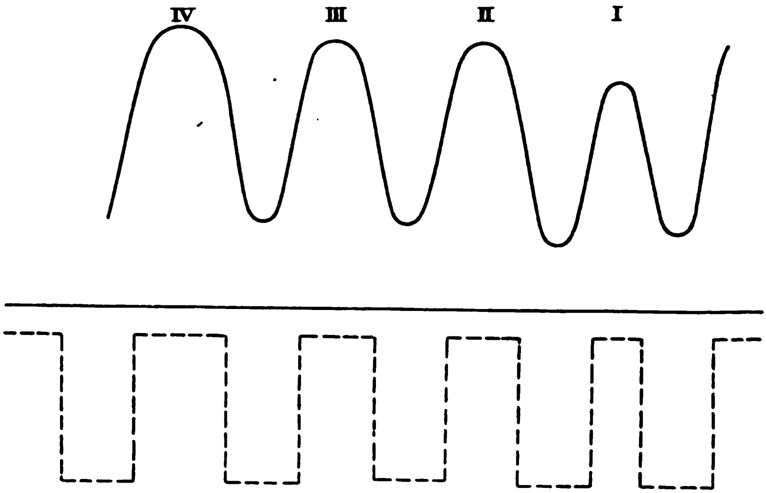


FIG. 12.—Flux Distribution Curve on Smooth Surface of the Pole-piece.
Air-gap = 5·3 mm. Average Flux Density at the Surface of a Tooth ≏ 13,500 lines per cm.².

APPLICATION OF THE EXPERIMENTAL RESULTS.

The results of the foregoing experiments may be used to find the effect of slotting the armature on the reluctance of the air-gap. It is necessary to know this in order to calculate the ampere-turns, and there are two methods of considering the matter.

First, when the armature is slotted the number of ampere-turns necessary to send the flux through the gap is determined by the

maximum flux density under the teeth; the average flux density, however, is smaller than this maximum value, and the ratio of the maximum density to the mean density is the ratio of the number of ampere-turns necessary to send a given flux through the air-gap with a

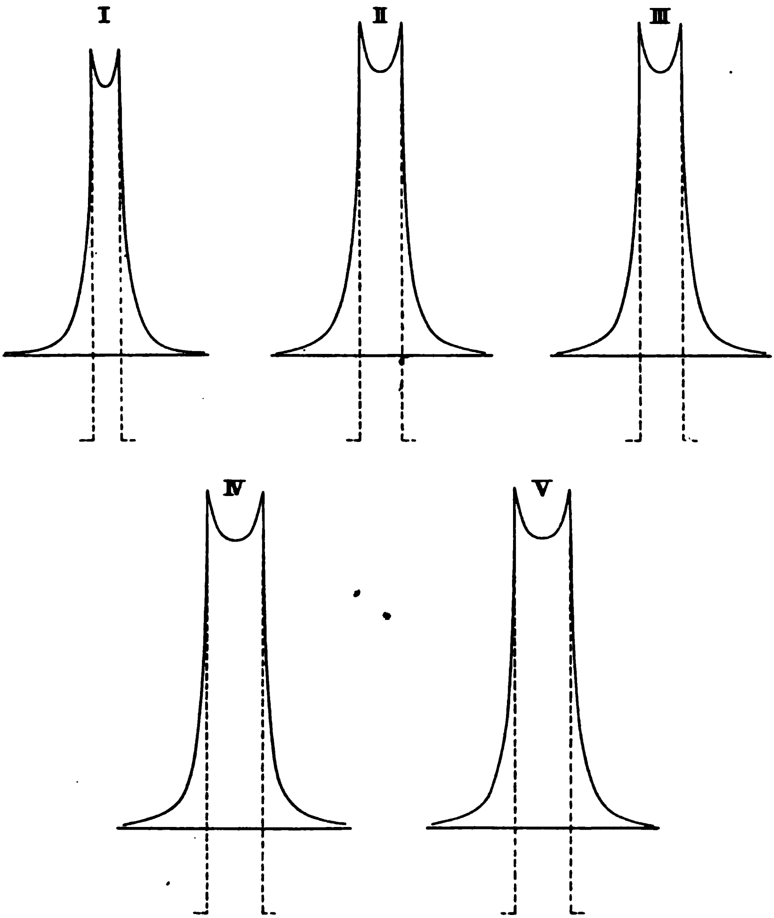


FIG. 13.—Flux Distribution on the Surfaces and Flanks of the Teeth.
Air-gap = 5·3 mm. Average Flux Density at the Surface of a Tooth ≙ 13,500 lines per cm.[2].

slotted armature, to the number of ampere-turns necessary to send the same flux through the same air-gap with a smooth armature. This ratio is a factor which Arnold calls k, and is a kind of contraction coefficient which enables the necessary ampere-turns to be calculated at once. For a smooth armature k_1 is, of course, equal to 1, and for all other cases is greater than 1.

Arnold* has given a curve showing how to find the value of this co-efficient for all practical values of slot, gap, and tooth width. The factor k_1 is given as equal to $\frac{t_1}{z_1 + X\delta}$ where t_1 is the slot-pitch; z_1 is the tooth width at the circumference of the armature; X is a factor dependent on the ratio $\frac{\text{slot}}{\text{gap}}$, and δ is the length of the air-gap. The factor X was determined by drawing the lines of force between the armature teeth and pole surfaces for various proportions of slot, gap, and tooth width. The lines of force are drawn by trial so that the total flux as measured from the drawing has a maximum value. By this means a curve was obtained showing the relationship of X to $\frac{\text{slot}}{\text{gap}}$. This curve is reproduced in Fig. 14.

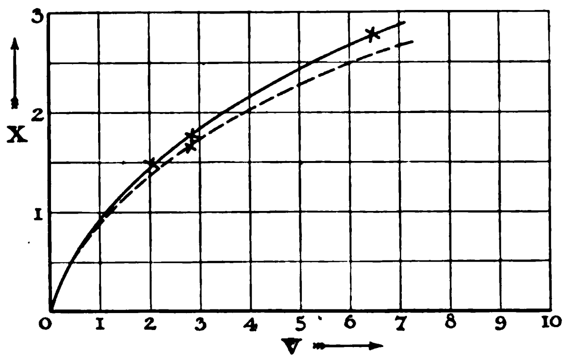


FIG. 14.

The Full-line Curve shows the Values obtained Experimentally.
The Dotted Curve shows the Values as given by Arnold.

From the experimental results given in the first part of this paper it is easy to deduce the values of X for different values of the ratio $\frac{\text{slot}}{\text{gap}}$, for k_1 can be obtained from the figures already given. Thus $k_1 = \frac{\text{maximum flux density in air-gap}}{\text{mean flux density in air-gap}}$. It must be remembered that the maximum flux density in the air-gap is to be taken as the mean of the flux density at the middle of the surface of a tooth and the maximum flux density at the surface of the armature just opposite. For, as was pointed out above, the flux density at the middle of the tooth face is, in general, higher than at a point on the pole surface just opposite, especially if the gap is large compared with the slot width. All the factors, except X, in the expression $k_1 = \frac{t_1}{z_1 + X.\delta}$ are thus known.

* *Die Gleichstrommaschine*, 2nd edition, vol. i., p. 271.

In the following table the values of the ratio of slot to gap and the values of k_1 obtained from the curves are given. In the third column the corresponding values deduced for X are to be found.

TABLE.*

Ratio $\frac{\text{Slot}}{\text{Gap}}$.	k_1.	X.	
1·99	1·15	1·49	From Figs. 10 and 11.
2·05	1·18	1·43	
2·05	1·15	1·51	
2·05	1·11	1·59	
2·83	1·26	1·88	From Figs. 12 and 13.
2·93	1·26	1·88	
2·77	1·23	1·69	
2·83	1·22	1·60	
2·92	1·31	1·79	From Figs. 8 and 9.
2·83	1·28	1·61	
2·83	1·25	1·60	
2·83	1·24	1·59	
6·73	1·55	2·81	From Figs. 6 and 7.
6·10	1·37	2·70	
6·30	1·40	2·90	
6·70	1·35	2·78	

Thus each of the Figs. 6, 7, 8, 9, 10, 11, 12, 13 gives a set of values for $\frac{\text{slot}}{\text{gap}}$ and X, and the means of the values for each set have been plotted as the full line curve in Fig. 14, the ratio $\frac{\text{slot}}{\text{gap}}$ being denoted by V. It is seen from this curve and the corresponding curve for the values as deduced from pictures of the lines of force that the experiments give slightly higher values for X than the graphical method; that is, the experimental values of k_1 are less, and therefore the reluctance of the air-gap is less.

Another method of taking account of the presence of slots in the armature and their effect on the reluctance of the air-gap is as follows: The total flux issuing from a tooth being known, the effective width of the tooth is taken as that width which would give the same flux if the density were constant and equal to the maximum density actually existent under a tooth, and the width of the pole is taken as equal to

* In this table the "equivalent" slots have been used to find the ratio $\frac{\text{slot}}{\text{gap}}$, *i.e.*, the slot width has been taken as equal to the distance between the minimum ordinates in the curves of flux distribution on the smooth face of the pole-piece.

the number of teeth under the pole multiplied by the effective width of a tooth as given above.

The amount of fringing from the flanks of the teeth as dependent on the proportions of slot, tooth, and gap, was given by Carter as a coefficient.

From the formula given by him a curve has been deduced showing how the amount, which must be added to the actual width of the tooth in order to get the ideal width, may be found for any practical value of the ratio of $\frac{\text{slot}}{\text{gap}}$.

From the experimental results this effective width is found as follows: Amount by which the actual width of the tooth is to be increased $= \frac{\text{area of flux curve}}{\text{maximum ordinate}}$ minus the tooth width, and $\Delta = \frac{\text{amount added}}{\text{slot}}$.

In this way the values of Δ have been deduced for the various ratios of $\frac{\text{slot}}{\text{gap}}$.

Thus—

$\frac{\text{Slot}}{\text{Gap}}$	Δ (Experiment).	Δ (Carter).
2·85	0·574	0·640
2·84	0·620	0·640
2·03	0·730	0·712
6·45	0·458	0·435
0·0	—	1·000

Application to Induction Motors.

When both the stator and rotor are slotted, as in an induction motor, the flux distribution curve for the air-gap becomes very complex, and, moreover, changes its shape as we proceed from the stator surface across the gap to the rotor surface. Further, the curve of flux distribution on the stator periphery has a period of pulsation corresponding to the movement of the rotor over a distance equal to the pitch of the rotor teeth, and the curve of flux distribution on the rotor periphery has a period of pulsation corresponding to the movement of the rotor over a distance equal to the pitch of the stator teeth.

In order to draw the flux distribution curve on the periphery of either the stator or the rotor the following may be considered:—

Suppose it is desired to find the flux distribution over the periphery of the stator. First imagine that the stator has a smooth surface; we can then draw the flux distribution curve on the surface due to the

slotted rotor (Curve I., Fig. 15). The method of drawing curves of flux distribution has been already indicated, and an examination of the various curves given will be of assistance as a general guide as to the shape of the curves for any given proportions of slots, teeth, and gap. After a little practice these curves of flux distribution can be drawn with considerable rapidity. Next suppose that the rotor has a smooth

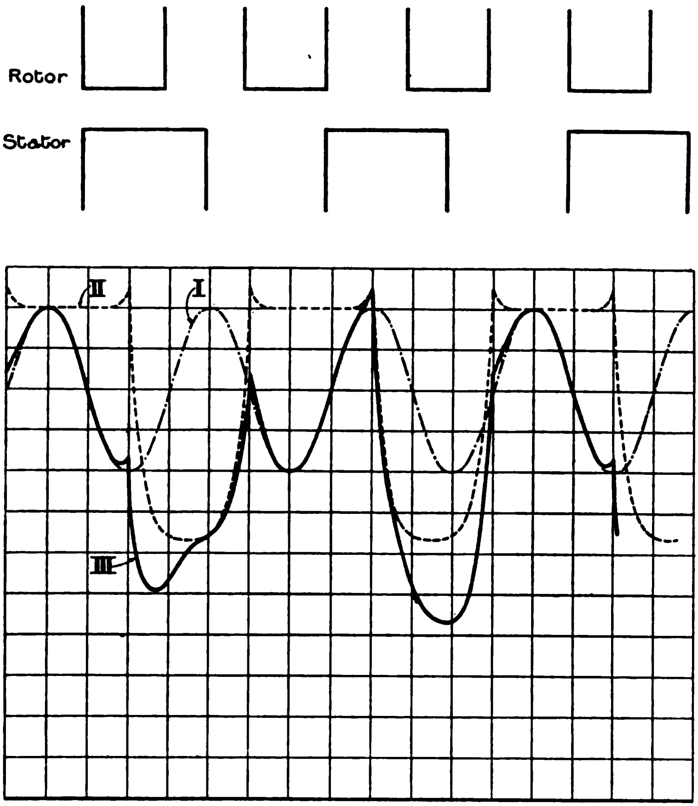


FIG. 15.—Showing Flux Distribution at the Periphery of the Stator.

surface and the stator is slotted; we can then draw the flux distribution curve on the surface of the stator teeth from the results of the experiments previously described. This curve is shown in Fig. 15 as Curve II. Now take the actual case in which both the stator and rotor are slotted, then the actual curve of flux distribution on the stator surface will be found from a combination of the two curves. Thus in Fig. 15 the ordinates of Curve I. are to be reduced in the ratio of the corresponding ordinate of Curve II. to the value the flux density

would have if both stator and rotor were smooth. In this manner the resultant Curve III. has been deduced from Curves I and II., *i.e.*, the ordinates of Curve III. are equal to the corresponding ordinates of Curve I. multiplied by the ratio of the corresponding ordinates of Curve II. to the height of the horizontal line, which gives the constant density which would exist in the air-gap if both stator and rotor had smooth cores. This horizontal line touches the peaks of Curve I.

Curve III. only holds for the relative position of stator and rotor teeth shown, and the curve will vary in shape as the rotor surface moves through a distance equal to the pitch of the stator teeth.

The corresponding curve for the flux distribution over the periphery of the rotor may be found in a similar way. It has been assumed in the above that the magnetic force is constant at all points of the air-gap. For any given curve of magnetic force a simple modification must be introduced.

In order to find the reluctance of the air-gap in induction motors, we may suppose that if the reluctance is unity when the rotor and stator surfaces are smooth, k_1 is the coefficient when the rotor alone has slots, that is, the reluctance of the gap with slotted rotor and smooth stator is k_1. Suppose further the gap coefficient to be k_2 when the stator is slotted and the rotor has a smooth surface. Then the reluctance of the gap when the stator and rotor are both slotted will be given without any great error by the product $k_1 \times k_2$. Now k_1 and k_2 can be found by the method previously described, from the dimensions of the teeth, slots, and air-gap, and therefore the reluctance of the gap can be at once determined.

In conclusion, I would like to express my appreciation of the facilities placed at my disposal for this work by Professor E. Arnold; of the valuable advice given and the interest shown in the experiments by Professor Bragstad, and further to acknowledge Dr. Kloss's kindness in critically reading the paper through.

DISCUSSION.

Dr. Marchant.

Dr. E. W. MARCHANT: There are certain matters in connection with the paper which I think are of great interest and importance, and I should like to refer to one or two of them.

In the first place, I must make a small criticism. On page 551 Mr. Wall states that the measurements were made by breaking the current through his field coil. That method seems to me to be open to objection, because the actual change in flux which is measured when the circuit is broken does not represent the total flux which is passing through the search coil. It represents the flux passing through the search coil less the permanent flux which passes when the exciting current is switched off. I think that explains one of the effects which Mr. Wall has observed. In describing the experiments, he said that when he made a measurement at one pole-tip, and went to another pole-tip, and came back to the first, he got a different result in the second position from that

which he got in the first position. I think that was due to the change in the residual magnetisation of the iron. When he switched his current on, the flux through the coil at its pole-tip was a certain amount, when he switched off it fell to a certain other amount. Now since the amount of residual magnetism retained after the magnetising force has been kept on for some time is greater than at first, the amount of flux change through his search coil should be less than it was before. Dr. Marchant.

It seems to be very doubtful whether a temperature change in the iron, and the very small effect which it produces on the permeability of the iron, is sufficient to produce any noticeable result. The tips in the curve on Fig. 3 are very interesting; I have certainly not seen anything of that kind before, and one would hardly have expected the fringing effect to be so strong as it actually is.

On page 553 Mr. Wall says "the point at which the flux density falls off rapidly appears to be at a distance equal to the air-gap, from the point just under the pole-tip." Later on he makes the same statement for a. different width of air-gap, in which the effect appears to fall off at twice the distance. Is there any simple expression for the distance from the edge of the pole at which the flux becomes uniform in terms of the air-gap?

In the curves in Fig. 7 the effect of the slot width in reducing the maximum magnetic flux is very noticeable. With a narrow slot or tooth the magnetomotive force which is required to send the flux across the air-gap for a given mean flux density appears to be reduced.

I have had a good deal of difficulty in following Mr. Wall in the paragraph in which he states that the ratio between the magnetomotive force, and the ampere-turns required to send the flux across the air-gap when the armature was smooth and when it was slotted was in the ratio of the main flux density to the maximum flux density under the teeth. I think that might be made a little clearer. In speaking of the maximum flux through an air-gap, one generally speaks of the maximum flux in the direction of the length of the air-gap; in this case one is dealing with variations in the flux from side to side of the air-gap and not along it.

I should like to know very much how his figures for k_1 compare with those obtained in the paper by Hele-Shaw, Hay, and Powell, to which reference has been made. A very useful series of tables was given there obtained by a variety of methods, and it would be interesting to see how it agrees with the figures given here.

Mr. W. Hoult: I have been very much interested in the flux distribution curves showing the pointed tips, and it is new to me to learn that the flux is much denser at the tips than at the middle of the pole surface. Mr. Hoult.

Mr. C. F. Smith: There are only two points to which I wish to refer, and in both cases they are points on which I should like some further information from the author. At first sight the curves of flux distribution on the pole-face given in Figs. 3 and 5 appear remarkable on account of the high peak at the tips, and, on a casual inspection, they Mr. Smith.

Mr. Smith. hardly appear to follow the distribution corresponding to the lines of electrostatic force due to a charged body or the stream-line distribution obtained by various methods. The falling off in intensity along the flank of the pole looks much more rapid than would be expected; the density on the flank seems to fall almost at once to practically half its value on the face. Possibly this change is only apparently more sudden than in the distribution one would sketch, and I should like to ask the author whether he has compared the actual flux distribution with that plotted according to Arnold's method. Can the small discrepancy between the calculated and experimental curves in Fig. 14 be attributed to a difference between the actual and assumed distribution of the flux?

The other point is as to the exact meaning of the change in the sign of the flux which the author referred to in connection with the curves in Fig. 11. Probably the explanation is a simple one, but I do not quite see what the explanation of that change of sign is.

Mr. Walker. Mr. MILES WALKER: Although the method of calculating the reluctance of the air-gap of a slotted armature is well known to

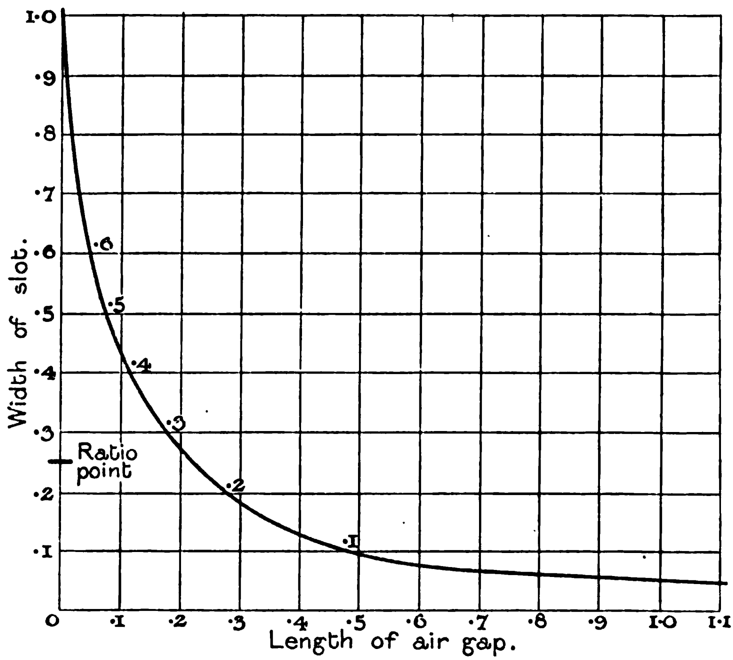


FIG. K.

engineers, it is interesting to have our theories confirmed by curves such as those given in Figs. 6 and 7 obtained experimentally. The horns at *b* and *c* in Fig. 3 are to be expected. If a permanent magnet

is dipped into iron filings, we always find more filings adhering to the corners than to the faces. The reason may be shortly stated by saying that the magnetic lines will crowd each other at the corners because by getting out there they find more room to spread than if they came out on the pole-face into a path which never grows any wider. The points of the horns in the curve should be, however, more rounded. I think that the definition of k_1 given in the paper is not quite accurate. The maximum flux density in the gap with a slotted armature is not necessarily the same as the flux density there would be with a smooth armature. This is seen from the hump 1 in Fig. 12, where the width of the tooth is not very great compared with the length of the gap. When a great deal of mathematical calculation and experimental work is necessary to aid the designer, it is desirable that the results of the work be put into such a form that they can be quickly and accurately applied in commercial calculations. For this reason every designer has his own quick method of calculating the effect of the armature slots and armature ducts. For the benefit of students who may be interested in this matter, I give in Fig. K a curve which has stood the test of many years in practical design, and which gives the value of k_1 with the minimum amount of calculation. The only quantities involved are— Mr. Walker.

g = The length of the gap in any units.
s = The width of the slot in the same units.
p = The pitch of the slots.
s_1 = The "reduced width" of the slot which is the slot width multiplied by a factor obtained from the curve.

Then—

$$k_1 = \frac{p}{p - s_1}.$$

The method of finding s_1 is as follows: Place one edge of a set-square so that it lies on the point on the vertical slot line which represents the width of the slot, and so that it also lies on the point on the horizontal gap line which represents the length of the air-gap. Slide the edge parallel to itself until it cuts the "ratio-point." Then from the point on the air-gap line now cut by the edge, run up vertically, and read off a factor from the curve, which when multiplied by s gives s_1.

Example—

$$\text{Let } g = 0{\cdot}3 \text{ in.}$$
$$s = 0{\cdot}5 \text{ ,,}$$
$$p = 1{\cdot}0 \text{ ,,}$$

Place the edge of the set-square on 0·5 of the slot line, and 0·3 of the gap line. Now slide it parallel to itself until it cuts the "ratio-point." It now cuts the air-gap line at 0·15, and the reading 0·33 on the curve gives the factor with which to obtain the reduced slot $s_1 = 0{\cdot}5 \times 0{\cdot}33 = 0{\cdot}165$. Therefore $p - s_1 = 0{\cdot}835$.

Then—

$$k_1 = \frac{1}{0{\cdot}835}.$$

Mr. Walker. The values obtained in this way correspond very closely with those obtained from the formulæ cited in Mr. Wall's paper.

BIRMINGHAM LOCAL SECTION.

DISCUSSION, *February* 12, 1908.

Mr. Matthews. Mr. H. B. MATTHEWS: The author is to be congratulated upon having successfully carried through difficult investigations yielding useful results. The air-gap distribution curves are especially interesting to me, as a few years ago I had opportunity of investigating this in the case of a particular machine.

The results I obtained, which have not been published, are shown in Fig. L. Curve A shows the flux distribution over two adjacent teeth, and the intermediate slot at and normal to the periphery of the fixed armature under normal excitation. Curve I shows the flux distribution at and normal to an intermediate surface half-way between the armature and the pole-face. Curve P shows the flux distribution at and normal to the face of the cast-steel pole. In each case the variations in density are seen to be sinusoidal, thus differing from Mr. Wall's results as given in Figs. 6, 8, 10, and 12. This is probably due to the slots in my case being semi-enclosed, reducing the $\frac{\text{slot-opening}}{\text{air-gap}}$ ratio to 1·4, the slots in Mr. Wall's case being open.

The total flux issuing from one tooth and one slot measured at the periphery of the armature is represented by the area under the distribution curve A between the ordinates B E and C D. This area is equal to the area of the rectangle B D, so that the ordinate B E represents the mean density over slot and tooth areas at the armature periphery.

The area under the curve A between the ordinates F E and G H represents the total flux issuing from the tooth-face. This area is equal to the area of the rectangle F H, so that the ordinate F E represents the mean density over tooth-face at the armature periphery.

The area under the curve A between the ordinates G H and C D represents the total flux issuing from the slot. This area is equal to the area of the rectangle Q D, so that the ordinate Q H represents the mean density over the slot opening at the armature periphery. The ratio

$$\frac{\text{Mean flux density over slot opening}}{\text{Mean flux density over tooth-face}} = \frac{\text{Q H}}{\text{F E}} = 0{\cdot}76.$$

In order to find the contraction co-efficient k_1 (Arnold) referred to by Mr. Wall (assuming this to be the ratio of the ampere-turns required with a slotted armature to those required to produce the same flux with a smooth armature and similar gap), it is only necessary to construct rectangle F K equal in area to the rectangle B D, representing the total flux issuing from one tooth and one slot at a uniform density represented by the ordinate E F. Then the contraction co-efficient—

$$k_1 = \frac{2\,\text{E D}}{\text{L D} + \text{E K}} = \frac{2 \times 12{\cdot}6}{12{\cdot}6 + 11{\cdot}5} = 1{\cdot}05.$$

The result obtained from Arnold's formula, $k_1 = \frac{t_1}{z_1 + X\delta}$ is 1·1, using Fig. 14 of Mr. Wall's paper to obtain X. Mr. Matthews.

A different contraction co-efficient is arrived at by assuming that the mean density over the tooth-face also exists over a certain fraction

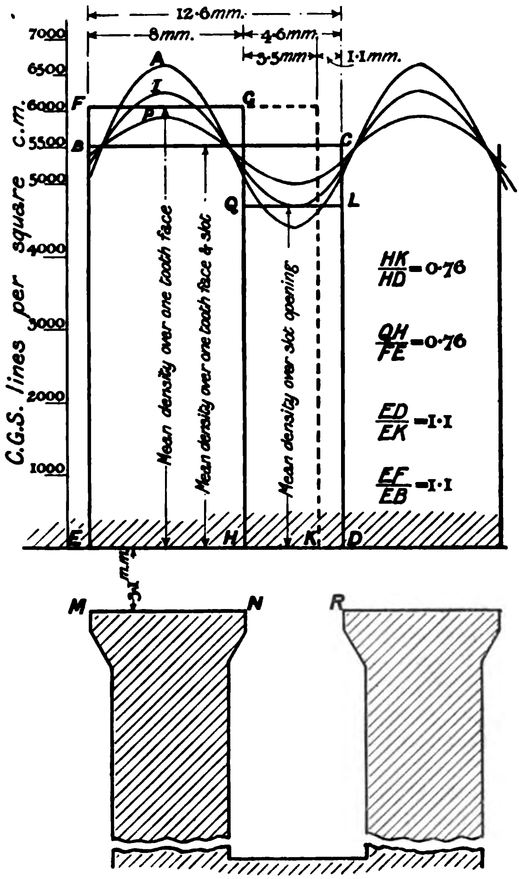


FIG. L.

of the slot. This fraction in this case would be $\frac{HK}{HD} = \frac{3 \cdot 5}{4 \cdot 6} = 0 \cdot 76$. This corresponds to Wall's and Carter's Δ given in the second table of the paper, and agrees fairly well with those results, the $\frac{\text{slot}}{\text{gap}}$ ratio in my case being 1·4.

The ratio of the maximum to the minimum density at the armature

Mr. Matthews.

periphery is given by the ratio of the highest to the lowest value of curve A, and equals about 1·5.

The ratio of the maximum density occurring at the face of the tooth to that at a corresponding point on the pole is given by the ratio of the maximum height of curve A to that of curve P, and equals about 1·12.

The ratio of the total flux passing through tooth-face M N to that passing through slot-opening N R is about 2. Hence the flux issuing from the two flanks of one of the teeth is about half that issuing from one tooth-face.

The flux distribution curves A and P have been deduced directly from observations of ballistic galvanometer deflections caused by shooting a search coil in connection therewith through the air-gap in a direction parallel to the axis of the machine,

(*a*) When the search coil was "shot" as near as possible to the armature (curve A), and
(*b*) When the search coil was "shot" as near as possible to the pole-face (curve P).

The actual observation curves so obtained proved to be sinusoidal, but had to be modified in order to allow for

(1) The width of the coil.
(2) The fact that the investigations in each case were not actually made at the armature and pole faces respectively.

The flux distribution curve I was obtained by taking the mean of curves A and P.

Mr. Coales.

Mr. J. D. COALES: The flux densities in the teeth used by the author are rather low, which fact is perhaps due to the research having been undertaken mainly with reference to induction motors. A comparison of Figs. 9 and 13, for which the air-gap lengths were equal, shows that the general distribution of flux is the same in each case, although the flux density of Fig. 13 is twice that of Fig. 9. This seems to show that the variation of the permeability of the teeth with flux density has not yet begun to make itself felt.

Dr. Sumpner.

Dr. W. E. SUMPNER: I should like to know the exact meaning of the abscissæ in Figs. 9 and 13. I presume that the ordinates represent the density of the lines of force on the surface of the iron. Mr. Wall's curves do not apply to the induction density in the actual air-gap, where the irregularities due to the slots, although appreciable, would be much less than those indicated in the curves. Moreover, these curves do not indicate the wave form of the electromotive force induced in the conductors, especially if these conductors are embedded in slots. The induced electromotive force is the product of the flux density at the conductor into the speed at which the lines cross the conductor. It must be remembered that for conductors in slots, while the flux density

may be low, the relative speed of the flux and conductor is much greater than the relative speed of conductor and pole. Hence the static distribution of the flux affords little indication of the wave-form of the electromotive force. Dr. Sumpner.

Mr. THOMAS F. WALL (*in reply*): With reference to Professor Marchant's remarks, it appears to me probable that the number of times the exciting current circuit was made and broken has had some influence in producing the effect mentioned on page 551. I found that the galvanometer deflections became regular after making and breaking the main circuit about fifteen or twenty times, and this would seem to point to the fact that the changes in the flux at make and break had then become cyclic, as Professor Marchant suggests. I do not think any general rule can be given as to the distance under the tooth at which the flux-distribution curves of the pole-piece surface begin rapidly to fall off, for this will depend on the proportions of the slot and gap, and probably also on the tooth width. The maximum flux densities referred to on page 563 are represented by the maximum ordinates of curves such as those in Fig. 12, and the mean flux densities are represented by the mean ordinates of the same curves. With regard to the results obtained by the stream-line method, these agree well with the values given by Arnold. Mr. Wall.

Mr. Smith has suggested a comparison between the flux distribution as found by sketching the lines of force with that given by the curves, but I do not think it would be easy to find the distribution in the neighbourhood of the tips of the teeth by the former method, and I believe the experimental curves will be found useful as an indication of the manner in which the lines of force should be drawn at these critical points.

Mr. Walker has referred to the difference between the maximum ordinate of curve I. and the maximum ordinates of curves II. to V. in Figs. 6, 8, 10, and 12, for it is clear that curve I. has in every case a smaller maximum ordinate than the other curves II. to V. I think this is mainly due to the small value of the ratio of width of tooth to length of slot in the case of the narrowest tooth. I would like to emphasise the fact that by the use of a curve such as that given in Fig. 14, the coefficient k_1 can be very easily found; all that is necessary is to find the ratio of width of slot to length of gap, to read off from the curve the corresponding value of X, and then to substitute in the formula for k_1. It is interesting to note that the values obtained by using Mr. Walker's curve agree closely with those found by means of the curve in Fig. 14.

In reply to the discussion at Birmingham. The curves given by Mr. Matthews are very interesting, and the information which can be deduced from them affords a useful comparison of results, although, of course, since mean values are dealt with in this case, the peaks shown in Figs. 7, 9, 11, and 13 could not be determined.

In reply to Mr. Coales, I think that if the flux density were increased to such an extent that the permeability would be appreciably

Mr. Wall. affected, the peaks shown in Figs. 7, 9, etc., would be correspondingly smoothed out.

As Dr. Sumpner remarks, the curves of Figs. 9, 13, etc., show the flux density at the surface of the iron, and not in the actual air-gap. Although the concentration of the flux at the tooth tips may not affect the wave form of the electromotive force induced in conductors embedded in the slots, it is probable that the reactance of the conductors will be influenced by this concentration; that is to say, the reactance as calculated on the assumption that the lines of force in a slot, due to a current flowing in the conductor, pass as parallel straight lines from side to side across the slot will not be quite correct. At the top of the slot, the lines concentrate at the tips, and the reactance of the conductor will be probably diminished thereby. The effect may, perhaps, not be very great, but it should be taken into account when attempting to calculate, for instance, the self-induction of short-circuited coils in direct-current machines. Moreover, in estimating the leakage flux in induction motors the concentration of the flux at the tooth tips should not be overlooked.

It is of importance that the factor k_1 should be calculated with as much accuracy as possible, for, in large machines especially, a small percentage error in the determination of k_1 may mean a considerable amount in the cost of material.

NEWCASTLE LOCAL SECTION.

DETERMINATION OF THE LOSSES IN POLE SHOES, DUE TO ARMATURE TEETH.

By THOMAS F. WALL, M.Sc., B.Eng. (Student), and STANLEY P. SMITH, B.Sc.

(*Received from the* NEWCASTLE LOCAL SECTION, *October* 7, 1907, *and read at Newcastle, February* 3, *and at Birmingham, February* 12, 1908.)

Amongst the sources of loss in dynamo-electric machinery, there is one which may, under certain conditions, become abnormally large. This is the loss due to the pulsations of the flux set up in the pole shoe by the movement of the armature teeth across the pole face. Owing to the scarcity of experimental data, the extent of this loss is difficult to estimate; and it is of importance to know under what circumstances it is desirable to use special devices—such as laminated pole shoes—for diminishing this loss, and so improving the efficiency.

The way in which these losses arise can be seen from Fig. 1, which shows the distribution of the flux in the air-gap when the machine is running light. From this figure it is at once obvious that the flux density at any point on the pole face will vary according to the position of the armature teeth relative to that point; consequently, as the teeth move across the pole face, the flux in the shoe will pulsate. The magnitude of these pulsations depends chiefly on the value of the mean flux density over the whole pole surface, and on the relation between the slot-opening and the air-gap. The E.M.F.'s set up by the flux pulsations produce corresponding eddy currents, and these, in turn, tend to damp out the fluctuations. The damping action is mainly dependent on the frequency of the pulsations, the thickness of the laminations (if any), and the nature and temperature of the iron. Further, the presence of a current in the armature has a large influence on these losses, as may be gathered from Fig. 2, which illustrates the distorting effect of the armature current on the field and its corresponding effect on the magnitude of the flux pulsations.

It is such complications as these which render the calculation of the eddy current losses in pole shoes difficult. Under the assumption that the flux pulsates according to a sine law, the calculation of these losses has been carried out by Potier and Rüdenberg. The formula,* as obtained by them, is as follows :—

* *Industrie Électrique*, 1905, p. 35; *Elektrotechnische Zeitschrift*, vol. 26, 1905, p. 181.

$$W_p = C\left\{(k_1 - 1)\, t_1\, B_l\right\}^2 \left\{\frac{100\, v}{t_1}\right\}^{1.5} 10^{-11} \text{ watts per sq. dcm. of pole surface :}$$

where—

C = constant.

t_1 = tooth-pitch in cms. at armature circumference.

k_1 = ratio of the magnetic permeance of the air-gap of a smooth armature to that of a slotted armature.*

B_l = mean flux density in air-gap.

v = peripheral speed of armature in metres per sec.

The flux pulsations are probably not sinusoidal, however,† and have, consequently, a larger form factor than a sine curve, whence it

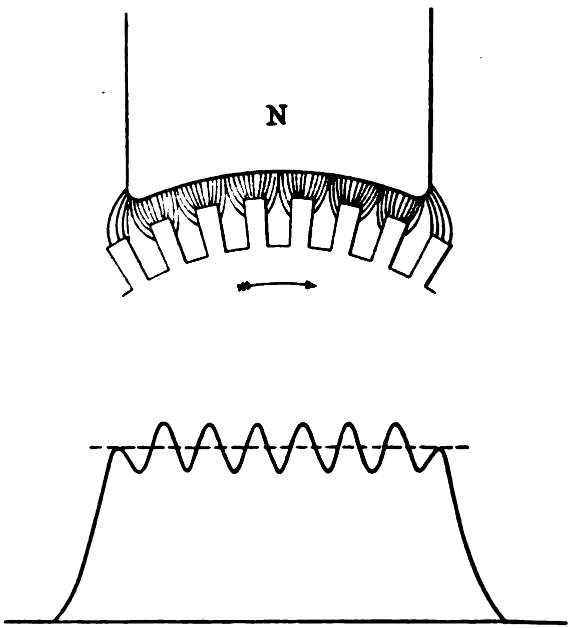


FIG. 1.—Flux Distribution on No-load.

is to be expected that the above theoretical formula would give results less than those obtained by experiment. It will be presently seen that this is actually the case.

The only previous experimental work in this connection appears to have been done in the same laboratories (Karlsruhe), where our experiments were carried out. This was undertaken by Mr. Dex-

* Arnold, *Die Gleichstrommaschine*, vol. i., 2nd edition, p. 269.

† G. W. Worrall and T. F. Wall, *Journal, Institution of Electrical Engineers*, vol. 37, 1907, p. 148.

heimer, with apparatus designed by Professor E. Arnold, in whose book a summary of the results is given.*

In this research the measurements were made as follows. The air-gap was made so large that no losses due to eddy currents were present in the pole shoe, and the power thus required to drive the machine was accurately measured by means of a spring dynamometer. Then, with the same flux density and a decreased air-gap, the increase of power supplied was taken as equal to the losses in the pole shoe due to the fluctuations of the flux, caused by the armature teeth. Thus, by keeping the gap constant, and using successively massive and laminated pole shoes, the increase of the losses would be given by the difference of the readings, whilst the absolute losses in each case could be found from the power required to drive the machine.

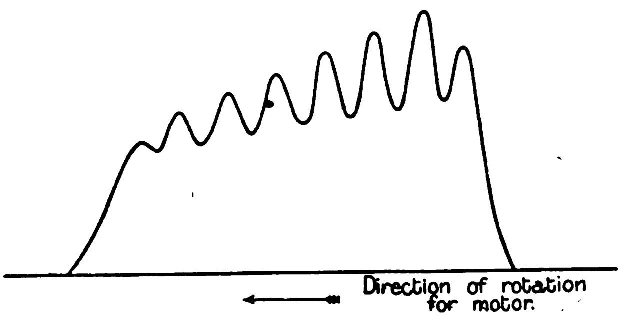


FIG. 2.—Flux Distribution on Load.

From a series of experiments made in this way, the following empirical formula was deduced :—

$$W_p = C\left\{(k_1 - 1)\,t_1\,B_l\right\}^{2\cdot 2}\left\{\frac{Zn}{60}\right\}^{1\cdot 7} 10^{-11} \text{ watts per sq. dcm. of pole surface;}$$

where—

$C =$ constant,
$Z =$ number of armature teeth,
$n =$ speed in revs. per min.,

and the other symbols as before.

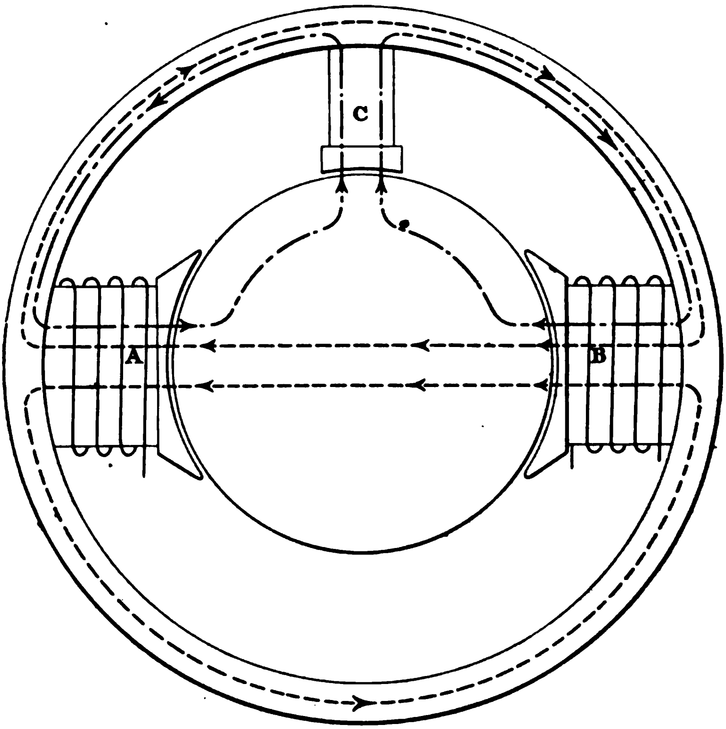
The chief difference between this empirical formula and the above theoretical formula is that the indices of B_l and n are higher in the former than in the latter.

Turning now to the experiments made by us. A description of the method has already appeared†, where the results of some preliminary tests were also given. The machine used for obtaining those results was the same as that used by Mr. Dexheimer in his

* Arnold, *Die Gleichstrommaschine*, vol. i., 2nd edition, p. 648.
† *Electrician*, vol. lvii., p. 568.

experiments; but this machine was not well adapted to the measurement of these losses by the method devised by us. Hence the experiments have been repeated and extended under more favourable conditions, and the results thus obtained are given below.

The experiments are based on the assumption that the whole of the energy loss, caused by the pulsating flux, is dissipated as heat, and the method consists in measuring the heat thus dissipated. This was done by determining the amount of energy—supplied from an external and measurable source—that must be expended in order to



Flux distribution when A and B have like polarity.

Flux distribution when A and B have unlike polarity.

FIG. 3.—Diagram of Experimental Machine.

obtain the same conditions of heating as when the actual losses are present. In other words, the heating due to eddy currents is imitated by artificial heating. It was of the highest importance, therefore, to make certain that all other forms of heating were identical in the two cases. This was done as follows :—

The experimental machine was a 5-H.P. dynamo with commutating poles, and had four main poles. Two of the main poles, which were diametrically opposite, were used for the production of the flux, the

other two along with all the commutating poles being removed. The experimental pole shoe was specially designed for these experiments. This was made of massive wrought iron and so arranged that the gap could be varied at will. The experimental pole is denoted by C in Fig. 3, whilst A and B represent the main poles. Suppose now that A and B are excited in such a way that A becomes a N pole and B a S pole. The flux will then pass directly from A to B and the *experimental pole* C

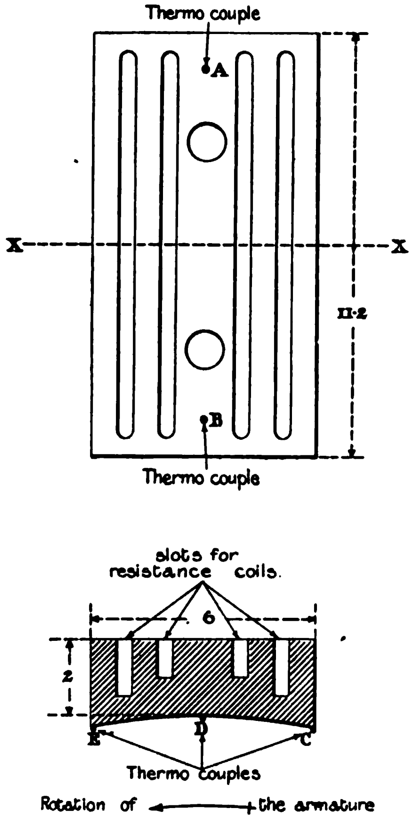


FIG. 4.—The Experimental Pole Shoe. (Dimensions in cm.)

will be free from flux, provided the magnet system is symmetrical with regard to it. Under these conditions, therefore, no eddy currents will be induced in the experimental pole shoe. Suppose now that the current in one of the exciting coils be reversed, the poles A and B will then have the same polarity, and the pole C will now form a return path for the flux issuing from A and B; further, whilst A and B have large pole shoes in comparison with C, it is at once obvious that

the flux density in the experimental pole shoe may be made to assume high values.

Curve I., Fig. 5, shows the flux density in the experimental pole shoe, when the main poles were of like polarity, whilst curve II. is the flux density curve when the main poles were of unlike polarity. From this it is seen that the experimental pole in the latter case can, for all practical purposes, be regarded as being free from flux.

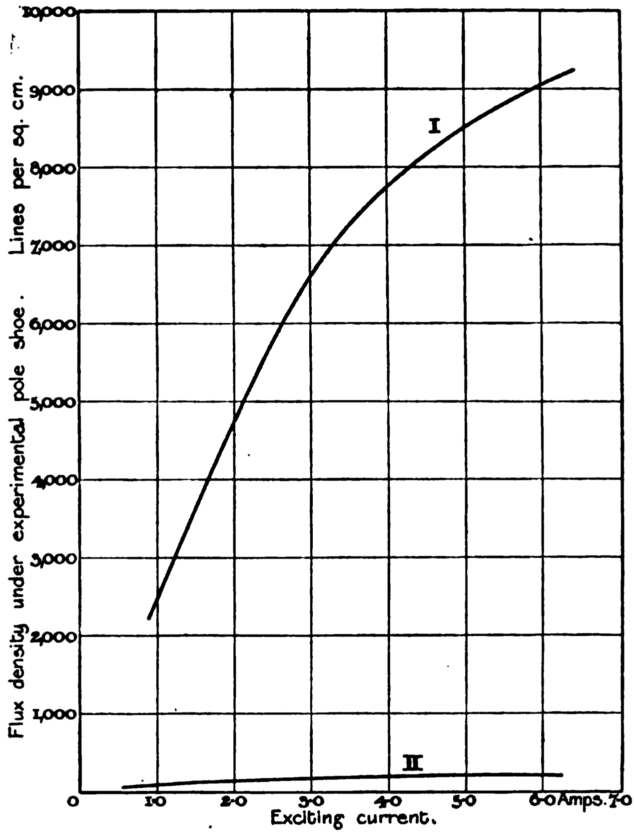


FIG. 5.—Magnetisation Curve of Experimental Pole.

Curve I. Main Poles A and B have *like* polarity.
Curve II. Main Poles A and B have *unlike* polarity.

In both the above cases, however, the heating due to the exciting current in A and B is precisely the same, provided the current is kept constant throughout. It will thus be seen that when A and B have unlike polarity, the heat in the pole shoe C is solely due to the heating of the machine; whilst when A and B have like polarity, the heating of the pole shoe C will be due to exactly the same causes as before, together

with the additional heating caused by the eddies and hysteresis produced by the flux pulsations in the shoe.

In passing, it may be pointed out that the iron losses in the armature will clearly not be the same when A and B have like polarity as when they have unlike polarity, and this might have an effect on the temperature of the pole shoe. By measuring the resistance of the armature winding, however, it was found that its temperature was practically the same in the two cases, consequently its effect on the heating of the pole system was constant.

Hence it may justly be concluded that the difference of temperature of the pole shoe in the two cases is due to, and is therefore a measure of, the losses caused by the pulsations of the flux in the shoe.

The experiment, therefore, resolves itself into finding out to what dissipation of energy this temperature-difference corresponds. This was done as follows. In the experimental pole shoe, Fig. 4, slots were cut, and in these slots resistance coils were laid. Each slot contained two coils, which could be connected non-inductively in series or parallel, as desired. Further, since the plane of the coils was perpendicular to the pole face, any possible flux, due to the current in the resistance (or heating) coils, which may have been present, was in the worst possible direction for producing any effect which might vitiate the results when it was desired to heat the shoe artificially. When the main poles were of opposite polarity, and consequently no flux went through the experimental pole, current was sent through these coils, and, by suitably adjusting this current, it was possible to make the temperature of the shoe the same as that produced by the eddy currents when the main poles had the same polarity and the flux went through the experimental pole. When this equality of temperature was reached, the power supplied to the heating coils was measured, and this power was then taken as equal to the loss of energy produced by the flux pulsations—an assumption which we now proceed to justify.

From the drawing in Fig. 4, it will be seen that the resistance coils are completely embedded in the pole shoe, so that all the heat developed by these coils must be absorbed by the pole shoe, to be afterwards dissipated from its surface. In comparing the losses given by the present experiments with those given in the preliminary report referred to above, it may be mentioned that in the latter case the heating coils projected beyond the ends of the pole shoe. This, coupled with the fact that the slot-openings were exceedingly wide in that case, may largely account for the fact that the losses were greater than those in the present experiments. To prevent as much as possible a transfer of heat the pole shoe was insulated from its core by a sheet of mica.

The temperature of the pole shoe was measured by means of thermo-couples, balanced on a potentiometer. There were five thermo-couples in all—shown by A, B, C, D and E in Fig. 4. These

were made by simply soldering copper wires on the shoe at these points, whilst an iron wire soldered to the shoe served as a common terminal to all the couples. In this way it was possible to explore the temperature-distribution over the whole shoe, both when it was heated by the losses and when it was heated artificially. This was important, since the validity of the equivalence of the artificial heating to the heating by the eddy losses depends chiefly on the distribution of the temperature being the same in the two cases. (A comparison of these is made in Tables II. and III.)

The arrangement of the potentiometer is seen from Fig. 6. When the galvanometer gave no deflection the distance *a b* gave a measure of the temperature of the shoe. Of course, for the purpose of the

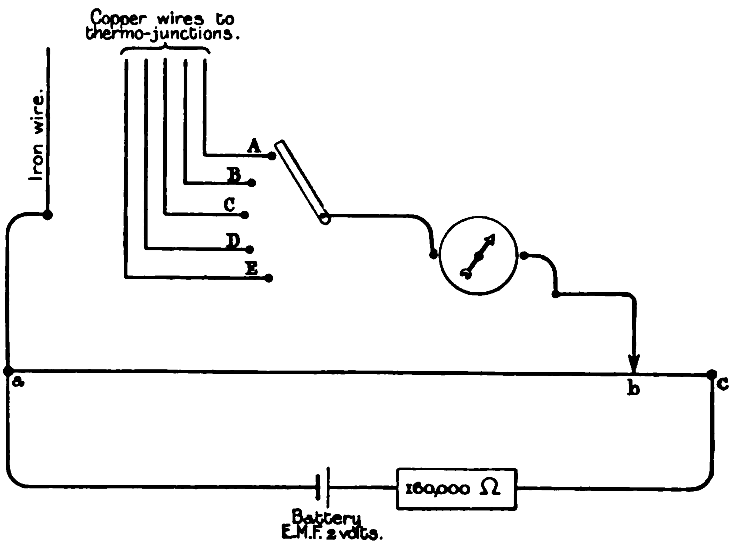


FIG. 6.—Scheme of Connections for Measuring Temperature of Pole Shoe.

experiments, the actual temperature of the shoe had little significance, consequently everything was measured in potentiometer divisions, which served as a very useful unit, measuring, since it was possible to measure, to the finest division on the instrument. An idea of the extreme sensitiveness of the balance may be gathered from the fact that the resistance of the potentiometer wire was increased by 160,000 ohms placed in series with a 2-volt battery, the resistance of the mirror galvanometer being 200 ohms.

Before any readings were taken, the machine was always allowed to run long enough for the temperature to reach an approximately steady state. The time taken for it to warm up varied from three to five hours, depending chiefly on the strength of the current in

the exciting coils. A curve of temperature-rise is shown in Fig. 7, taken when the current in the field coils was 4 amperes. This was the maximum exciting current used, consequently this curve represents the most unfavourable conditions, and it will be seen that the temperature became practically steady after five hours.

The order of each experiment was as follows. The machine was allowed to warm up with flux (and therefore eddy currents) in the experimental pole shoe, then, after attaining a steady temperature, the E.M.F.'s of the thermo-couples were balanced. After this, the current in one of the exciting coils was reversed and at the same time artificial heating was applied by means of the resistance coils; then, after one to two hours, the temperature would have again become steady, and the thermo-couples could be balanced. This had to be repeated, by varying the heating current in the resistance coils, until

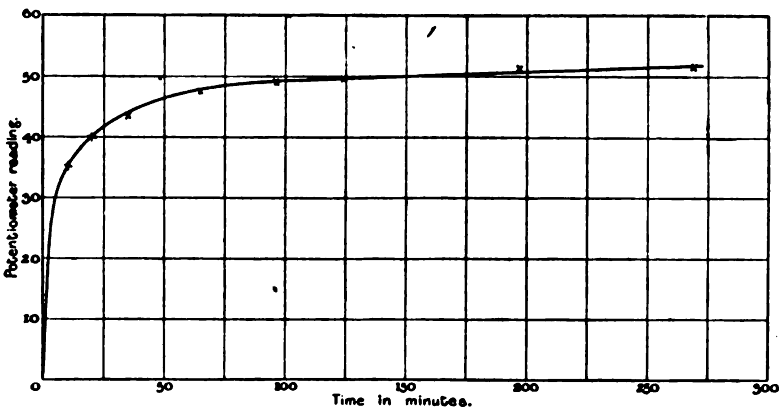


FIG. 7.—Curve of Heating for the Machine with Maximum Exciting Current.

sufficient points were obtained for drawing a curve containing the point corresponding to the potentiometer readings with flux in the pole shoe (see example in experiments A).

EXPERIMENTS A.

The first series of experiments was made to find the law connecting the eddy losses in the pole shoe with the flux density. These losses were measured for various flux densities, and are given in Table I. As will be seen, the losses in each case have been reduced to the "losses per square decimetre of pole shoe face," and are given in watts (area of pole shoe face $= 1 \cdot 12 \times 0 \cdot 6 = 0 \cdot 67$ dcm.^{2}).

TABLE I.

Showing the Relation between the Eddy Losses in the Pole Shoes and the Flux Density.

Flux Density in Air-gap under Pole Shoe. B_l	Losses in Watts per Sq. Dcm. of Pole Face. W_B
2,500	8·2
4,700	26·8
6,700	54·3
7,800	76·0

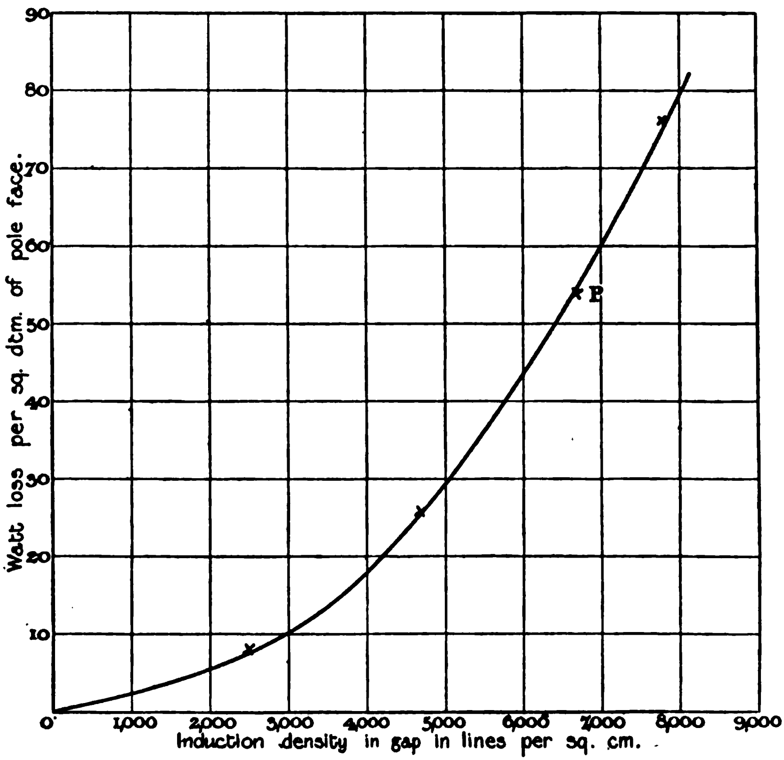


FIG. 8.—Curve showing Relation between Losses in Pole Shoe and Flux Density in Air-gap.

Experiments A $\begin{cases} \text{Air-gap constant } \delta = 1{\cdot}9 \text{ mm.} \\ \text{Speed constant } n = 580. \\ \text{Armature current} = 0. \end{cases}$

These readings are shown plotted in Fig. 8, and the equation for the curve is—

$$W_B = k B_l^{2 \cdot 1} \text{ watts per sq. dcm.}$$

where k is a constant.

If these losses are calculated from Potier's or Rüdenberg's theoretical formula, they will be found to be considerably lower than the above. Further, those obtained by Dexheimer's experimental formula are also less than those given in Fig. 8.

As an example of the readings taken for finding the points for this curve, the accompanying Tables II. and III. are given, from which the point marked P on Fig. 8 was obtained. In Table II. the readings refer to the first part of the experiment, where the heating was produced by eddy losses, whilst from Table III. is obtained the heating curve, when the shoe was warmed by the resistance coils.

TABLE II.

Heating due to Eddy Losses in the Shoe.

Reading taken after 4 hours' running.
Flux density in gap under the pole, $B_l = 6{,}700$.

Thermo-couple.	A.	B.	C.	D.	E.	Mean of A. B. C. D. E.
Potentiometer reading	27·0	25·5	29·5	27·0	25·5	26·9

TABLE III.

Heating due to Current in the Resistance Coils.

Readings taken at intervals of 1½ hours.
Flux density in gap under the pole shoe, $B_l \backsim 0$.

Thermo-couples.	A.	B.	C.	D.	E.	Mean of A. B. C. D. E.	Resistance Coils. Volts.	Amps.	Watts.
Potentio-meter reading.	20·1	20·5	20·1	20·5	21·7	20·60	11·4	2·0	22·8
	26·0	25·2	26·2	26·5	27·5	26·30	14·0	2·5	35·0
	31·5	31·3	32·5	32·5	34·0	32·36	16·3	3·0	48·9

The curve in Fig. 9 was then plotted with the mean values of the potentiometer readings for the five thermo-couples as abscissæ and the corresponding watts consumed in the resistance coils as ordinates. Thus from Table III. is obtained the curve for the heating of the pole

shoe which now serves as the calibration curve for the particular experiment in question. Taking, then, the mean value 26·9 of the potentiometer readings in Table II., the corresponding watts are 36·5 (see point P in Fig. 9). Reducing this to the loss per square decimetre of pole face, we get for point P, when the flux density is 6,700 :—

$$\text{Eddy current loss} = \frac{36{\cdot}5}{6 \times 11{\cdot}2} \times 100 = 54{\cdot}3 \text{ watts per dcm.}^2.$$

This point P is then plotted on the curve showing the relation between the losses and the flux density (see Fig. 8).

A similar process had to be repeated for each point, and owing to

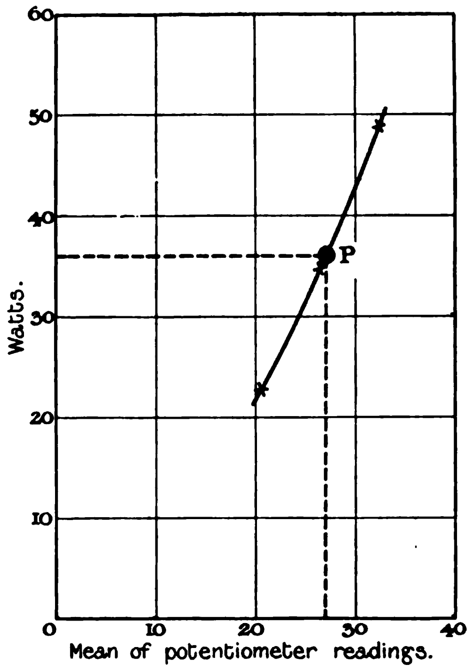


FIG. 9.—A Calibration Curve for Heating of Experimental Pole Shoe.

the time required to reach a steady state whenever a reading had to be taken, the whole day would be occupied in obtaining one point. Thus, on the average, the number of points of any of the following curves represents the number of days (neglecting mishaps) spent in taking that curve.

EXPERIMENTS B.

This series of experiments was devoted to finding the relation between the pulsation losses in the pole shoe and the speed of the armature.

The several points were obtained in the same way as the points in the previous series of experiments, except that in the present case the only variable was the speed.

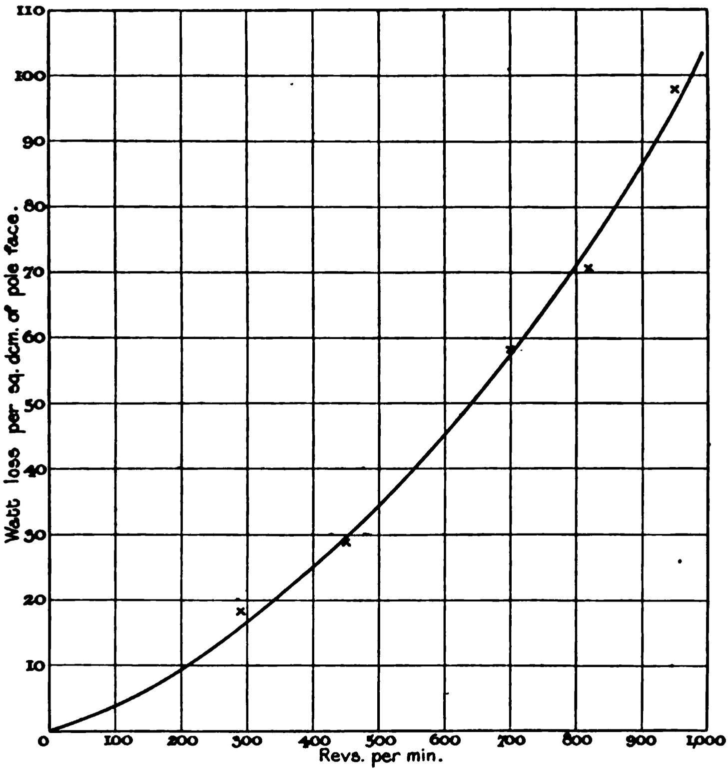


FIG. 10.—Curve showing Relation between Losses in Pole Shoe and Speed of Armature.

Experiments B { Air-gap constant δ = 1·9 mm. Flux density constant B_l = 5,800. Armature current = 0.

The data obtained are given in Table IV., and the curve from these plotted in Fig. 10. The equation of this curve is—

$$W_n = k\left(\frac{Z\,n}{60}\right)^{1\cdot 5} \text{ watts per dcm.}^2,$$

where Z = number of armature teeth = 53,
n = speed in revs. per min.,
and k is a constant.

TABLE IV.

Showing the Relation between the Eddy Losses in the Pole Shoes and the Armature Speed.

Speed in Revs. per Min. n.	Losses in Watts per Dcm² of Pole Face. W_n.
290	18·6
450	29·0
700	58·5
820	70·6
950	98·1

EXPERIMENTS C.

The object of this series of experiments was to find the effect of the ratio of the length of the air-gap to the slot-opening on the eddy current losses in the pole shoes. To keep the flux constant whilst the

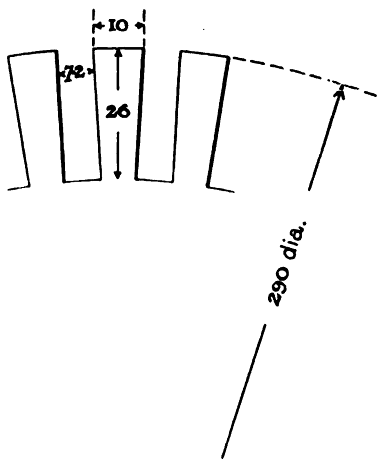


FIG. 11.—Showing Armature Teeth.

gap was varied the following device was used. The exciting current in the field coils was kept constant throughout, and the gap was varied by inserting pieces of cardboard between the pole shoe and the pole core. By this means both the magnetomotive force and the reluctance of the magnetic circuit (neglecting any alteration in the leakage

coefficient brought about by the above change) were kept constant, consequently the flux must remain constant. That this was actually the case was confirmed by the ballistic galvanometer. In this way it was possible to change the effective gap, *i.e.*, the distance between the armature core and the pole shoe, without having to take a magnetisation curve in each case.

The gap was thus varied from about 6 mm. to a mean value of 1·5 mm., but it was found that the losses were imperceptible when the gap was greater than about 4 mm.

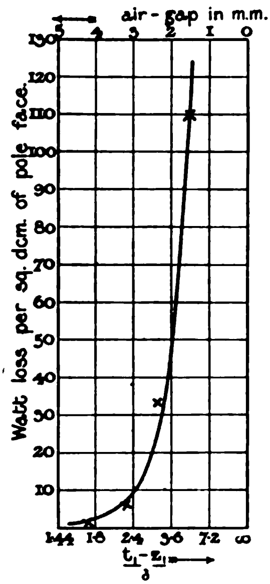


FIG. 12.—Curve showing Relation between Losses in Pole Shoe and Ratio $\frac{\text{Slot-opening}}{\text{Air-gap.}}$

Experiments C $\begin{cases} \text{Speed constant } n = 900. \\ \text{Flux density constant } B_l = 6{,}000. \\ \text{Armature current} = 0. \end{cases}$

It is not of much interest, however, to know how the losses vary with the length of the air-gap without at the same time knowing the relation to the opening of the slot and the width of the tooth; for upon this relation the losses depend, as expressed by the coefficient k_1 quoted in the formulæ in the early part of the paper. The dimensions of the teeth are shown in Fig. 11, whilst Fig. 12 is plotted from the data given in Table V. The equation of this curve is—

$$W_\delta = k\left(\frac{t_1 - z_1}{\delta}\right)^{3\cdot5} \text{ watts per dcm.}^2;$$

where—

t_1 = tooth-pitch at armature circumference,
z_1 = tooth-width at armature circumference,
$t_1 - z_1$ = slot-opening at armature circumference,
δ = mean length of air-gap (measured in same units at t_1 and z_1),
and k = a constant.

TABLE V.

Showing the Relation between the Eddy Losses in the Pole Shoes and the Ratio of the Slot-opening to the Air-gap.

Mean Length of Air-gap in Mm., δ.	$\frac{\text{Slot-opening}}{\text{Air-gap}} = \frac{t_1 - z_1}{\delta} = \frac{7{\cdot}2}{\delta}$	Losses in Watts per Dcm.² of Pole Face. W_δ.
1·50	4·80	110·0
2·35	3·06	34·2
3·20	2·25	5·9
4·20	1·70	1·5

An idea of the magnitude of the heating may be gained from the fact that, with the smallest gap (1·5 mm.), it became impossible to hold the hand on the experimental pole shoe after the machine had been running a few minutes.

EXPERIMENTS D.

All the previous experiments were carried out when the machine was running without current in the armature. It is also of importance, however, to know what effect the load has on the eddy losses set up in the pole shoes by the fluctuations in the field caused by the teeth. At the beginning of the paper attention was drawn to the distorting effect of the armature field, and Fig. 2 was given, showing the waves of the pulsations.

The current was sent through the armature as follows: When running without flux in the experimental pole, the machine was driven by a motor, and current was simply passed through the armature from an external source. The armature being wave-wound, with the brushes at 90° (geometrical) apart, no E.M.F., of course, was induced between the brushes due to the rotation of the armature in the field, so that the armature winding acted simply as an ohmic resistance. When running with flux in the experimental pole shoe, the machine ran as a motor,

and was loaded until the same current flowed in the armature, as in the previous case. The exciting current was maintained constant throughout.

TABLE VI.

Showing the Relation between the Eddy Losses in the Pole Shoe and the Current in the Armature.

Armature Current in Amperes.	Current-volume or Ampere-conductors per Cm. length of Armature Periphery.	Losses in Watts per Dcm.² of Pole Face.
0	0	26·1
7·9	55	29·6
15·4	107	35·2
23·6	165	40·0
32·2	224	40·8

The results obtained are tabulated above (Table VI.), whilst Fig. 13 shows how the eddy losses in the pole shoe increase as the load increases, which is what we should have predicted on account of the distortion of the field.

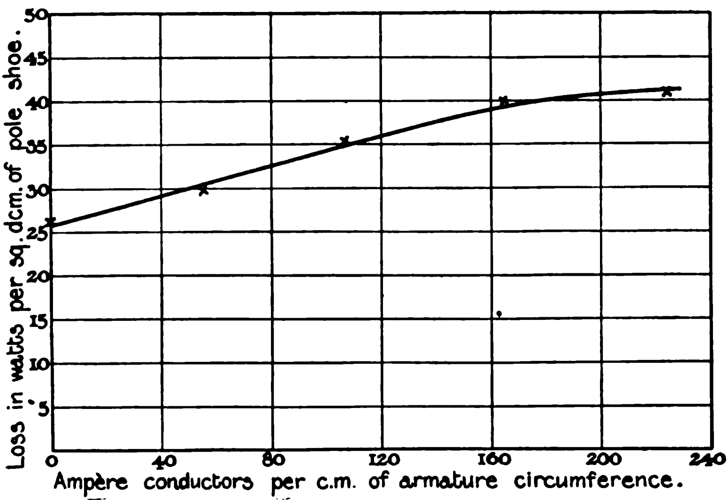


FIG. 13.—Curve showing Relation between Losses in Pole Shoe and Load Current.

Experiment D { Air-gap constant $\delta = 1{\cdot}9$ mm. Flux density constant $B_l = 4{,}700$. Speed constant $n = 600$.

The readings given by the thermo-couples in this series of experiments are illustrated in Table VII., and the distribution of temperature in this case, when artificial heating was applied, is shown in Table VIII.

TABLE VII.

Heating due to the Eddy Losses in the Shoe.

Reading taken after $3\frac{1}{2}$ hours' running.
Armature current $= 23{\cdot}5$ amperes.

Thermo-couple.	A.	B.	C.	D.	E.	Mean of A, B, C, D, E.
Potentiometer reading	26·5	25·0	29·0	25·0	25·0	26·1

TABLE VIII.

Heating due to Current in Resistance Coils.

Reading taken at intervals of $1\frac{1}{2}$ hours.
Armature current $= 23{\cdot}5$ amperes.

Thermo-couple.	A.	B.	C.	D.	E.	Mean of A, B, C, D, E.
Potentiometer reading	26·5	26·5	27·5	27·0	28·0	27·1
" "	23·5	23·5	24·0	24·0	24·5	23·9

CONCLUSIONS.

Summarising the results given by the above experiments, the following equation is obtained for the losses in pole shoes due to eddy currents induced by the pulsations in the field set up by the armature teeth :—

$$W_p = k\left(\frac{t_1 - z_1}{\delta}\right)^{3\cdot5} B_t^{\ 2\cdot1}\left(\frac{Z\,n}{60}\right)^{1\cdot5} 10^{-11} \text{ watts per dcm.}^2 \text{ of pole face;}$$

where—

$k =$ a constant depending on the iron, etc.,
$= 0{\cdot}046$ in the present case.

Far more can be learnt from the curves directly, however, than from such an equation, which can be nothing further than a comparison. Moreover, it is much better to know how to avoid these losses than how to calculate them, and to this end the above curves

form a comparatively safe guide. Undoubtedly the most important of them all is the curve showing the relation between the losses and the ratio of the slot-opening to the length of the air-gap (Fig. 12). This curve—which was taken for a fairly high flux density in the gap (6,000) and a moderately high speed (900)—shows that, provided the ratio $\frac{\text{slot-opening}}{\text{air-gap}}$ does not exceed 1·5, the losses in the shoe are practically zero. Even when this ratio is as high as 1·8, the eddy losses are only some 3 watts, and probably the designer's practical rule of keeping this ratio below 2 can be accepted as about right. At any rate, this may safely be taken as the limit at which it is necessary to discard solid pole shoes in favour of laminated. In other words, unless the gap is very small (*i.e.*, $\frac{t_1 - z_1}{\delta}$ very large), or the flux density is very great, or the speed is very high, the use of laminated shoes is not necessary—for the small increase thereby gained in efficiency does not compensate for the increased cost of construction.

Finally, we take this opportunity of expressing our appreciation of the valuable advice and assistance which Dr. E. Arnold has at all times so readily given, and the facilities for research work placed at our disposal in the laboratories in the Electrotechnical Institute, Karlsruhe.

Discussion.

Dr. W. M. Thornton: With reference to the remark in connection with Potier and Rüdenberg's formula that the flux pulsations are probably not sinusoidal, I may point out that the effects of hysteresis would make the wave-form far from a sine curve. It would be a "spiky" wave-form. Dr. Thornton.

The losses shown in Fig. 8 are probably greater than those obtained in practice on account of the small air-gap. The numerical values of the constants in the various formulæ must largely depend upon the conductivity of the iron. The curve of Fig. 7 does not do itself justice, and might well have been drawn through the experimental points so as to have been quite horizontal after four hours' run. I should like to ask whether the presence of the slots for resistance coils in the experimental pole shoe would not disturb the heat flow, and whether the thickness of the shoe would not have some effect in this connection.

The results in Fig. 8 are expressed in terms of watts loss per square decimetre of pole-face; to convert into English units, watts per square foot, it is necessary to multiply by 9·3. A figure of half a kilowatt per square foot of pole-surface is thus easily obtained—a most important loss in practice. Referring to Fig. 13 the extended importance of this loss on a loaded machine is seen: presumably the pole-face loss is increased 50 per cent. at full load.

It is almost certain that the influence of the movement of the magnetic lines caused by the passage of the teeth is more than a skin effect. When the poles are solid the eddy currents are certainly more in a

Dr. Thornton. condition of "boiling" than of steady sinusoidal flow—more like turbulent motion in water flow, at least towards the armature end of the pole-core. For one reason, not only is there a weakening and strengthening of the field, but a variation of the leakage lines which wave in and out, cutting the flanks of the poles and shoes, but not the ends. The resultant eddy-current path must therefore be irregular.

The statement concerning the relative cost of laminated and solid pole shoes may be challenged, as there is a widespread idea that laminated pole shoes are cheaper, not dearer, than solid. I would ask if any experiments were made on the influence of the shape of the pole shoe and the number and depth of teeth under the shoe. There is also the influence of the shape of the pole-tips and the effect of ventilation of the spaces between the poles to be noticed.

Mr. Turnbull. Mr. C. TURNBULL: I should like to know if the authors have anything to say with regard to "humming." Some makers have apparently solved the problem, but others do not appear to have done so. As regards the cost of laminated pole shoes, as compared with that of solid shoes, the author mentions on page 595 that if the ratio of the slot opening to the air-gap does not exceed 1·5, the losses in the shoe are practically nil. Makers, however, find it cheaper to make the slot openings large, using laminated pole shoes and a small air-gap.

Mr. Hunt. Mr. F. O. HUNT: I should like to know how the thermo-couples were arranged on the pole. Some kind of illustration would have been useful to show how the heating was distributed. Another point is that the variations in the length of the air-gap seem to have been obtained by putting on what one might call a "non-magnetic slice," and I should like to know if the slice was of the same area as the pole shoe.

If there is any considerable difference in sectional area of limb and pole face the new "gap" does not correspond to the part by which the original "gap" has been shortened.

Have the authors made any checking experiments upon the change of relative curvature of armature and pole face? Dr. Thornton mentioned that he considered eddy-current effects likely to go deeper into the pole face than has been assumed by the authors, but I do not think he put forward all the evidence he might have done.

It would seem that the stream-line experiments shown to us by Dr. Thornton himself afford almost conclusive proof that the "tufting" of the magnetic field goes considerably deeper than would, at first sight, be supposed, and where this uneven distribution occurs eddies must be present.

Mr. Carter. Mr. T. CARTER: The authors are perfectly correct in saying that the most important set of experiments is that summed up in the curve given in Fig. 12. The conclusion that the ratio of slot-opening to air-gap should be less than 2 if pole shoes are not to be laminated, seems almost to preclude the use of solid pole shoes altogether, at any rate in the smaller sizes of a range of machines. For, to get the ratio less than 2, either the slot-opening must be small or the air-gap large. The former means generally a large number of slots, involving great

expense in notching and a large amount of waste space in slot insulation; and the latter involves extra field copper, unless machines are to be worked with weak fields, which are again undesirable from many points of view which do not concern the present discussion. From my own experience, I should say the tendency is to arrive at values of the ratio increasingly in excess of 2, and so to make absolutely necessary the use of laminated pole-shoes or complete laminated poles. If this be so, the experiments other than those in section C are interesting more from a theoretical than from a practical point of view, as they concern conditions which section C shows ought not to occur in practice. Mr. Carter.

The authors suggest that laminated poles involve increased cost of construction. I doubt this. In fact, I know that figures could be produced showing that laminated poles are actually cheaper than solid poles and pole shoes as regards material and labour (given correct methods of manufacture). It must also be remembered that solid cast poles are very uneven in their surface, and very much more clearance between them and the field coils is required than in cases where laminated poles with accurately flat surfaces are used, so that the latter tend also to some saving in copper, other things being equal.

The authors are deserving of sincere thanks for their paper, the value of which is considerable as a source of information upon a subject concerning which data are, as a rule, difficult to find.

Dr. ALFRED HAY (*communicated*): The authors' ingenious method of reproducing the thermal condition of the pole-piece by the aid of heating coils embedded in it does not appear to hold out much promise of success at first sight, but the consistent results obtained by them show what may be achieved by patience and experimental skill. The curves and tables contained in the paper should prove of great interest to all dynamo designers. Dr. Hay.

There is one matter connected with the pulsation losses to which it may be worth while to draw attention. The depth to which the disturbances penetrate from the polar surface inwards would appear to depend—up to a certain limit—on the ratio of the pole-arc to the pitch of the teeth. If we consider a very narrow pole—to take an extreme case, one whose width does not exceed the width of a single slot (a case which would, of course, never occur in practice, even with commutating poles)—then it is evident that the variations in the magnetic flux throughout the entire circuit of such a pole would be considerable, and the magnetic disturbances would extend practically along the entire length of the pole. As the width of the pole increases, the percentage fluctuations in the total flux become less and less, and the disturbances due to the armature teeth become more and more localised, being confined chiefly to a comparatively small depth within the pole shoe itself, the variations in the total flux higher up the pole-piece having become inappreciable. In the most general case, the pulsation losses occasioned by the teeth may be regarded as made up of (1) the losses due to changes in the total reluctance of the

Dr. Hay. magnetic circuit, and (2) those due to lateral local swaying of the flux close to the polar surface. In the case of commutating poles, these two sources of loss are probably of about the same order of magnitude; while in a main pole, the second kind of loss preponderates, the first kind being negligible.

BIRMINGHAM LOCAL SECTION.

DISCUSSION, *February* 12, 1908.

Dr. Sumpner. Dr. W. E. SUMPNER: The members will heartily agree with me in saying that our thanks are due to the authors for their paper. The tests have been carefully designed and carried out, and the curves of experimental results will be found valuable. I am afraid, however, the empirical formula given is far too complicated for ordinary use, and has no theoretical justification. The results would be more clearly shown by curves suitably plotted, so as to make them useful for ready reference.

Mr. Coales. Mr. J. D. COALES: The losses in the pole shoes, as measured by the authors, should not be entirely attributed to eddy currents, but must include a certain (though possibly small amount) of hysteresis loss. The method of varying the air-gap adopted by the authors will not keep the reluctance of the circuit quite constant, owing to the changes produced in the distribution of the lines in the neighbourhood of the teeth. This may be inferred from the formula on page 564 of Mr. Wall's other paper on "The Reluctance of the Air-gap in Dynamo Machines."

Mr. Bartlett. Mr. A. T. BARTLETT: I should like to ask why laminated pole shoes have not been used in connection with the research, as the numerical results from these would have been much more valuable than those obtained.

Dr. Kapp. Dr. G. KAPP: I should like to ask why the authors consider it necessary to use for electric heating coils which have very little self-induction. Did they use alternating current for heating?

Mr. Wall. Mr. THOMAS F. WALL (*in reply*): With regard to Dr. Thornton's remarks as to the depth of the pulsations in pole shoes, it is quite obvious that if the boiling effect takes place as he suggests, it is hopeless to attempt to calculate the losses. Some time ago I tried to deduce the amount of these losses from a knowledge of the electromotive forces induced in the pole shoes by the flux pulsations. The depth to which the eddy currents penetrated was estimated, and from this the resistance of the eddy currents' circuit determined; but, as far as I recollect, the results were too high.

As regards the comparative cost of laminated and massive pole shoes the whole question is rather complicated, for it is clear that not merely the cost of production of the shoes themselves, but the whole design of the machine is affected by the type of pole shoe used.

I have not much information as to the "humming" mentioned by Mr. Turnbull, but it was distinctly noticeable that when the air-gap

was small the "humming" was very pronounced; and it appears, therefore, that the intensity of the "humming" depends on the ratio of slot to gap. Mr. Wall.

Mr. Hunt has referred to the method used for varying the air-gap. The pole-core was of slightly less width than the pole shoe, but experiment showed that the flux density was not appreciably altered; on that account, when the "non-magnetic slice" was varied, no experiments were made to find the difference caused by the relative curvature of the pole shoes to the armature face, and it is not quite easy to see how this could be checked; for if pole-pieces of various curvatures had been used, the comparison would not have been altogether satisfactory, as difference in quality of material, working, etc., would also have entered into the question.

In the case of the above experiments the ratio of pole-width to slot-pitch was 3·5, and, as Dr. Hay has pointed out, there must have been considerable magnetic disturbance throughout the whole circuit; and in all probability if the ratio of pole-width to slot-pitch had been a whole number, the losses due to the flux pulsations would have been much less per unit of pole-shoe area than in the case examined.

In reply to the discussion at Birmingham. We agree with Dr. Sumpner that the use of such a formula as that deduced from the experiments is not desirable for practical purposes, and the object in putting the results in that form was to compare them with the values as deduced from theoretical considerations.

In reply to Mr. Coales, there will be some hysteresis loss due to the flux pulsations, but this will be negligibly small in comparison with the loss due to eddy currents. The experiments are not exhaustive, as Mr. Bartlett suggests, and it would be desirable to repeat them for laminated pole shoes as well as to find the effect of temperature, and also the effect of hardening the surface of a cast shoe.

With reference to Professor Kapp's remarks as to the winding of the auxiliary heating coils, these were wound two in one slot so that they could be connected up non-inductively, the object being to get rid as far as possible of the flux due to the currents in the coils, which were heated by direct current. Moreover, by winding the coils two in one slot, they could be connected in series or parallel according to the current required for heating, but it was not found necessary to change the connections at all throughout the experiments.

I should like here to suggest another method by which these losses could be measured, and that is by "running down" curves. All that is necessary is, in the first place, to have a machine in which the air-gap under one pole could be varied. Then, by choosing a gap so large that there would be no appreciable loss due to flux pulsations, a "running down" curve would be taken and the power dissipated by all the losses in the machine deduced in the usual way from this curve. The air-gap under the experimental pole would then be reduced to a value which would allow of eddy current losses in the pole shoe, and,

Mr. Wall. keeping the flux density constant, another "running down" curve would be taken. The power deduced from this latter curve would then give all the losses occurring in the first case plus the losses due to eddy currents in the pole-shoes, and by simply subtracting the results the losses could be at once deduced. I hope to publish an account of some experiments made in this way later on.

In conclusion, I would like to express on behalf of Mr. Smith and myself our appreciation of the kind way in which the paper has been received.

BIRMINGHAM LOCAL SECTION.

THE HEAT CONDUCTIVITY OF IRON STAMPINGS.

By THOMAS MORGAN BARLOW, M.Sc., Student.

(*Received from the* BIRMINGHAM LOCAL SECTION, *October* 11, 1907, *and read at Birmingham, January* 15, 1908.)

SYNOPSIS.

Introduction and object of research—Theory of flow of heat or heat conduction—Description of apparatus—Temperature measurement by thermo-junctions—Test on German silver and silver thermo-electric couples—Method and connections of conductivity tests—Theoretical calculation of the conduction coefficient across iron and paper—Calculation of curves for temperature rise in iron stampings—Method of calculating temperature gradient for variations of induction.

Appendix.—Determination of cooling constants for iron laminations (edges only)—Summary of research.

INTRODUCTION AND OBJECT OF RESEARCH.

The most important subject to be considered in the design of all electrical machinery is the question of heating, as it not only affects the designer and maker, but also the buyer and user.

Heat is being continuously generated in all dynamo electrical machinery, both in the iron portions subjected to a varying flux, and in conductors carrying current. This implies that energy is being dissipated in the form of heat by conduction, convection, and radiation. By far the most important is the dissipation by conduction. In fact, the limit of output is regulated by the temperature to which any part may rise in a given time. The rise of temperature, however, obviously depends upon the amount of cooling surface provided, and also its effectiveness under the given conditions to dissipate heat.

Inefficient cooling affects the following :—

1. Efficiency.
2. Voltage regulation.
3. Durability of insulation.

Considering the first—coils which are embedded in iron (laminated) which has not sufficient cooling surface for the conditions agreed upon over-heat, thus causing great increase in the resistance, and therefore lower efficiency. For the same reason the voltage regulation is bad, and the compounding of the machine is liable to be upset. Also the iron losses are increased with the rise of temperature. Continual over-heating may also char the insulation, breaking it down, and thus causing a short circuit. These few points show especially that the iron part usually laminated and heated internally by hysteresis and

eddy currents must be carefully designed as regards cooling surfaces. This can be most efficiently done if the heat conduction coefficient is known in all directions from the source of heat. Further, the maximum temperature to which any part of the machine may rise after a steady state has been reached depends upon two physical quantities, namely, the internal conductivity of the material in question, and the external conductivity or method of cooling.

Hence the main object of the research undertaken was to determine (1)* the conductivity of heat in a direction parallel to the insulated iron stampings, and (2) the conductivity in a direction at right angles to the plane surface of the stampings, *i.e.*, across iron and paper (insulation).

No attempt was made to use artificial cooling by oil or forced draught, as the method employed did not lend itself to air cooling other than by the natural convection currents in the air.

Theory of Flow of Heat or Heat Conduction.

Consider the case of a lamina or wall with parallel faces. One face kept at a fixed temperature θ_1 while the other is maintained at θ_2. If

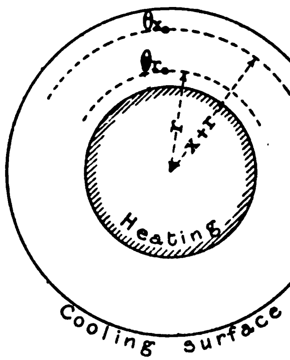


Fig. 1.

there be established a permanent state and uniform flow of heat the temperature may be taken to fall uniformly if the wall be made of the same material throughout, and if the conducting power does not depend upon the temperature.

From first principles the quantity of heat which flows through such a wall is directly proportional to the difference of temperature $(\theta_1 - \theta_2)$ of its faces. The quantity of heat which flows through an area A of such a wall in time t seconds will be proportional to A and t, also if Q be the quantity of heat, a the thickness of the wall, and K be the heat conduction coefficient depending upon the nature of the substance and numerically equal to the quantity of heat which flows per unit time

* *Mitteilungen über Forschungsarbeiten auf dem Gebiete des Ingenieurwesens*, Nos. 35 and 36. Dr. Ott, "Wärmeleitvermögen der lamellierten Armatur."

through unit area of a plate of unit thickness having unit difference of temperature between its faces, then—

$$Q = K . \frac{\theta_1 - \theta_2}{a} A t.$$

(The units in all calculations are Centigrade and C.G.S.)

If the plate has an infinitely small thickness dx and an infinitely small temperature difference $d\theta$, then the quantity of heat which flows through it in a small time dt is—

$$d Q = K A \frac{\theta - (\theta + d\theta)}{dx} dt . = - K A \frac{d\theta}{dx} dt,$$

or when the steady state has been reached—

$$- d\theta = \frac{1}{K} . \frac{Q}{A} dx \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Now consider the flow of heat through a circular lamina heated from the inside (see Fig. 1). This corresponds to the actual case of the experiment. Let the temperature at a distance r cms. from the centre be θ_r and at a radius $x + r$ be θ_x. For a distance dx and a temperature difference $d\theta$ from equation (1)—

$$- d\theta = \frac{1}{K} \frac{Q}{A} dx.$$

Therefore at a radius $(x + r)$—

$$- d\theta = \frac{1}{K} \frac{Q\, d(x + r)}{2 \pi (x + r)}$$

at radius r—

$$- d\theta = \frac{1}{K} \cdot \frac{Q\, d r}{2 \pi r},$$

i.e., by integration—

$$- \theta_x = \frac{1}{K} \cdot \frac{Q}{2 \pi} . \log_e (x + r) + C_1,$$

and—

$$- \theta_r = \frac{1}{K} \cdot \frac{Q}{2 \pi} \log_e (r) + C_1.$$

Therefore—

$$(\theta_r - \theta_x) = \frac{1}{K} \cdot \frac{Q}{2 \pi} \log_e \left\{ \frac{x + r}{r} \right\}.$$

If the temperature difference between the two points be denoted by θ, then—

$$\theta = \frac{1}{K} \cdot \frac{Q}{2 \pi} . \log_e \left(1 + \frac{x}{r} \right).$$

If l cm. be the length of a cylinder built up of stampings or laminæ, the surface of the cylinder being the total cooling surface, then—

$$\theta = \frac{1}{K} \frac{Q}{2 \pi l} . \log_e \left(1 + \frac{x}{r} \right) \quad . \quad . \quad . \quad . \quad . \quad (2)$$

From this formula the values of K for iron stampings, both in the direction parallel to the plane surface of the stampings and across the iron and paper at right angles to the surface, were calculated.

If K_a be the heat conduction coefficient for iron laminations in a direction parallel to the surface of one stamping, and K_b in a direction at right angles to this surface, *i.e.*, across iron and paper or insulation, then their ratio $= \frac{K_a}{K_b}$.

If C equals the ratio of the sides of two square surfaces, one being

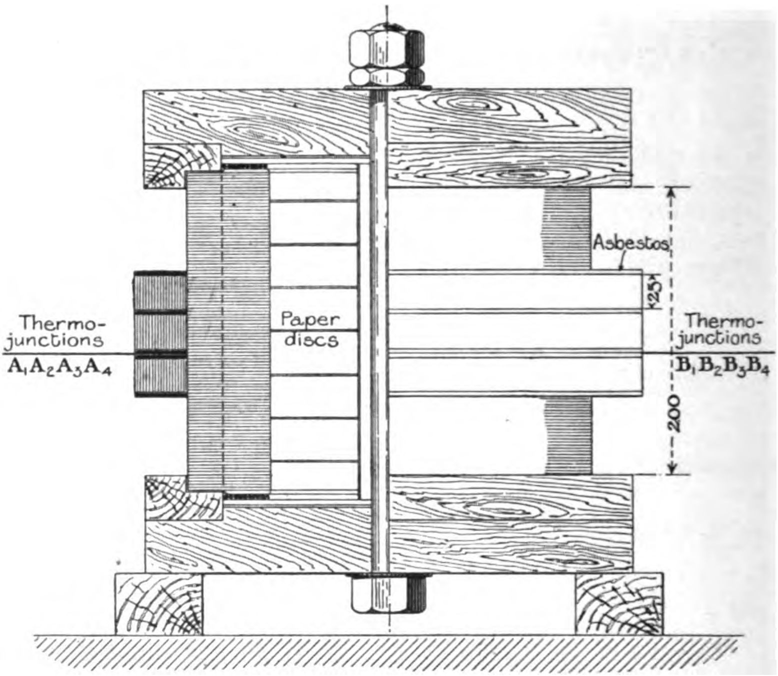


FIG. 2.—Section and Elevation of Apparatus.

the edge surface of a packet of stampings and the other being the plane surface of the outside stamping of the packet, and if both these surfaces are made to dissipate the same amount of heat, *i.e.*, equal cooling, then—

$$C = \sqrt{\frac{K_a}{K_b}}.$$

APPARATUS.

To obviate the difficulty of calculating the heat leakage if the iron stampings were heated on one side, a cylinder built up of stampings

was used, being heated on the inside edge and cooled on the outside (see Fig. 2). This was built up of stator stampings made by Messrs. Sankey & Co., Bilston, 0·0345 cm. thick, and insulated with a preparation known as "Insuline." The plates were firmly clamped together, and the slots wound with ten turns of D.C.C. copper wire round each tooth, giving twenty turns per slot. The slots were insulated with paper tubes built up by rolling cartridge paper stuck with shellac varnish round a former. The thermo-junctions for the core were fixed between two fine sheets of condenser paper cut to the same shape as the stampings, the two leads of each junction being carefully brought out so that there was no fear of short circuit. The stampings were clamped together between two wooden rings by means of a bolt and two heavy cross-pieces. The wooden rings were insulated with asbestos from the iron stampings. To prevent the iron bolt from getting hot and so allowing the nut and head to dissipate heat, the air inside was divided off into sections, so as to localise the heat currents by means of discs of paper. The bolt was also shielded by a paper tube. The top and bottom of the core were insulated also with asbestos sheet, cord, and cotton waste. These precautions practically ensured that all the power supplied to the windings would be dissipated in heat from the outside edge surface of the stampings.

For the second part of the work, *i.e.*, the measurement of the heat conductivity across iron and paper, iron strip was wound over the outside of the cylindrical core, each layer being interleaved with paper. The strip was 2·5 × 0·05 cms., and was of best soft steel of bluish colour. The paper was 0·005 cm. thick. Three coils were wound, the thermo-junctions being placed at suitable intervals in the centre one, the two outside rings acting as guard rings. The coils were insulated with asbestos sheet from each other, and the edges of the two outside coils were also insulated from the air with asbestos sheet rings.

Some difficulty was experienced in winding the iron strip on tight, and in one case the coil had to be re-wound owing to a thermo-junction being broken due to tightening. The position and method of carrying away the leads of the thermo-junctions are shown in Fig. 3. In the core A_1, A_2, A_3, A_4 were placed 8 mm. apart. In the strip B_1, B_2, B_3, B_4 were 10 mm. apart, giving a total distance between A_1 and A_4, B_1 and B_4, of 2·4 cms. and 3 cms. respectively. After the strip had been wound on it was found that A_3 and B_3 had become short-circuited or broken. As this could not be remedied without dismantling the whole apparatus, readings were only taken from the six remaining thermo-junctions, these being quite sufficient for calculating the results required.

In the preliminary trials some trouble was experienced with the switches and terminal mountings owing to leakage. To overcome this a change-over switch with a paraffin block as base and mercury cups for contacts was designed (Fig. 4). This was most effective, and is a class of switch recommended for galvanometer work, and very small currents of any description.

TEMPERATURE MEASUREMENT BY THERMO-JUNCTIONS.

The measurement of the temperature by electrical resistance thermometers * (*i.e.*, the temperature calculated from the variation of the

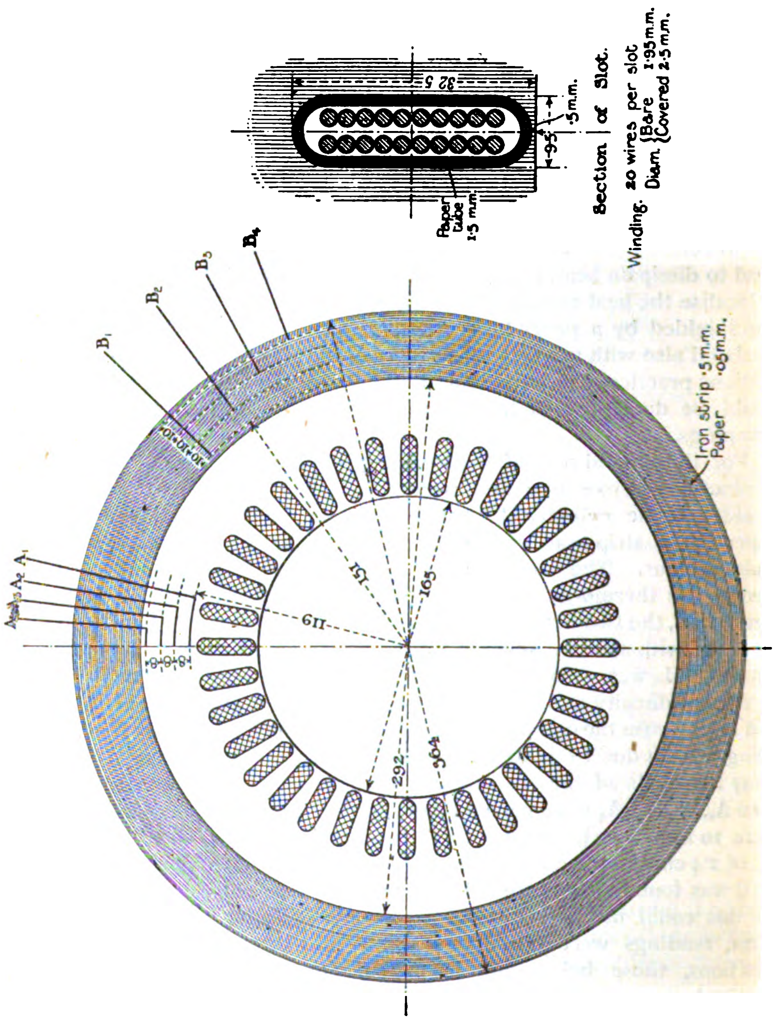


FIG. 3.—Section of Core and Strip, showing Positions of Thermo-junctions (all Dimensions in Millimetres).

resistance of embedded coils), was impracticable in this experiment, owing to the depth of leads to the coils, which would be embedded in

* *Journal, Institution of Electrical Engineers*, vol. 34, p. 613, 1905. Report on Temperature Experiments, National Physical Laboratory (Rayner).

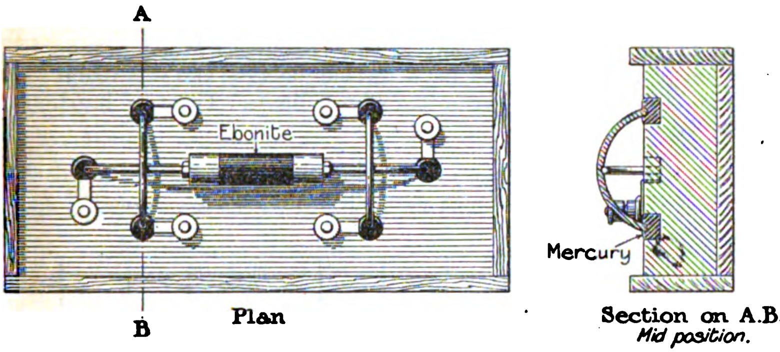


FIG. 4.—Paraffin Wax Base Mercury Contact D.P. Change-over Switch.

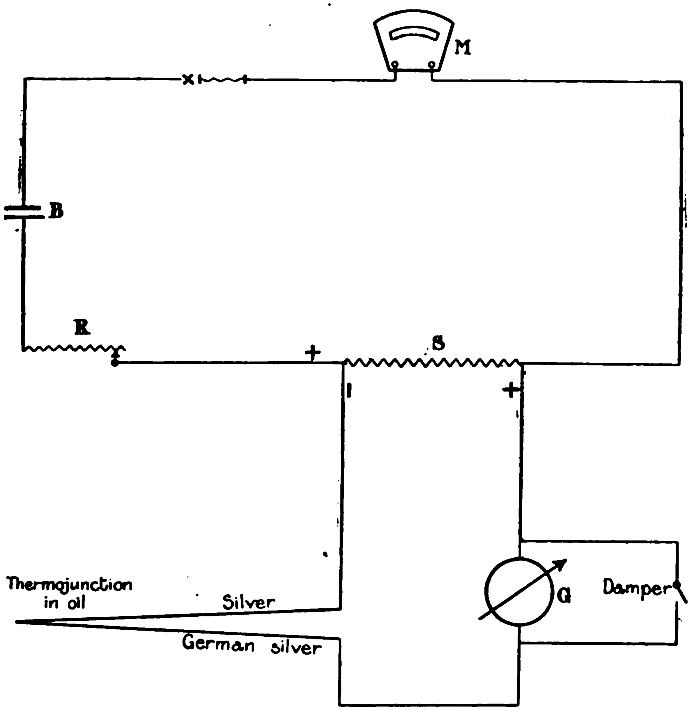


FIG 5.—Diagram of Connections (Thermo-junction Test).

the iron, being also subjected to a variation which would need an assumed temperature drop to be allowed for.

As the temperature was only to vary from 15–80° C., a new combination of metals was tried for a thermo-junction, and was found to give a straight line between the above limits. The two junction wires were—

Positive (+), No. 40 German silver silk covered.
Negative (—), No. 32 pure silver wire bare.

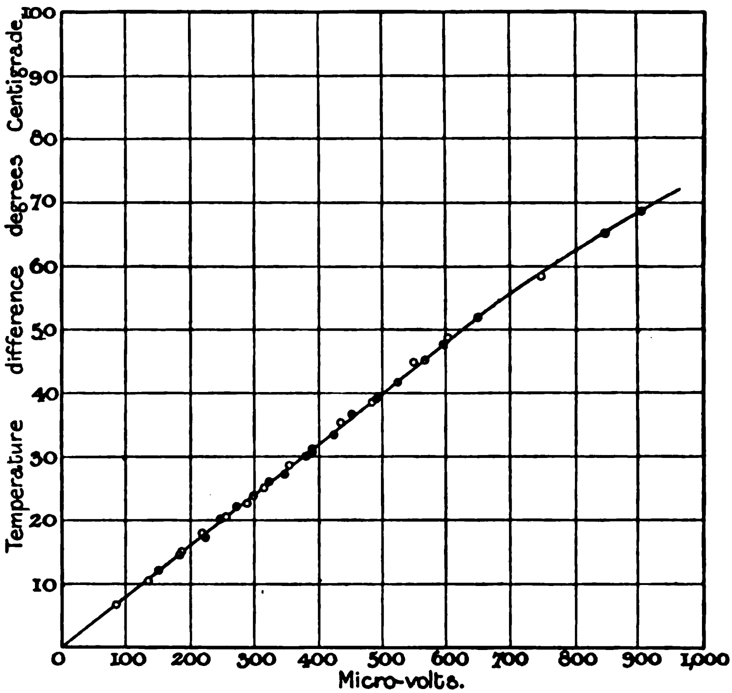


FIG. 6.—Tests on Thermo-junctions A_1 and B_1.

The silver wire was absolutely virgin silver. The actual junction was made by twisting the ends for about 0·3 cm. and dipping in silver solder. The junction was then flattened by light taps.

TEST ON GERMAN SILVER—SILVER THERMO-ELECTRIC COUPLES.

The following are particulars of tests carried out on two of the thermo-junctions used. The thermo-elements were heated in an oil bath, which was kept well stirred when cooling down. The oil was

first heated to about 90° C. and allowed to cool slowly and uniformly. The temperature of the oil, *i.e.*, the hot junction, and the temperature of the cold junction were measured by delicate mercury thermometers. The micro-voltage was measured by a potientiometer method, which is the only accurate way of dealing with micro-volts from thermo-junctions, as then no current is flowing in the junction leads, and therefore in the galvanometer, when balanced. The diagram of connections is shown in Fig. 5.

S. Shunt $\frac{1}{1000}$ *w* resistance.
M. Milli-ammeter, reading 0–1·5 amperes for measuring current through S.
G. Galvanometer (moving coil type).
B. Two-volt accumulator.
R. Variable resistance for regulation.

From these tests (see Tables I. and II.) the micro-voltage per 1° C. rise of temperature was 12·73. Other junctions tested gave points on the line Fig. 6, so this figure was accepted as correct for temperature measurement.

TABLE I.

Test on Thermo-junction A_1.

Temperature of Hot Junction.	Temperature of Cold Junction.	Temperature Difference.	Amperes through $\frac{1}{1000}$ *w* Shunt.	Micro-volts.	Micro-volts per 1° Centigrade.
87·50	19·0	68·50	0·903	904·0	13·20
84·20	19·0	65·20	0·850	850·9	13·10
70·75	19·0	51·75	0·654	654·7	13·14
66·40	19·0	47·40	0·599	599·6	12·80
64·00	19·0	45·00	0·564	564·6	12·55
60·40	19·0	41·40	0·525	525·5	12·70
57·60	19·1	38·50	0·491	491·5	12·75
55·40	19·2	36·20	0·453	453·5	12·55
52·30	19·3	33·00	0·422	422·4	12·85
50·00	19·3	30·70	0·394	394·4	12·85
48·20	18·2	30·00	0·381	381·4	12·70
46·00	18·2	27·80	0·348	348·4	12·55
44·20	18·2	26·00	0·326	326·4	12·55
42·00	18·3	23·70	0·300	300·3	12·65
40·40	18·3	22·10	0·273	273·3	12·40
38·50	18·5	20·00	0·250	250·3	12·55
36·10	18·5	17·60	0·222	222·2	12·60
32·80	18·5	14·30	0·181	181·2	12·65
30·70	18·6	12·10	0·156	156·2	12·90
25·50	18·8	6·70	0·085	85·1	12·70

Table II.

Test on Thermo-junction B_1.

Temperature of Hot Junction.	Temperature of Cold Junction.	Temperature Difference.	Amperes through $\frac{1}{1000}$ *w* Shunt.	Micro-volts.	Micro-volts per 1 Deg. C.
75·6	17·2	58·4	0·748	748·7	12·80
66·0	17·3	48·7	0·603	603·7	12·50
62·1	17·5	44·6	0·550	550·6	12·50
59·5	17·6	41·9	0·525	525·6	12·56
56·3	17·6	38·7	0·482	482·5	12·46
52·7	17·6	35·1	0·436	436·5	12·45
49·2	17·7	31·5	0·389	389·4	12·40
46·1	17·8	28·3	0·357	357·4	12·60
42·9	17·8	25·1	0·319	319·4	12·70
41·0	17·8	22·2	0·287	287·3	12·90
38·1	17·8	20·3	0·260	260·3	12·81
34·9	17·8	17·1	0·219	219·3	12·80
32·9	17·8	15·1	0·190	190·2	12·60
28·1	17·6	10·5	0·138	138·1	13·20

Average micro-volts per 1° C. = 12·73 for Tables I. and II.

Method and Connections for Conductivity Tests.

The experiments were conducted in a room free from draughts and kept as near as possible at a uniform temperature, so that the temperature of the cold junctions would remain constant. The method of carrying out a test was as follows:—

The core was heated up for eighteen hours by passing through the heating coils the current necessary to give the watts required. Connection was then made through one of the junctions, the shunt, and the galvanometer. The galvanometer mirror was then brought to zero by passing a current through the shunt in the opposite direction. This was very finely regulated, giving the current through the shunt, and so the micro-volts for any particular junction, hence the temperature of the iron at the point where that junction was fixed. Care was taken to keep all junctions and connections free from any draughts, and no readings were taken for fifteen minutes after any connection had been touched. This allowed everything to settle down to its normal temperature.

The micro-volts A_1, A_4 were first taken. Then B_1 and B_4, and finally A_2 and B_2. The whole set of readings was repeated three times and the average taken if there was any slight variation. The instruments were all millivolt-ammeters by Siemens & Halske and were used with suitable shunts. The main resistance, carefully calibrated, was made by the same firm, and was of 0·0005 manganin. The regulating resistances were of the ordinary wire type of 42 ohms and 31·5 ohms

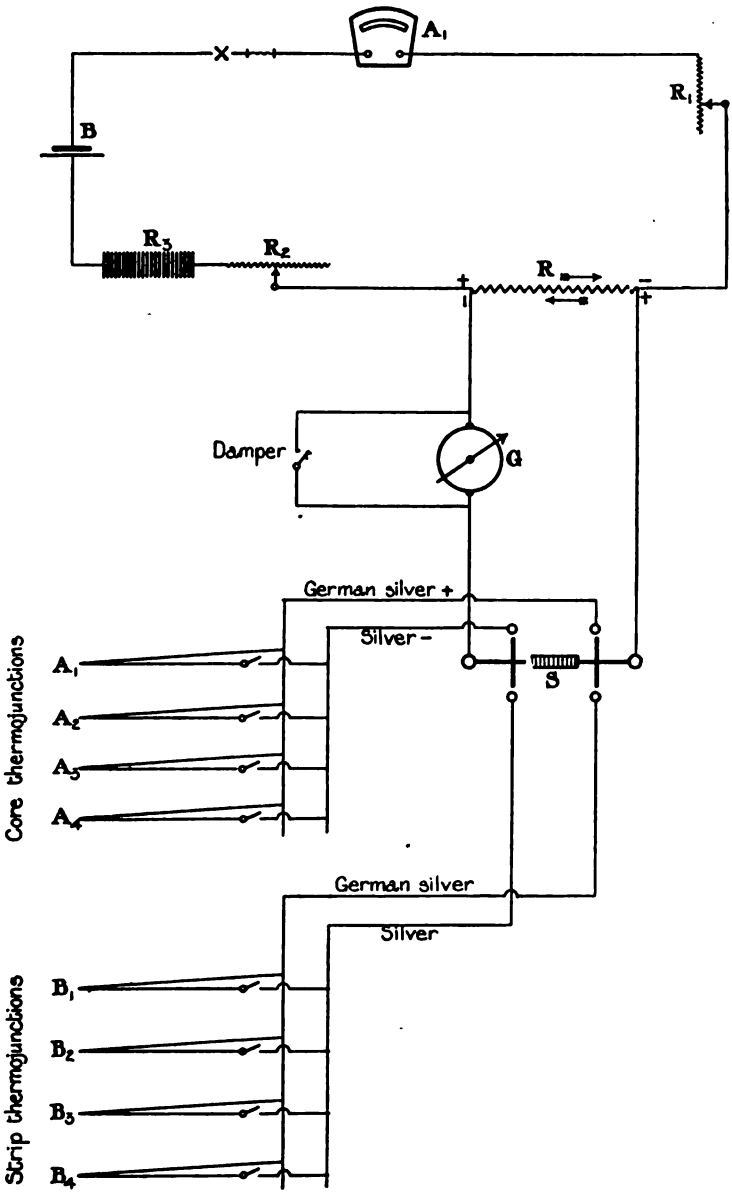


FIG. 7.—Diagram of Connections (Conductivity Tests).

respectively. A carbon resistance, which was always kept tight during the tests, was placed in the circuit for final adjustment. In the diagram of connections (Fig. 7)—

R. 0·0005 ohm manganin shunt.
R_1. Variable wire resistance, 42 ohms.
R_2. Variable wire resistance, 31·5 ohms.
R_3. Carbon rheostat.
A_1. Milli-ammeter shunted to read 0–1·5 amperes.
G. Galvanometer moving coil type.
S. Change-over paraffin block switch.

The instruments used for recording the watts supplied were—

Voltmeter, to read from 0–15 volts.
Ammeter, to read from 0–15 amperes.

Theoretical Calculation of the Conduction Coefficient across Iron and Paper from the Values for Iron (K_1), Paper (K_2), and Air (K_3).

Consider the flow of heat across one lamina of iron and one of paper in a direction at right angles to the surface. If Q be the total amount of heat across both iron and paper, θ the total drop of temperature, and K_b be the conduction coefficient for iron and paper interleaved, d_1 thickness of iron, d_2 thickness of paper, then—

$$Q = \frac{K_b \theta}{d_1 + d_2}$$

If Q_1 and θ_1 are the amount of heat and the temperature drop respectively across the iron lamina, and Q_2 and θ_2 the values for paper, then—

$$Q_1 = \frac{K_1 \theta_1}{d_1} \quad \text{and} \quad Q_2 = \frac{K_2 \theta_2}{d_2}.$$

Therefore, since $Q = Q_1 + Q_2$

$$\therefore \quad \frac{K_b \theta}{d_1 + d_2} = \frac{K_1 \theta_1}{d_1} + \frac{K_2 \theta_2}{d_2},$$

$$\therefore \quad \frac{d_1 + d_2}{K_b} = \frac{d_1 d_2 (\theta_1 + \theta_2)}{K_1 \theta_1 d_2 + K_2 \theta_2 d_1};$$

and—

$$\theta = \theta_1 + \theta_2;$$

$$\therefore \quad \frac{d_1 + d_2}{K_b} = \frac{d_1}{K_1} + \frac{d_2}{K_2} + \left\{ \frac{\frac{K_1}{K_2} \theta_1 d_2^2 + \frac{K_2}{K_1} \theta_2 d_1^2}{K_1 \theta_1 d_2 + K_2 \theta_2 d_1} \right\}$$

$$= \frac{d_1}{K_1} + \frac{d_2}{K_2} + \left\{ \frac{K_1^2 d_2^2 \theta_1 + K_2^2 d_1^2 \theta_2}{K_1^2 K_2 \theta_1 d_2 + K_2^2 K_1 \theta_2 d_1} \right\}$$

The expression in the brackets can be neglected since d_2^2 and K_2^2 are very small quantities; approximately—

$$\therefore \quad \frac{d_1 + d_2}{K_b} = \frac{d_1}{K_1} + \frac{d_2}{K_2}.$$

If we reckon that the iron and paper do not fill the entire distance, but have very small layers of air between (this is actually the case), and if K_3 and d_3 are the conductivity and thickness of air, then similarly—

$$\therefore \quad \frac{d_1 + d_2 + d_3}{K_b} = \frac{d_1}{K_1} + \frac{d_2}{K_2} + \frac{d_3}{K_3} \quad \ldots \ldots \quad (1)$$

In the experiment—

$$d_1 = 0{\cdot}05 \text{ cm.},$$
$$d_2 = 0{\cdot}005 \text{ cm.},$$
$$d_3 = 0{\cdot}00125 \text{ cm.},$$

(d_3 is worked out from the fact that 53 laminæ of iron and paper took up a distance of 3 cms.)

The following are values of K used for iron, paper, and air (C.G.S. Centigrade units):—

K_1 Iron	... ...	0·1528 at 28° C. (Hall).*
K_2 Paper	... ...	0·0003 (Lees). †
K_3 Air	... ...	0·0000479 at 0° C. (Compan). ‡

From the formula for variable conductivity $K = K_0(1 + \alpha t)$ the correct conductivity was calculated for the approximate temperatures. The values of α being—

− 0·000282	... ...	Lorenz for iron. §
+ 0·0013	... ...	Compan for air. §

This gives the value of K_1, K_2, and K_3 at the approximate temperature of the experiment, *i.e.*—

$$K_1 = 0{\cdot}152.$$
$$K_2 = 0{\cdot}0003.$$
$$K_3 = 0{\cdot}0000528.$$

From formula (1) the theoretical value of K_b from these figures is—

$$K_b = 0{\cdot}00138.$$

* E. H. Hall, *Physical Review*, vol. 10, p. 277, 1900.
† Ch. Lees, *Phil. Trans.*, vol. 183A, p. 481, 1892.
‡ P. Compan, *Comptes Rendus*, vol. 133, pp. 120–2, 1901.
§ Landholt-Bornstein, *Physikalisch-Chemische Tabellen.*, Tab. 173.

Constants for the Experiment in Formula for Temperature Difference.—In the formula—

$$\theta = \frac{1}{K} \cdot \frac{Q}{2\pi l} \log_e \left(1 + \frac{x}{r}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

the constants for the two tests for K_a, K_b, respectively, are the values of x and r, and for both l is taken as constant.

Calculation for K_a.—In this case, $x = 2{\cdot}4$ cms. and $r = 11{\cdot}9$ cms. In both cases $l = 20$ cms.

Then—

$$\theta_a = \frac{1}{K_a} \frac{Q}{125{\cdot}67} 0{\cdot}1825.$$

If $P =$ watts lost, then $P = 4{\cdot}18$ Q (Q being in Centigrade C.G.S. units);

$$\therefore \quad K_a = \frac{P}{\theta_a} . 0{\cdot}0003476.$$

Calculation for K_b.—Similarly for K_b—

$$x = 3 \text{ cms.}, r = 15{\cdot}1 \text{ cms.}, l = 20 \text{ cms.},$$

the temperature drop being θ_b.

Then—

$$K_b = \frac{P}{\theta_b} . 0{\cdot}0003439.$$

From Table III. of results the average values obtained are—

$$K_a = 0{\cdot}1405,$$

and

$$K_b = 0{\cdot}00137.$$

This gives an average value of C of 10·1. Hence the ratio of sides of square edge and plate surfaces of iron stampings for equal cooling averages about 10. In actual practice the pressure on the stampings would be slightly more than that used in the experiment. This would lower the value of C by raising K_b a little.

The temperature-drop curves shown (Fig. 8) are for Test 6.

Calculation of Curves for Temperature Rise in Iron Stampings from the Values of K_a and C.

Consider the case of a packet of iron stampings heated internally by (we may suppose) hysteresis and eddy currents.

TABLE III.

Conductivity Tests.

Number of Test.	Date.	Watts to Heat Core = P.	Thermo-junction Micro-volts.						Temperature θ_a.	Temperature θ_b.	Temperature of Cold Junction.	K_a.	K_b.	$\frac{K_a}{K_b}$	C.
			A_1.	A_2.	A_4.	B_1.	B_2.	B_4.							
1	6/2/07	122·8	708·70	707·15	705·000	630·00	432·00	222·0	0·2900	31·98	17·49	0·1470	0·0013210	111·0	10·53
2	6/2/07	140·0	762·00	759·50	758·150	680·00	505·00	236·0	0·3300	34·80	17·51	0·1471	0·0013820	106·0	10·30
3	13/2/07	121·0	711·00	709·00	707·250	647·50	451·00	265·5	0·2940	30·00	16·55	0·1430	0·0013900	103·0	10·16
4	14/2/07	81·1	515·50	514·40	512·900	467·50	342·50	211·5	0·2040	20·10	16·50	0·1376	0·0013820	99·5	9·98
5	19/2/07	68·5	472·50	471·55	470·290	424·50	315·00	206·5	0·1735	17·10	15·55	0·1371	0·0013790	95·6	9·79
6	20/2/07	75·2	500·50	499·35	498·075	447·00	325·75	207·5	0·1900	18·81	15·55	0·1374	0·0013735	100·0	10·00
7	21/2/07	92·8	589·50	588·40	586·525	537·20	400·50	230·7	0·2330	24·05	15·80	0·1380	0·0013250	104·0	10·20
8	22/2/07	99·3	611·70	608·89	606·995	549·25	402·00	237·0	0·2495	24·50	15·46	0·1381	0·0013920	99·2	9·97
9	23/2/07	108·0	650·50	649·90	647·070	579·50	428·53	236·5	0·2690	26·95	15·40	0·1390	0·0013800	100·7	10·03
10	27/2/07	117·5	679·45	676·90	675·940	597·50	448·50	245·5	0·2750	27·60	17·90	0·1410	0·0013920	101·2	10·06

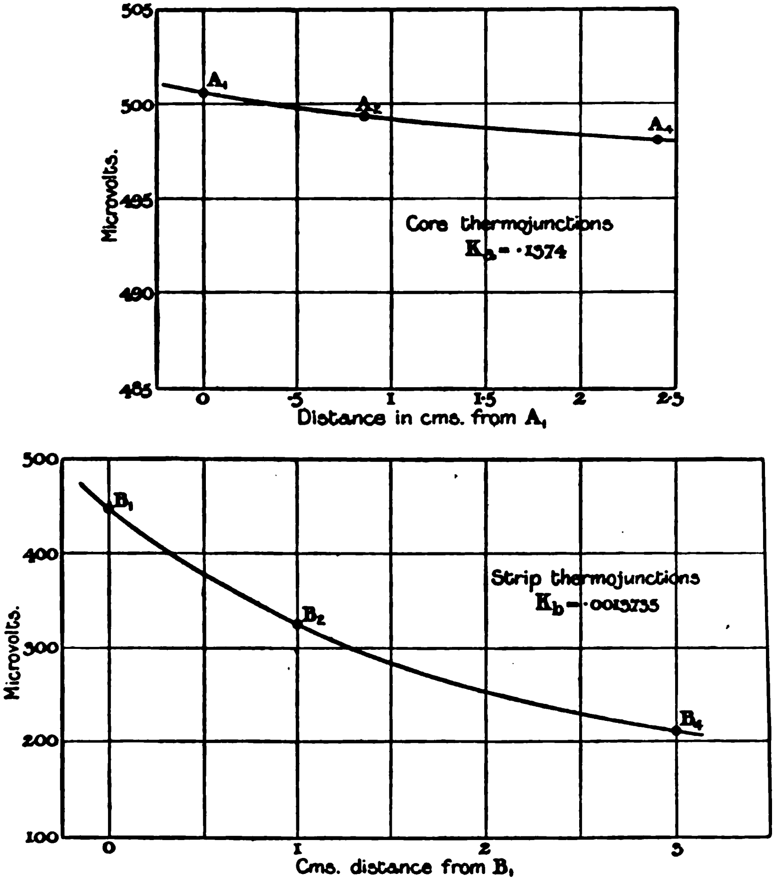


FIG. 8.—Temperature Curves (Test 6).

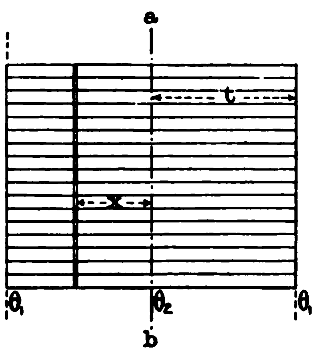


FIG. 9.

Let the temperature at a mid-point distance t from either face be θ_2, and θ_1 be temperature at outside surface, then $\theta = \theta_2 - \theta_1$.

Consider an element distance x from ab having a cross-section of 1 sq. cm. The element has a temperature difference of $d\theta$ and a volume of x c.cms. Then heat transferred between $x = 0$ and $x = x$ is $\frac{d\theta}{dx} \cdot K_a$ after a steady state has been reached.

But the heat transferred equals the heat generated. Also the heat generated in x c.cms. $= f(x) = \lambda x$.

$$\therefore \quad \frac{d\theta}{dx} K_a = \lambda x$$

where λ = calories per cubic centimetre—

$$\therefore \quad \frac{d\theta}{dx} = \frac{\lambda x}{K_a}$$

Integrating—

$$\left[\,\theta\,\right] = \left(\frac{\lambda x^2}{2 K_a}\right)^t_o$$

$$\therefore \quad \theta = \frac{\lambda t^2}{2 K_a}.$$

λ depends upon the induction frequency and thickness of the iron plates. Therefore curves can be drawn for various inductions showing the temperature gradient for any length of heat path, both in a direction parallel to the iron stampings, and in a direction at right angles—that is, across iron and paper since $C = \sqrt{\frac{K_a}{K_b}}$ is known. The value of P_t—*i.e.*, watts lost per kilogramme corresponding to any particular induction—are taken from tables published by Sankey & Sons, Bilston, for their "Stalloy" iron stampings 0·35 mm. at 50 ∿.

B (Lines per Cm.²) (Induction).	P_t (Watts Lost per Kg.).
5,000	0·64
6,000	0·84
7,000	1·08
8,000	1·32
9,000	1·56
10,000	1·87
11,000	2·15

Method of Calculating θ for Values of B.

From the table we can obtain the loss in watts per kg. = to P_t.

This reduced to calories per cubic centimetre $= \lambda = \frac{P_t}{540}$.

$$\therefore \quad \theta = \frac{P_t}{540} \cdot \frac{t^2}{2 K_a}.$$

K_e average value = 0·1405—

$$\therefore \quad \theta = \frac{P_t t^2}{152}.$$

The curves (Fig. 10) are applicable to the flow of heat across iron and paper by using the top scale, which is $\frac{1}{10}$ times the bottom scale, C being taken equal to 10.

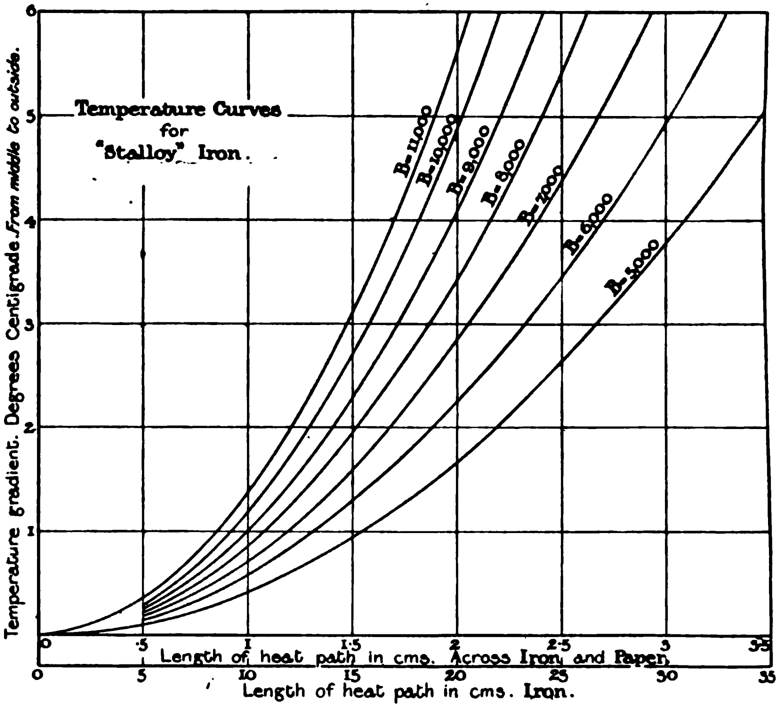


FIG. 10.

APPENDIX.

DETERMINATION OF COOLING CONSTANTS WITH STILL AIR AND MODERATE AIR BLAST, FOR IRON STAMPINGS (edges only).

After the completion of tests as above the iron strip and paper were removed. The core was then heated up to various degrees and the temperature noted by means of the thermo-junctions. Two series of tests were conducted: (1) Still air cooling, (2) moderate

blast cooling. The latter was obtained by sending a blast from an ordinary ventilating fan round the core.

Then if P watts be supplied to heat the core and S be the cooling surface in sq. cms. then the specific cooling surface σ (defined as the number of sq. cms. allowed per watt) $= \frac{S}{P}$.

TABLE IV.

Cooling Coefficients.

Date of Test.	P. Watts to Heat Core.	Temperature Rise T.		σ. Specific Cooling Surface.	C. Still Air.	C. Moderate Air Blast.
		Air Cooled.	Moderate Blast.			
4/3/07	40·95	16·70	5·55	45·00	750	250·0
5/3/07	73·60	26·87	10·10	24·95	670	252·5
6/3/07	59·50	22·60	8·97	30·90	700	277·0
7/3/07	147·10	48·70	27·60	12·45	602	344·0
8/3/07	95·60	34·50	15·10	19·20	665	290·0
19/11/06	49·40	19·68	—	37·20	735	—
21/11/06	118·10	41·60	—	15·55	645	—

The temperature rise can then be expressed approximately by the formula—

$$T = \frac{C}{\sigma}$$

when C is a coefficient. If C were constant a curve showing the variation of temperature with the specific cooling surface would be a hyperbola. The coefficient, however, varies slightly, but for ordinary calculations the value for C, which gives a hyperbola closely approximating to the actual cooling curve, may be taken. The curves (Fig. 11) are drawn from Table IV. The hyperbolas drawn are the nearest to the two curves for cooling by still air and moderate blast. Their equations are:—

$$\text{Still air} \quad \ldots \quad \ldots \quad T = \frac{650}{\sigma}.$$

$$\text{Moderate air blast } T = \frac{300}{\sigma}.$$

It will be noticed that for still air the theoretical hyperbola does not lie very well on the curve obtained by experiment. This is due to the convection currents which are set up in the still air by the

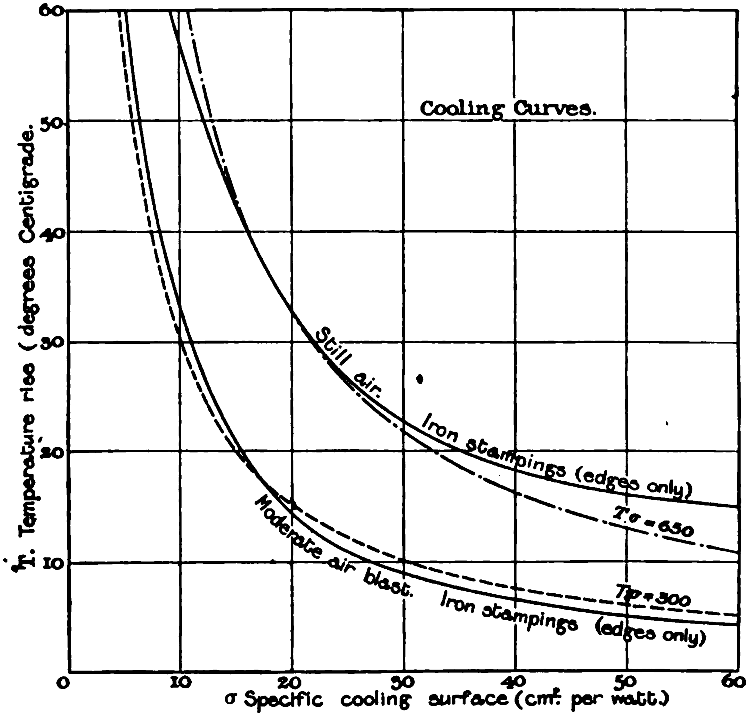


FIG. 11.

dissipation of the heat. In the case of the strong air blast these have no effect. Hence the theoretical hyperbola, $T\sigma = 300$, more closely approximates to the actual curve obtained.

SUMMARY OF RESEARCH.

The main conclusion to be drawn from the values of the heat conductivities of iron insulated stampings found for a heat path (1) in a direction in the same plane as the plate surface of the stampings, and (2) in a direction at right angles to the stampings (across iron and paper), is the importance which must be given to the amount of edge surface of the stampings of a laminated iron core exposed to the cooling medium, either air or oil.

It is seen from Fig. 10, which may be drawn for any iron stampings of which the hysteresis and eddy current losses are known, that the

temperature gradient will be almost a straight line in a direction with the plane of the stampings. Thus the internal temperature of the core is quickly reduced if the method of cooling employed, either air or oil, is used on this edge surface. On the other hand, if the iron laminated core is only cooled on the plate surface, the temperature will rise, comparatively, very rapidly as the core is penetrated across the iron and paper. This rise amounts to as much as 5° C. for a distance of 2 cms. for an induction of 10,000 lines per cm.^{2} at 50∾ in the iron for which the curves are plotted. The effect of this steep temperature gradient in an iron core thus cooled is the increase of the hysteresis loss in the centre portion due to the decrease of the permeability of the iron with the rise of temperature. Also for any iron rectangular laminated core, which is heated internally, from the values of $C = \sqrt{\frac{K_a}{K_b}}$ (the ratio of the linear dimensions of the sides of the core, *i.e.* edge side to plate side) for the most efficient cooling must equal 1 : 10. This ratio is somewhat higher than is used at present in common practice, the figure averaging about 1 : 6 for small armature cores and transformers and 1 : 7·5 for large cores.

Further, dealing with the question of the ventilation of direct-current armatures, it will be noticed that the ventilating ducts between the packets of iron stampings are not so useful on account of the extra plate surface exposed to the cooling-draught induced, but act more as an outlet for the air inside the centre space of the armature, which has been heated in cooling the inside edge surface and so the internal parts of the core. This also applies to the ducts in large transformer cores cooled by a direct air blast.

In connection with the few tests for cooling coefficients for iron cores, these formulæ may be used for the temperature rise (surface) of the iron core of a transformer, where the effective plate surface for cooling, in comparison with the edge surface, can be neglected. Values of the coefficient may be approximated for different rates of air cooling, but further work is necessary on this part of the work before any definite conclusion can be inferred.

In conclusion, I have to thank Professor Kapp, of the Electrical Department of the University of Birmingham, at whose instigation the research was carried out, for the facilities afforded me.

Discussion.

Dr. W. E. Sumpner: The apparatus used is very well designed for its purpose, the experiments have been well carried out, and the results obtained should prove valuable. It is stated in the paper that C^2 is the ratio of conductivity along the stampings to the conductivity across the stampings. This formula is quite true if the temperature gradient is the same in each direction. Inside the core the heat flow tends to be quickest in the direction of the greatest conductivity, but also depends on the temperature gradient determined by the thickness of Dr. Sumpner.

Dr. Sumpner. the iron and the outside temperature. On the outside surface, unit areas, if at the same temperature, dissipate equally quickly whether edge-on or face-on to the air. The temperature excess of the outside surface over the air is therefore different for different portions of the surface, so that the temperature gradient will be least in the direction of greatest conductivity. The ratio referred to as C^2 depends consequently upon shape and dimensions as well as on conductivity. The discrepancy referred to on the last page of the paper can perhaps be explained in this way.

Mr. Forster. Mr. A. LINDSAY FORSTER: As the result of considerable experience with similar heating work I have doubts whether the heat does actually come out of the stampings as assumed in the paper. Do the wood ends used in connection with the testing apparatus form a sufficient insulation against heat leakage? I know an example of design in which the cooling of the armature iron is effected solely by edge conductivity.

Dr. Morris. Dr. D. K. MORRIS: It is remarkable how long designers have been content to do without such important information as that contained in the paper. Although the paper, and particularly the mathematics, may be criticised, still the results obtained are broadly correct, and should prove useful to the designer. I would not agree with the statement on page 601 that the iron loss increases with the rise of temperature. Losses due to both hysteresis and eddy currents decrease with increase of temperature.

Dr. Kapp. Dr. G. KAPP: There are a few remarks I should like to make. First, as to how the investigation came about. Some time ago at my suggestion the Committee of the German Institution of Engineers voted a sum of money to Professor Linde to carry out experiments on the subject. In these experiments, which were done on packets of plates and paper, one face was heated with hot water and the other cooled with melting ice. The work has been in progress two years, and as a preliminary result Professor Linde has given the value of the constant C^2 as 80. But this result has not been confirmed, so I suggested to Mr. Barlow that he should investigate the subject, using an apparatus which would approach more nearly to standard conditions.

Referring to the paper itself, the equation given on page 612 is correct, but the process of deriving it is wrong. A question raised by the paper was, Is the flat surface of a packet of stampings of any real value? In order to get a clearer idea I have worked out the value of the combined or total surface in terms of the edge surface for different shapes of packets:—

Ratio $\dfrac{\text{flat surface}}{\text{edge surface}}$ =	1	2	3	4	5	7	10.
Factor expressing value of total surface ... =	1·01	1·04	1·09	1·16	1·25	1·5	2.

Professor Kapp.

In practice it is customary to make packets about 7 times as long as thick. Theoretically this should be 10, but such a ratio would lead to waste of room, so that 7 times is not a bad compromise.

In reference to Mr. Forster's remarks, it is quite possible that the wood ends used with the apparatus do not entirely prevent the flow of heat, but in any case this is of no importance on the medium line. I have no doubt that the experiments are substantially correct. Mr. Barlow's figure for C^2 is 100, as compared with Professor Linde's 80. The difference is probably due to the amount of end compression on the stampings.

Mr. Barlow.

Mr. T. M. BARLOW (*in reply*): I agree with Dr. Sumpner that the discrepancy of the experimental curve from the theoretical hyperbola could be explained in the way he suggests.

In view of the criticism that there is a somewhat illogical reasoning in the theoretical calculation of the conduction coefficient across iron, paper, and air laminæ, perhaps the following method may make the solution clear:—

If Q is the total quantity of heat which flows across one lamina of each material in contact with each other, and d_1, d_2, d_3 are the thicknesses of the lamina, the conduction coefficients being K_1, K_2, K_3, and temperature drops θ_1, θ_2, θ_3 respectively, then—

$$Q = \frac{K_1 \theta_1}{d_1} = \frac{K_2 \theta_2}{d_2} = \frac{K_3 \theta_3}{d_3} = \dots$$

But—

$$\theta \text{ the total temperature drop} = \theta_1 + \theta_2 + \theta_3 + \dots$$

and—

$$\theta_1 = \frac{d_1 Q}{K_1}, \quad \theta_2 = \frac{d_2 Q}{K_2} \dots$$

$$\therefore \frac{\theta}{Q} = \Sigma \left(\frac{d_1}{K_1} + \frac{d_2}{K_2} + \frac{d_3}{K_3} + \dots \right).$$

But $\frac{\theta}{Q} = \frac{d}{K_b}$ where $d = d_1 + d_2 + d_3 \dots$ total thickness and K_b = conductivity of the packet considered as a whole.

$$\therefore \frac{d_1 + d_2 + d_3 \dots}{K_b} = \Sigma \left(\frac{d_1}{K_1} + \frac{d_2}{K_2} + \frac{d_3}{K_3} + \dots \right).$$

In reply to Mr. Lindsay Forster, the apparatus was insulated at top and bottom with asbestos in addition to the wood. The heat radiated by these ends was negligible. This was proved by the extremely small temperature difference between the iron bolt, clamping the core, and the air.

The importance of edge cooling of the iron, this being the sole method employed in one case referred to in the discussion, is further emphasised in the use of ventilating ducts, such as in the case of induction motors.

Mr. Barlow.

If the ducts in the stator and rotor are exactly opposite there is practically no flow of air over the outside edges of the rotor and the inside edges of the stator, the cooling being mostly on the plate surface, which, as shown, is inefficient. If the ducts are placed alternately in the stator and rotor the air drawn through the rotor passes over the inside edges of the stator before passing out through stator ducts. This is the case in a certain standard line of induction motors in which the stator has no ducts, the air from the rotor ventilating ducts passing out horizontally over the inside stator edges.

I have to thank Professor Kapp for the idea of a factor showing a comparative value of the total cooling surface for different ratios of edge and plate surface.

The difference in the ratio of the conductivities, as found by Professor Linde and myself, is due to the experimental difficulty of making the pressure on the laminæ of iron and paper in the wound strip the same as the pressure on the stampings of which the core was built up.

DIRECT-CURRENT TURBO-GENERATORS.

By WILFRED HOULT, M.Eng., B.Sc., Associate Member.

(Paper received from the MANCHESTER LOCAL SECTION, *January* 29, *and read at Manchester, February* 18, 1908.)

The subject of direct-current turbo-generators design and working has lately been brought into prominence by the Technical Press and also by a paper by R. Pohl, lately read before this Institution, on which a very interesting discussion arose.

Messrs. Parsons, of Newcastle, originated the turbo-generator as well as the turbine, and up to about six years ago were the only manufacturers who had experience of them. The Westinghouse Company of America then followed, and latterly other firms, notably Siemens Brothers, Brown-Boveri, the British Westinghouse, and the British Thomson Houston have entered the market for this class of work. Owing to the high peripheral speed of the armature and commutator great difficulties have arisen in the mechanical and electrical design, requiring much experimental work and careful attention to each detail.

Parsons' first machine had an output of about 7½ k.w. at a speed of 18,000 revs. per minute, the diameter of the armature being 3 in. The winding was all on the surface and was kept in place by binding wire covering the whole length of the armature. Each commutator bar was made up of several cast bronze segments which were fitted on a sleeve and kept in position by steel dove-tailing rings so that each individual segment was relieved from radial stresses. This machine proved very satisfactory, but on building larger ones difficulties were encountered which to a great extent retarded the progress of the turbine.

Until the last few years the position of the brushes had to be adjusted for variations of load. Messrs. Parsons have an automatic brush control which is actuated by the initial pressure of steam in the turbine. The pressure of the steam is proportional to the load on the turbine and acts on a piston which is loaded by a spring on the opposite side. This control gear is working very successfully on many sets, even on traction loads. It has, however, a tendency to lag, and also in the event of the turbine going to atmosphere (thus requiring a higher initial steam pressure) the brushes move forward, whereas the load has most probably decreased.

This arrangement is not now fitted, as special windings are used to obtain a fixed brush position with varying loads.

The methods mostly used may be put under three headings :—

1. By compensating windings.
2. By commutation poles.
3. By a combination of compensating windings and commutation poles.

In the first method windings which are in series with the armature are placed in slots similar to those on an alternating-current machine, so that at all loads their ampere-turns are equal but of opposite direction to the armature ampere-turns, thus balancing all armature reaction. Sparking due to distortion is thus prevented, but that due to the current reversal in the short-circuited coil is left, to overcome which either the brushes are given a lead or the machine is over compensated. This method, which in principle was first suggested by Fischer-Hinnen, was patented in 1893 by Professor H. J. Ryan, but was allowed to lapse for the reason that manufacturers, being able to build machines to give every satisfaction with natural commutation, would not take it up. A patent for a similar winding was taken out in 1903 by Messrs. Parsons and Stoney.

In the second method auxiliary poles placed midway between the main shunt poles are used, through the winding of which the main current or a portion of the main current passes. The object is to give a magnetic field so as to produce in the coil undergoing short circuit by the brush an E.M.F. acting in opposition to its reactance voltage, and of such a value as first to reduce to zero the current present in the coil, and then, while still in opposition to its reactance voltage, to induce a current in the opposite direction, which as the coil leaves the position of short circuit shall have become equal in strength to the current of which it then becomes a part. To obtain such an ideal reversal the field under the commutation pole should be uniform. If the pole-pieces are made uniform, however, the armature reaction distorts this field so that it is weaker at one edge of the pole-piece than at the other, but in practice it is found that uniform pole-pieces are quite good enough, and that alterations to the shape of them are a refinement which is hardly necessary.

In the third method both compensating windings and commutation poles are used, thus giving no distortion to the main field but giving a reversing field to overcome the E.M.F. in the coil undergoing short circuit by the brush.

In Fig. 1 curves are drawn showing the flux distribution due to the various windings.

Curve A shows the flux distribution of the main shunt field and the commutation field separately excited with normal full-load current.

Curve B shows the flux distribution due to the current in the armature.

Curve C shows the resultant flux distribution of curves A and B.

Curve D shows the flux distribution due to the compensating wind-

ing, which for the purposes of making the diagram clearer has been assumed to exactly neutralise that due to armature reaction.

Curve B being neutralised by curve D, the resultant field is that shown by curve A, except that the commutation pole flux is much smaller, as in this case ampere-turns are not required on the commutation pole for the armature ampere-turns.

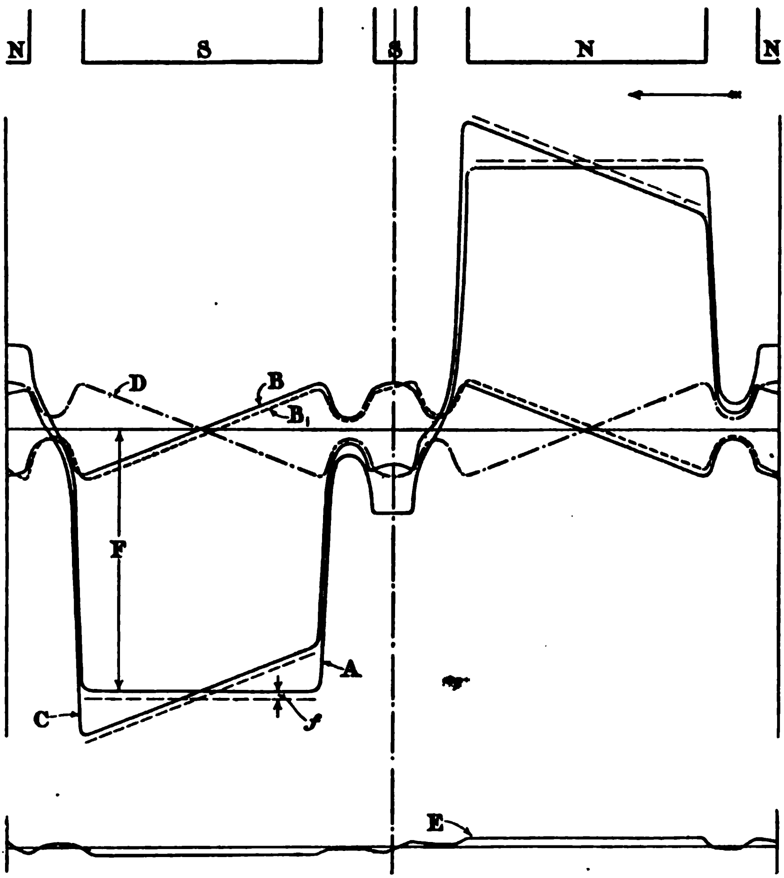


FIG. 1.

Figs. 2, 3, and 4 are flux distribution curves of a 700-k.w. 600-volt generator running at 1,500 revs. per minute, and fitted with commutation poles. The curves were taken by a Duddell high frequency oscillograph, which was connected to an armature coil by brushes pressing on two of the steel binding rings on the commutator which in turn were connected to two adjacent commutator segments.

Fig. 2 shows the shunt field excitation only.

Fig. 3 shows the shunt field excitation with the commutation coils separately excited with 300 amperes.

Fig. 4 shows the flux distribution with the machine at full load, the current through the commutation coils being 950 amperes.

They show plainly the effect of the armature reaction on both the main field flux and the commutation flux. It will be noticed that the commutation flux does not distort the main field at no load.

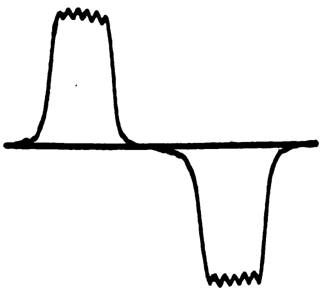


FIG. 2.

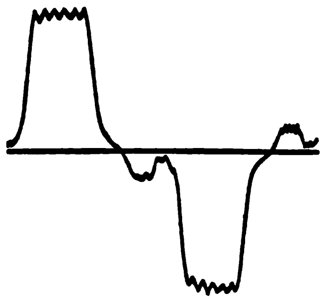


FIG. 3.

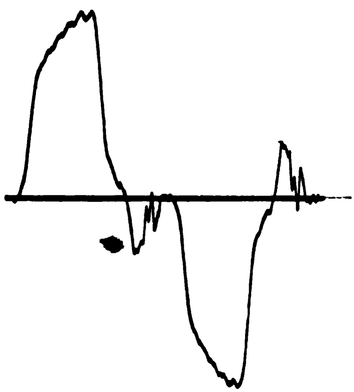


FIG. 4.

Fig. 5 shows the field-magnet system of a Siemens 750-k.w. 420–550-volt direct-current turbo-generator.

Fig. 6 is a photograph showing the field-magnet system of a Westinghouse 1,000-k.w. 600-volt set.

Fig. 7 shows that of a Brown-Boveri 600-k.w. 550-volt set.

Fig. 8 is a diagram showing the general scheme of connections for a generator with compensating windings and commutation poles.

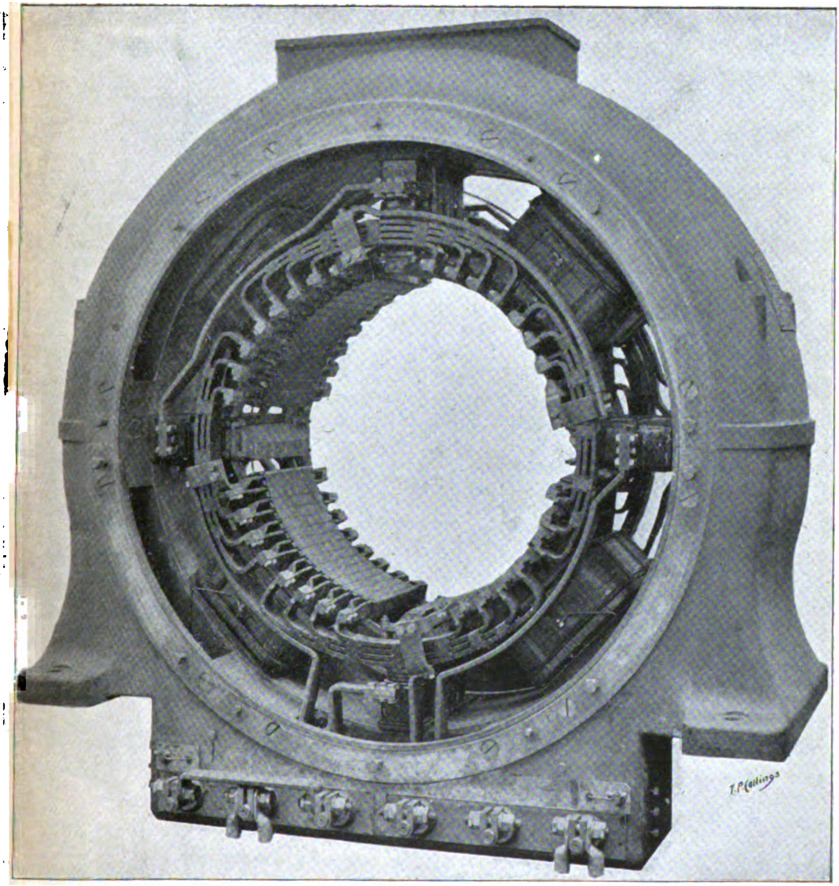


FIG. 5.

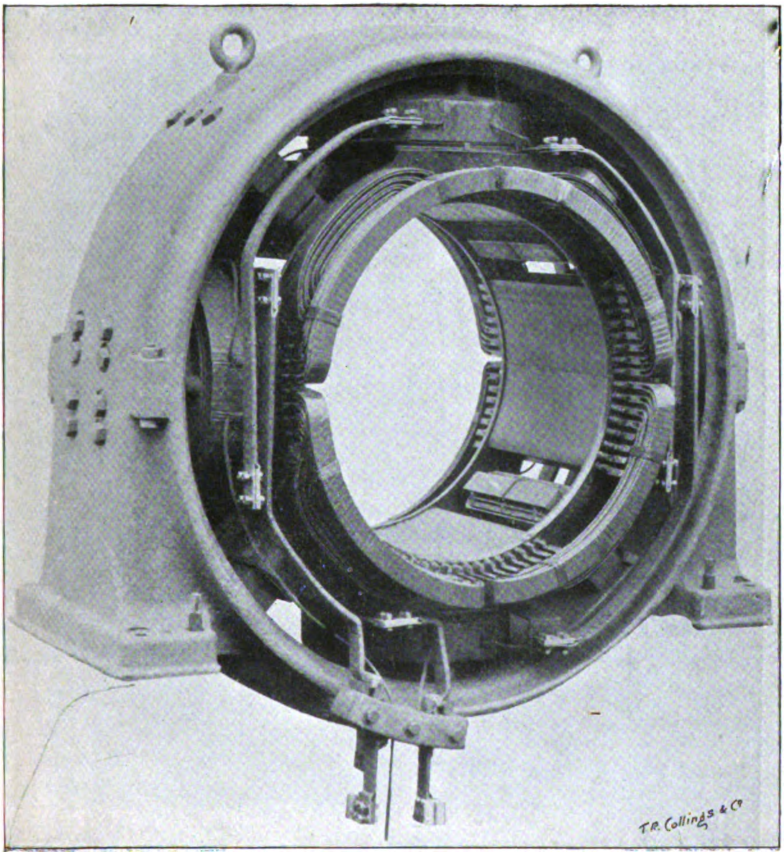


FIG. 6.

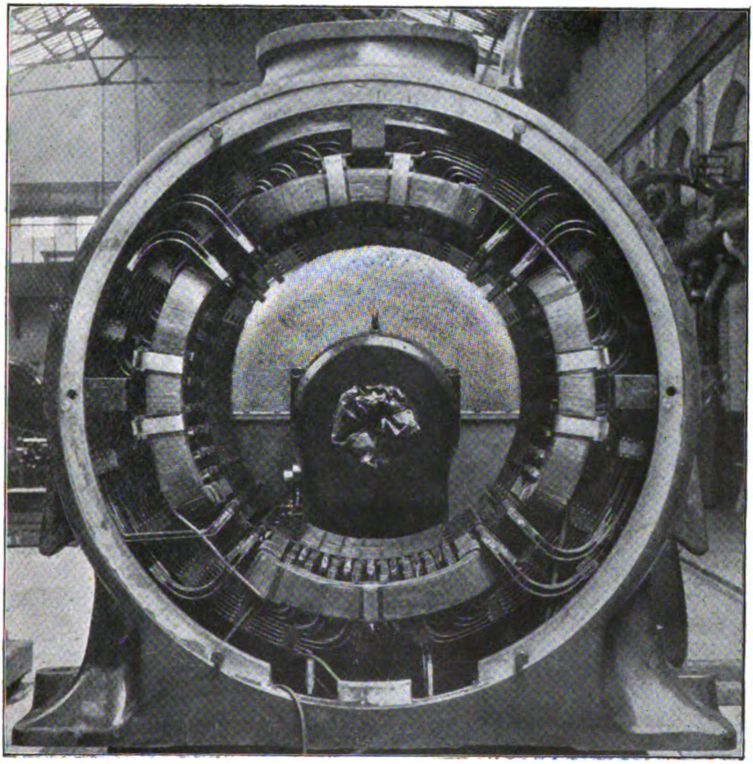


FIG. 7.

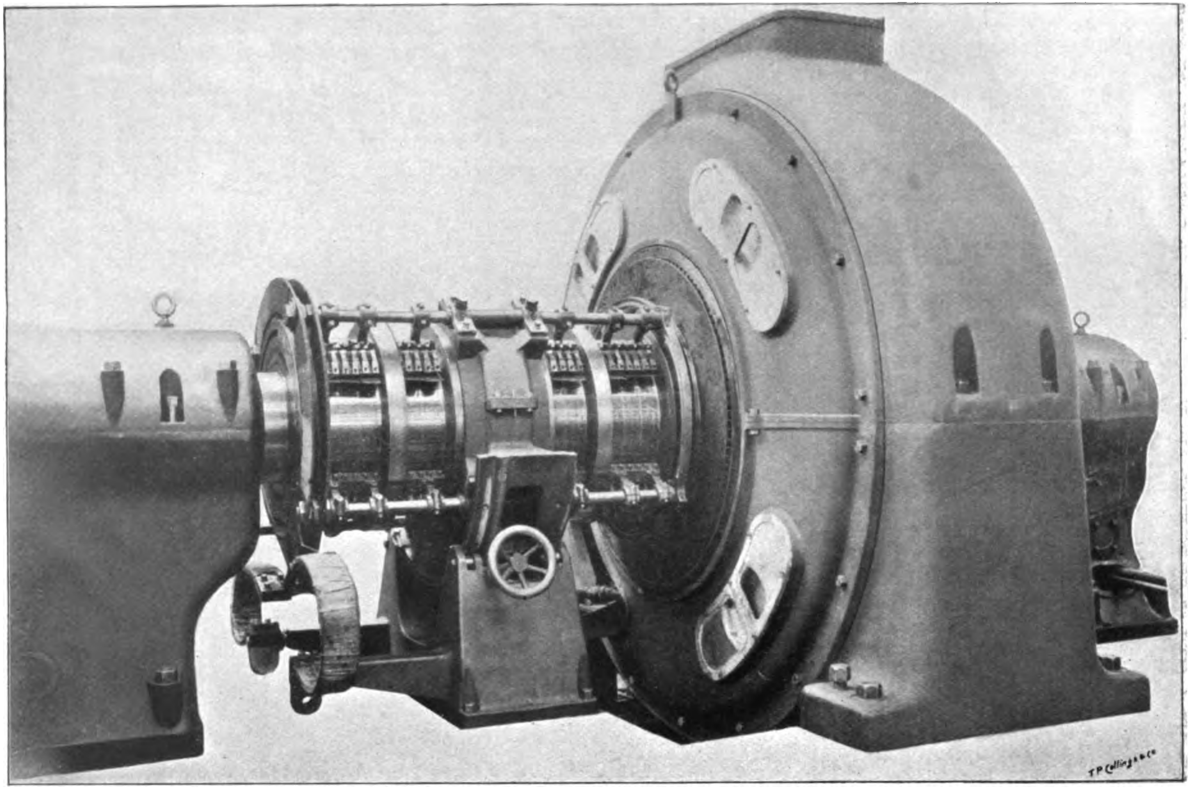


FIG. 9.

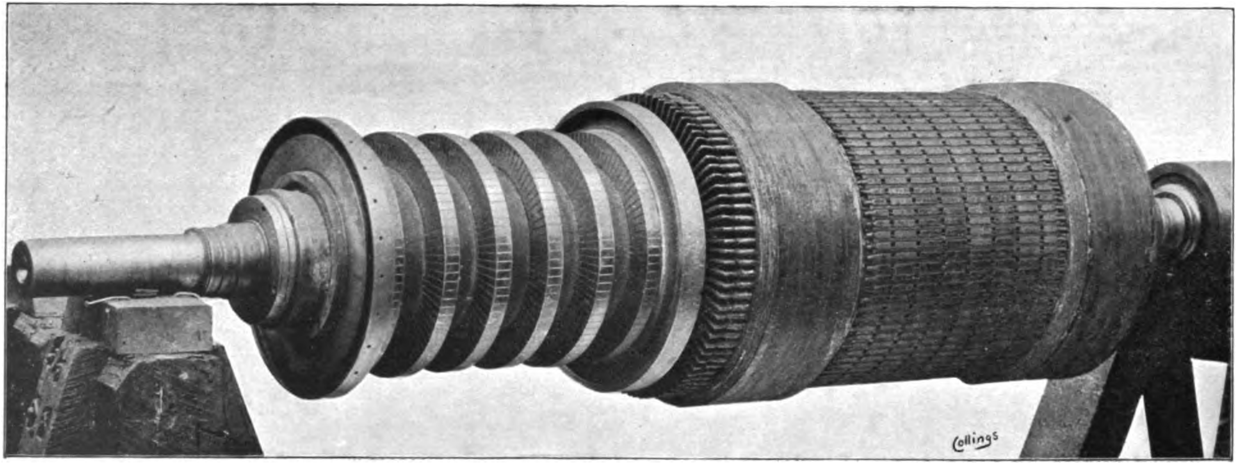


FIG. 11.

The main drawback of machines fitted with commutation poles alone is that the main field is distorted by the armature reaction, so that the maximum voltage per segment is considerably increased on approaching full load, thus giving a greater tendency for "flashing over" with dust on the commutator. Absolutely sparkless commutation can be obtained from no load to large overloads without any movement of the brushes.

Machines fitted with commutation poles alone can be built about 10 per cent. to 15 per. cent. cheaper than those fitted with compensating windings, and for machines in which the maximum voltage per

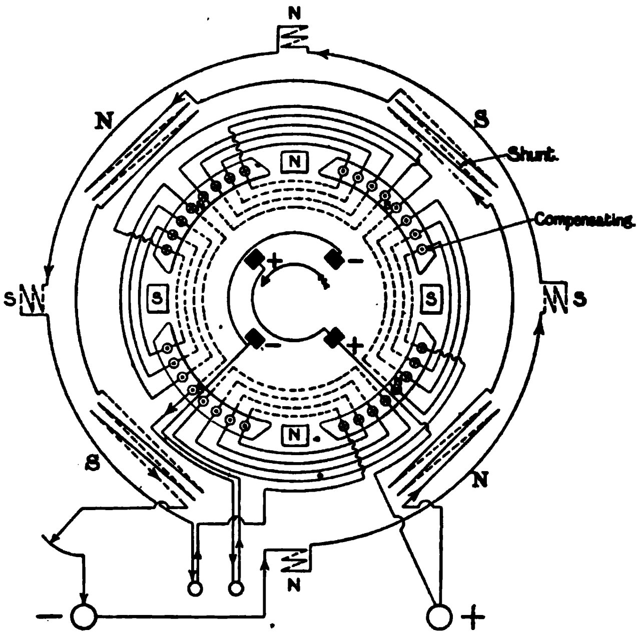


FIG. 8.

segment can be kept below about 30, and in some cases below 45, they are perfectly satisfactory. It is necessary that the iron of the commutation pole should be worked on the straight part of the **B.H.** curve, and consequently at a very low density, so that the reversing field increases in the same proportion as the armature current increases. The correct reversing field can be obtained by adjusting the air-gap of the commutation pole or by means of a diverting resistance in parallel with the commutation pole windings.

The former method is that now mostly in use especially with machines fitted with compensating windings. The air-gap in this case should be designed so large that the correct commutation field

can positively not be obtained. When the machine is on test the poles, or preferably the pole-shoes, can be slacked back and iron plates inserted under, say, the N poles. If this is not satisfactory the plates can then be put under the S poles, and so on until sparkless commutation and mimimum heating of the commutator is obtained. It is quite possible to have perfect commutation and still have considerable heating due to currents in the short-circuited coil.

With large machines fitted with commutation poles only, the correct reversing field is often obtained by means of an adjustable diverter. The machine can then be run up on test, the load gradually increased, and the correct amount of resistance obtained in the minimum of time, without having to stop the machine and alter air-gaps.

If new brushes are put on the machine of a quality different from those with which the diverter was set, it is quite possible that to regain sparkless commutation the resistance will require to be altered, which can readily be done if the diverter is adjustable. Although it can be said that the brushes of to-day are very satisfactory, yet there is every likelihood of still further improvements being made of which the engineer with a turbo-generator in his station will wish to take advantage, and it would not be a fair test for a brush to be fitted to a machine which had been set with another type.

If an ordinary non-inductive resistance is used in parallel with commutation coils, the latter, owing to their being highly inductive, will take less than the normal amount of current on a sudden increase in the load, thus causing momentarily very bad conditions for commutation, which, if the variation is large as on a "short," would cause the machine to flash over on the commutator.

To overcome this the diverter winding should be placed round an iron core with an adjustable air-gap, the air-gap being altered to suit the number of turns through which the current is flowing so that the time constant of the diverter circuit equals that of the commutation system. The diverter can also be arranged so that a larger current than the normal will flow momentarily through the commutation coils so as to hurry up the magnetisation of the commutation poles.

An ordinary shunt-wound generator on increase of load has for the same position of field rheostat a considerable drop in voltage owing to the demagnetising action of the armature on the main field, but with a turbo-generator having compensating windings the field is hardly weakened at all, consequently, unless special methods are used such as placing back series turns on the main poles, the regulation is extremely good.

If, instead of having the brushes in the neutral position, they are moved backward a compounding effect is produced. In Fig. 1 curve B_1 shows the flux distribution due to the current in the armature when the brush is moved backward from its neutral position, so that the armature current has now a compounding effect, the amount (f) being the difference between the ordinates of curves B and B_1, which is

shown in curve E. The field under the main pole is then increased from F to F + f.

The smaller the air-gap the greater will be the number of lines that will be forced through, and consequently the greater the compounding obtained.

In the same way, by assuming the brush position to be moved forward, it can be seen that an under-compounding effect is produced.

If the generator is to work on a circuit by itself a compounding effect is an advantage, but if, as is most usual, it has to work in parallel with other machines, it would take the peaks on a fluctuating load so that it has to withstand much heavier duties than the ordinary shunt machine. Difficulties have also been experienced with compensated machines working in parallel on traction loads, and Messrs. Parsons brought out a patent in 1900 "to improve the stability" by bringing the equalising connection to tappings on the compensated winding instead of the junction of the compensating winding and the compound winding.

If the air-gap is very small it is quite possible that a machine may be very much over-compounded by a very slight backward movement of the brushes, and if metal ones are used trouble can arise when on load owing to their necessary adjustment.

It is not advisable to give a turbo-generator a backward lead, as if the reverse current circuit breaker should not act when taking the machine off the mains there is a danger of the machine motoring and running to a dangerous speed owing to the weakening of the field.

Up to about the last two years all direct-current turbo-generators have been fitted with either metal or a combination of metal and carbon brushes. It has, in fact, often been stated that it was a practical impossibility for high-speed generators to run with carbon brushes alone, but it is now becoming generally recognised that they will supersede the metal brushes in the same way as they have done on slow-speed machines.

The prejudice created against carbon brushes has been caused to a large extent through their being used on commutators that were not suitable both from the construction and the heating point of view. It is essential for their successful operation that the brushes be kept cool, and consequently the commutator should be specially ventilated and be of a thoroughly sound mechanical construction.

The Morgan Crucible Company, Limited, have helped the manufacturers considerably in their endeavour to obtain suitable brushes. They have also brought out a special pneumatic brush holder in which the brushes receive their tension by means of compressed air. This brushgear with Morganite brushes has been fitted to a 200-k.w. generator running at a speed of 3,000 revs. per minute at their works, and has been in operation for about twelve months with great success, the surface of the commutator being excellent and the wear very slight.

The author has made many experiments with carbon, metal, and graphite brushes, and although the latter type of brush is very satisfactory on slow-speed machines, he considers that they are as yet hardly mechanically strong enough for the high speeds of turbo-generators. This type of brush is usually made by compressing the graphite in moulds, thus forming a series of layers which easily break away at the leaving edge. If these brushes are run at a high density and become heated they swell in their holders and require constant inspection.

In the author's opinion, the only graphite brush of that type capable of being used on a high-speed machine is one in which the layers are in the direction of the running of the commutator, and consequently having the cross resistance in an axial direction instead of tangential. The brushes then do not break away so easily, the commutating qualities are about as good, and advantage of the low friction coefficient is still obtained. Other graphite brushes that were tried, although of a soft nature, ground fine particles of copper from off the commutator that were quite perceptible to the eye.

Endruweit brushes with carbon brushes slightly in advance of them are used to a considerable extent on turbo-generators. These brushes are made by wrapping layers of copper foil and paper together, the whole being then baked so as to form the paper into carbon.

The Boudreaux brush, which is made of a special metal, has very good commutating qualities, but even with a very light pressure they tend to cut the commutator.

Parsons' use a brush made of brass wire which usually works on a corrugated commutator. The corrugations increase the area for radiation and decrease the current density at the surface of the brush. Some of their machines are fitted with all the positive brushes in one cell and all the negative in the other.

Fig. 9 shows the brushgear on a Siemens standard machine running with carbon brushes. Sets ranging from 400 k.w. to 1,500 k.w. at speeds from 1,500 to 1,800 revs. per minute have been fitted with this gear with every success.

As it is essential for the proper running of carbon brushes that the brushes and commutator should be kept cool, specially ventilated commutators are fitted. The construction is shown in Fig. 10, from which the ventilating arrangements can be seen.

The commutator bars are provided with tunnels along their entire length, through which the air is drawn axially from each end by the action of a radial fan placed between the two commutator units. The blades of this fan also form the electrical connection between the two commutators. The inner unit is cooled by air which passes through channels in the spider close to the shaft, so that the air has no connection with the ventilating ducts of the armature core, the outer one receiving its air directly from the engine-room. The sides of the shrink rings are protected by specially treated wood plates, which are held in position by a wedge-shaped metal ring, thus ensuring that no

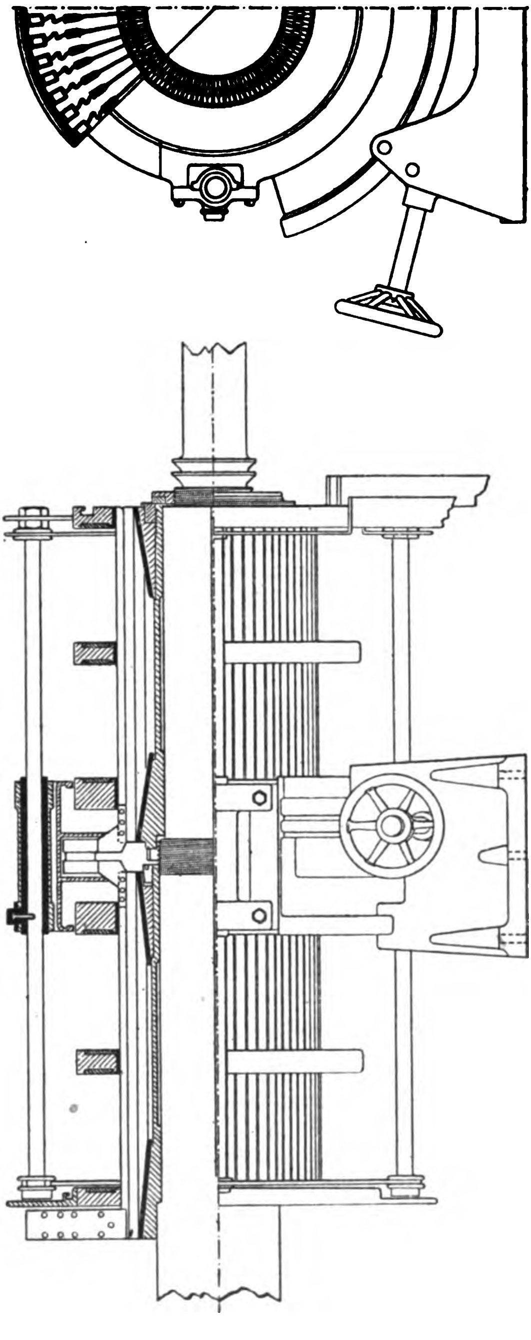


Fig. 10.

connection can be made from the commutator to the shrink rings by dust on the commutator or by flashes from the brushes.

Fig. 11 is a photograph of an armature of a Westinghouse machine having an output of 250 k.w. at 3,000 revs. per minute at a pressure of 500 volts. The armature is provided with a radial-type commutator, on which it is claimed that carbon brushes can be used at a speed of 3,000 revs. per minute without trouble from chattering, that there are no exposed shrink rings to increase liability to flash over, and that the commutator is kept very cool owing to the large radiating surface which, combined with the good commutating conditions with carbon brushes, enables the machine to run well without special attention or fine adjustment of the brushes.

The great advantage of carbon brushes is that when once adjusted they require no attention when the machine is running, and very seldom require attention when the machine is stopped. They also are not sensitive as regards commutation, so that if the commutation poles of the machine are not on the straight part of the saturation curve on overloads, sparkless commutation can still be obtained. It would appear that owing to the much lower current density at which carbon brushes can be worked, that the commutator would require to be proportionately longer, but as the arc of contact can be much greater, owing to the higher surface contact resistance and cross resistance of the brush, the commutator does not of necessity become excessively long.

The drawback to metal brushes and combinations of metal and carbon brushes is, that although perfect commutation can be obtained, they require attention when on load. They may even run for three days without much attention, but although no sparking can be seen by the attendant, considerable sparking takes place under the brush owing to the arc which was originally covered being increased, and this sparking often damages the surface of the commutator. Also owing to their low resistance large currents circulate through the short-circuited coil, especially if the brushes owing to their adjustments are not in their correct position. These currents work either with or against the main field and the commutation field, giving a magnetising or demagnetising effect so that if the brushes chatter at all, parallel working with other machines can become very difficult unless the generator has a separate excitation.

Messrs. Brown-Boveri, in their turbo-dynamos, have departed from the ordinary construction of magnet frames, the field system of their machines being built up of a number of soft iron core plates in the slots of which the windings are fitted. This gives a continuous inner cylindrical surface which has the advantage of decreasing windage resistance and allowing silent running.

A striking feature of their machines is that they are fitted, except when a steady source of supply is available, with a direct-coupled exciter, for the purpose of preventing the machine reversing its polarity. Owing to the type of construction the space for the shunt field coils is rather limited, so that exciting at a low voltage allows

slightly more room for copper. On sudden overloads, owing to the turbine dropping in speed, the voltage of the exciter is lessened proportionately, and consequently the generator has not such a tendency to take up all the load if running in parallel with other machines. A separate exciter is of considerable advantage with this type of machine as their fields have not the same tendency to "build" as those with solid field magnets.

As regards the construction of direct-current turbo-generators it is of course essential that in every part both the material used and the workmanship must be of the highest class. Special care must be taken to have all the materials homogeneous and the windings and insulation kept perfectly rigid so that there may be no possibility of their moving either from centrifugal force or variations of temperature.

Shafts are usually made of mild steel and run in white metal bearings, which have a spherical seating so that they can follow any whipping of the shaft. Most firms now use former wound armature coils fixed in open slots and held in position by strong fibre wedges, driven into grooves in the armature teeth, so that the pressure is evenly distributed over the entire length, the end windings being held by manganese bronze shrouds. The core plates, as in a slow-speed machine, are separated at intervals by distance pieces so as to form ventilating ducts. The yoke and field system are usually enclosed in an iron housing which reduces the noise inherent to this type of machine to a minimum. The housing is provided with an inlet and outlet which enables the armature to act as a fan, the current of air being directed to those parts of the machine that require cooling, so that the temperature distribution is more uniform than in an open-type machine, and undue heating is thus prevented.

All parts of the armature must be accurately balanced piece by piece as manufactured, and when assembled must be balanced both statically and dynamically as perfectly as possible. Usually arrangements are made so that balance weights can be added at each end of the commutator and at the turbine end of the armature. To obtain the static balance the armature is laid on two well levelled steel rails with hardened surfaces, and as it is very heavy (say 5 tons or more), the axis of the shaft will be curved so that the rails will actually only touch the shaft in one spot. If this does not damage the shaft it will at least increase the resistance to revolving owing to the power required to overcome the deformation of the shaft. In order to avoid this the rails are laid on knife edges so as to enable them to adjust themselves to the slope of the shaft. With this arrangement the armature gets more sensitive for revolving and balancing becomes easier.

When the armature is lying across the rails there are several forces acting. First of all there may be an out-of-balance weight at a certain radius producing a certain torque. The friction between the shaft and the rails causes a torque in opposition to this. The deformation of the shaft causes another torque which is also in opposition. Both the

friction torque and the deformation torque can be regarded as constant. The armature will not revolve so long as the torque due to the weight is equal or less than the sum of the friction and deformation torques, so that there is a limit of the angle within which the armature never starts moving of its own accord, even when having an out-of-balance weight.

The armature is then revolved until it just reverses of itself. This gives the limit on one side. This position is then marked by a line through the centre which marks the limit. The armature is then revolved in the other direction and the other limit determined in the same way. By dividing into halves the angle between these two, the position of the out-of-balance weight is found so that the weights to balance this are placed diametrically opposite. The armature is then put so that the line dividing this angle is horizontal.

To obtain the weight a thin string attached at the top by a wooden plug is hung over and weights are added until the armature starts to revolve clockwise. The weight is then determined, at which the armature just starts revolving counter-clockwise. The mean of these two weights is then the amount required to balance the armature.

After having been balanced statically, the armature still requires to be balanced dynamically, for if the out-of-balance weight and the balance weights which were put in are not in the same plane perpendicular to the axis, they will act as a couple of forces. This couple must be overcome without altering the static balance, which is done by taking off some of the balance weights, and putting them on the corresponding place at the other end of the armature. If on running the armature up to speed a good balance cannot be obtained by this method, it shows that there is another couple acting in another axial plane. To get rid of this couple equal weights should be put in a diametrically opposite position at the two ends. The two weights are then moved round, always keeping them in opposite positions to each other until the place is found where the balancing is best. Then the weights are altered until the vibration disappears.

It is, however, sometimes necessary to balance an armature when there are no means of balancing it statically. To do this, each end of the shaft and also the steel shrink rings are marked by a pencil. If the shaft is stiff, marks made at varying speeds will be fairly constant in position, but if the shaft is structurally weak the marks will vary their position at different speeds. The high marks, however, are very seldom in the position where the out-of-balance lies, so that the position in which to put the weights is not that opposite to the mark. If the armature is badly out of balance, the actual high place on the shaft leads the pencil mark by approximately 90°, so that the balance weights should be placed approximately 90° behind the mark. If, however, the armature is only slightly out of balance, the high place may lead the mark by about 30°, so that the weight should then be added about 150° behind or almost opposite.

If, however, this method does not quickly improve the balance

of the armature with which one is dealing, the author finds it best to take out every removable balance weight, and then to start again with a certain weight in the balancing ring nearest the middle of the armature, and to vary its position until an improvement is noticed. The weight in this position is then increased or decreased until the limit for that position is found. Then proceed in the same manner with the two ends, always taking diagrams of the marks and recording them together with remarks on the vibration at the time, so that the effects of the altered weights can be seen, and also any previous combination of weights can be readily obtained and no trial is made similar to a previous one.

It is seldom that two armatures, even similar ones, are found to be alike as regards their behaviour during balancing. Sometimes a satisfactory result is obtained on the first run after balancing statically, but often it is a matter of many hours, and especially if there is no arrangement made for quickly stopping the machine, it is often a question of days.

Although the armature may be balanced so that there is no vibration near the machine, it sometimes happens that vibrations appear in another part of the building some distance away. This trouble, however, usually only appears on running up the machine when it may have to pass through its critical speed.

Although in the past few years many difficulties have arisen owing to the very high speeds, the direct-current turbo-generator has reached such a stage that it can now be considered as a successful and reliable unit, and there is little doubt that engine-driven sets will eventually be superseded by those that are turbo-driven.

DISCUSSION.

Mr. G. STONEY: I think Mr. Hoult must be congratulated on his paper. It puts very clearly the position of continuous-current high-speed machinery, and I think he very fairly shows that there are three methods of compensating high-speed turbo-dynamos. Compensating is necessary with a turbo-dynamo because commutation constants come out very much worse than in ordinary slow-speed machines, so much so that even with carbon brushes one cannot overcome the effects of armature reaction. Mr. Hoult has spoken about commutation poles. They have not proved satisfactory in practice, and there is great liability to flash over, as he says. The combination of compensating winding and commutation poles seems much more satisfactory, but still it has its defects, and I believe the real solution is to have compensating windings only and to put on sufficient compensating winding to give a commutation field in the gap. In 1885 we tried the compensating winding. It did not work, because we had only the same number of ampere-turns on the field magnets of compensating as we had on the armature. A few years ago we returned to the same subject, and increased the number to about 2 and 2½ times the ampere-turns on the armature, and we immediately got Mr. Stoney.

Mr. Stoney. exceedingly good results. Some of the old machines which had not been compensated before were altered and partly compensated. For instance, Mr. Pearce's 1,800-k.w. machines at Dickinson Street were our first very large continuous-current machines, and at first they were not compensating. We then compensated them as far as we could, but they have only got about 1¼ times the ampere-turns in the armature, and that is not enough. If there had been space enough to allow of increasing this to about 2 or 2½, we would have got perfect commutation such as we get in our modern machines. The great advantage of having compensating winding only without interpoles is that the lagging of these interpoles is avoided, and with sudden changes of load one also avoids sparking, also the liability to flash over. This is especially the case if there is a short and the circuit breaker comes out on traction load. Also it is not actually necessary to make the compensating winding of any certain amount, and thus we get rid of the use of diverters. In two cases we put in diverters, but they were in our very early machines, where we went to extremes. I think that a great thing is to have a good range on the brushes. If the brushes can be moved a fair distance without sparking, it means that we get what we call "soft commutation" as compared with "knife-edge commutation." With compensating winding only a very smooth field is obtained in the air-gap, and as a result the brushes can be shifted considerably. We find then that brass wire brushes, or copper foil brushes, or any metallic brushes, are perfectly satisfactory, and there is no trouble whatsoever due to such effects as Mr. Hoult speaks of, of sparking under the brush, and there is no movement of the brushes from no load to 25 per cent. overload. We have tried carbon brushes in several cases, and in some cases—for example, at Norwich—they are running very well; but they require careful treatment, and the number of brushes is much larger than is necessary with brass wire. On the whole, the trouble of keeping the commutator in first-class order, as is necessary with carbon brushes, is much more than the trouble of brass wire brushes. I see there is one machine supplied to the Steel Company of Scotland which has commutating poles and compensating windings, and also that there are 96 carbon brushes to carry 2,000 amperes. We could have done the same thing with 20 brushes.

A great deal has been said about surface-wound armatures as against slotted. We experimented on the latter, and found that they did not give nearly as good commutation as surface-wound. Also I do not think the surface-wound are any more likely to break down. Within the last few months I have heard of several very serious cases of breakdown of slotted armatures, and, although we have had breakdowns with surface-wound, we have, considering the number of machines out, amounting to some 400 or 500, fewer breakdowns than there have been with slotted armatures.

Another point about having compensating winding only without commutating poles is that there is absolutely no trouble in parallel

running. If, as Mr. Hoult has mentioned, compounding coils are used, we have found in certain cases it is better to cross-connect a portion of the compensating winding as well as the compounding, but that is the only case we have to cross-connect. In ordinary shunt-wound machines with compensating windings we have never yet had to put any cross-connections of any sort at all. Mr. Stoney.

Speaking of collecting with brass wire brushes, I may say that we have built machines of 300 k.w. at 110 volts, and we built a short time ago eight of 400 k.w. at 110 volts for supplying power and light on the big express Cunarders *Mauritania* and *Lusitania*, and we have never had any trouble. We prefer, if possible, to have 2 poles instead of 4. We have never yet had a case of a flash over in a 2-pole machine. We have had flashes over with 4-pole machines, but it has been stopped by thickening the mica in the commutator; but we find the 2-pole give much better commutation than 4-pole, and they are just as cheap to build; therefore it is obviously an advantage to have only 2 instead of 4 poles. We have never considered it necessary to put wood rings for protecting the commutator rings from flashes. I am afraid those wood rings will in time shrink and dirt will get under them and cause short circuits. I know that this has occurred with some commutators which had protected rings. There is a point also which I think designers of high-speed turbo-dynamos have hardly laid sufficient stress on, namely, the question of high speeds. The Brown-Boveri standard speed is 2,100 revs. per minute for a 500-k.w. machine. I understand from the paper that a 400-k.w. Siemens set runs at about 1,800 revs. per minute. I may say that our speed for 500 k.w. is 3,000 revs. per minute, which means a turbine that has only about half the spindle volume, and also has from 5 to 6 per cent. better steam consumption. High-speed turbo-dynamos are not cheaper to build than the slow-speed, but the gain comes in the turbine. A much cheaper and much more efficient turbine is obtained if the dynamo is run at as high a speed as possible, and I think that is one advantage of our formation of compensating winding and surface-wound armatures, that one is able to build dynamos with much higher speeds than is possible with the ordinary slotted armature and a mixture of compensating and commutation poles.

Mr. S. L. PEARCE: We have had two of the classes of turbo-generators that Mr. Hoult has classified in page 2 of the paper, namely, those provided with compensating windings and those with commutation poles only for consideration. The machines that were installed some six years ago in Dickinson Street were neither provided with compensating windings nor commutation poles. They were of 1,800-k.w. capacity (2–900 k.w. on the same shaft). The armatures were of the smooth-core type, and the cores secured by means of binding wire. The peripheral speed of the armature is 17,000 ft. per minute, and the peripheral speed of the commutator 6,500, and those machines were provided with the automatic brush rocking gear that Mr. Hoult describes in his paper, so that the position of the brushes can be varied automatically according to Mr. Pearce.

Mr. Pearce. the pressure of the steam corresponding to the load. That was not saticfactory, and subsequently, as Mr. Stoney has told us, compensating windings were put on as far as permissible with the design of machine. He has stated that it was not possible to get on the amount that they would have supplied had the machines been designed for compensating windings in the first place, and as a matter of fact I believe they are only supposed to give a fixed position of the brushes from no load to a little over half-load. At any rate, it is something under three-quarter load in actual practice. Before the compensating windings were added there was a movement of 4 in. to the brushgear from no load to full load. With the compensating windings the movement is now about 1 in., that is from no load to full load, to give sparkless commutation. Parsons' arrangement of confining brushes of opposite polarity each to their separate cells is a good one.

Dealing with the question of the brass wire brushes *versus* the carbon brushes, we certainly did expect that with the addition of compensating windings we should see the bill for renewals and repairs of brushes considerably reduced, but that is not the case. We find that our brushes wear away now at pretty much the same rate as they did before, and I think Mr. Hoult is quite right in the remarks that he makes, that there is, although the brushes may appear to be working sparklessly, a considerable amount of wear and tear going on underneath the surface of the brush; at any rate, we find that the consumption of wire brushes is just about the same with the compensating windings as it was before they were added. As regards the commutators themselves I must say that they have stood remarkably well. I think in six years they have only been turned up once, and although the makers when the machines were specified and bought would only give ten years' life on the commutators, I am perfectly certain that they will very greatly exceed that, and are probably good for twenty years or more.

Therefore we have not had very much trouble with the commutators, and our expense has been wholly the cost of the wire brushes, which has been so heavy as to neutralise any saving that we got due to lesser oil consumption, etc., as compared with reciprocating units. It may interest the members to know that at the present time a complete set of the brass wire brushes is used up in 336 hours, that is, every 336 hours we have to renew the brushes completely.

The second type of machines we had installed in our station were fitted with commutating poles only, and with these machines we have not been free from trouble. We find that our chief trouble has been due to flashing over. These machines, unlike the Parsons corrugated type, have smooth commutators fitted with carbon brushes. To prevent flashing to earth we have had the inside faces of the shrink rings lined with wood, and that has made a satisfactory job as far as our experience at the present times goes. We have not had the difficulties that Mr. Stoney seemed to fear we should have. Several flash overs, especially on traction loads, have occurred, but I think I

may say that these have to all intents and purposes been cured by the insertion of a diverter in the commutating-pole circuit, such a diverter as Mr. Hoult described with the adjustable air-gap. On lighting loads we do not have the difficulties that we have on traction loads, and that one would only expect. Mr. Pearce.

As to the type of brushes, probably we have tried nearly every brush that is procurable. We have tried the various types of the Morganite Crucible Company's productions, and we have tried the ordinary hard carbon brushes, and the Endruweit, and at the present time we get a successful combination with three Endruweit and one hard carbon brush. I quite agree with Mr. Hoult in his statement that the only graphite brush of a type capable of being used on high-speed machines is one "in which the layers are in the direction of the running of the commutator, and consequently having the cross-resistance in an axial direction instead of a tangential one." I can very fully confirm that. I think the question of satisfactory running with the smooth commutators and carbon brushes is to a very great extent a question of ventilation. We tried the use of external blowers, and we certainly were able to effect a very considerable reduction in the temperature of the commutator bars, and got very much more satisfactory running, and I noticed some eighteen months ago that that arragement was in use pretty largely on the Continent, being used by the Simens-Schuckert Company, and, I believe, the Allgemeine Elektricitäts Gesellschaft.

With both types of machine we have not experienced any difficulty in parallel running. I do not think, perhaps, that sufficient credit has been given to the work of Parsons and Stoney. There is no doubt there are quite a large number of direct-current turbo-generators at work, and are doing very good service in the country to-day. My chief criticism, I think, with regard to the Parsons machine is that it does not appeal to me as being a very mechanically constructed machine. To see those large 900-k.w. armatures we have in Dickinson Street, surface wound, in which the windings are only apparently relying entirely upon the binding wire, does not strike me as being good from a mechanical point of view, and one does not like to contemplate what might happen if the binding wires gave way when the machines were on load. It is true we have not experienced the slighest difficulty, but still it does not appeal to one as being good, sound mechanical construction; but, as Mr. Stoney has told us, the percentage of failures is very small. At any rate, I feel certain that whichever type finally emerges as, shall I say, the standard type for turbo-generator construction, I do not think, in the light of our experience to-day, the statements that were made two or three years ago that only one firm could build turbo-generators, and that not a British firm, are at all justified.

Mr. V. A. H. M'Cowen: I had to face the question of direct-current turbo-plant about eighteen months ago, and went to inspect some Continental machines after seeing some of the English ones, and I was agreeably surprised at the operation of the Brown- Mr. M'Cowen.

Mr. M'Cowen. Boveri machines abroad. In Rotterdam they were operating both tramways and lighting, but working in parallel with batteries, which I thought was probably the reason for the good commutation. I got them to disconnect the batteries and run the whole system of tramways on the turbines, but it made practically no difference, and from what I saw there I was quite satisfied with the machines, and recommend similar ones for Belfast. Since that time I have had a 1,000-k.w. Willans-Brown-Boveri machine running in Salford, and it has been running remarkably well. We have been free from commutator troubles, but there is no doubt that the brush expense is very considerable. On a continual run of six months the wear on the commutator itself is not appreciable, but the cost of the brushes is excessive, and the amount of attention is large. From some figures I had carefully taken over a month's running, I find that the cost of brushes alone works out to about 0·003d. per unit. These machines are arranged with separate exciters, and as far as parallel running is concerned we have had no trouble. They simply keep their steady load and the reciprocating sets take up the variations. I have had an opportunity just recently of inspecting machines with the radial type of commutator and carbon brushes. I have seen a 400-k.w. set run up from no load to full load without any movement of the brushes, and then the whole load thrown off with absolutely sparkless commutation. I have never seen better commutation on any machines, and I think that it is most satisfactory to us British engineers to know that the British firms have taken the matter up in this way, and that we can look forward to getting machines at home which are quite as satisfactory as foreign ones.

Mr. Peck. Mr. J. S. PECK: I believe that we must come to the use of the carbon brush for direct-current turbo-generators. All the arguments that are now advanced in favour of copper brushes and surface-wound armatures were advanced many years ago for direct-current machines of ordinary speeds, and as the carbon brush and the slotted armature have driven the copper brush and the surface-wound armature out of existence for standard speed generators, so I believe they will drive out the copper brush and the surface-wound armature for turbo-generators, and, after what we have heard to-night, I am more than ever convinced that we shall never get an entirely satisfactory direct-current turbo-generator until we adopt carbon brushes and slotted armatures.

One point I should like to ask Mr. Hoult, and that is regarding the ventilation of the commutator. The ventilating duct through the commutating core must certainly reduce the wearing depth very much. When the commutator wears down to the duct, is it possible to operate the machine without a new commutator?

The illustration, Fig. 10, does not show clearly the wood barriers inside the shrink ring. Do these barriers reach to the top of the rings?

I am rather amused at what Mr. Hoult says about balancing. After going through numerous tests and refinements to obtain satisfactory

balance he does not get it, and then he takes out all weights and starts over again. He has evidently had some experience in balancing. Mr. Peck.

Dr. E. ROSENBERG: Mr. Stoney has undoubtedly had great experience and success in direct-current turbo-generator work, and it is quite natural that his success makes him conservative. His remarks on brushes, smooth-core armatures, and number of poles are all connected with this conservatism. If smooth-core armatures are adhered to, copper brushes can be used, and, in this case, on account of the necessarily great air-gap, a 4-pole machine is no cheaper than a 2-pole machine. It is hardly possible, however, with large machines of this type to use a fixed brush position, and this is what the station engineer wants, whereas slotted armatures, commutating poles, and compensating winding allow of a fixed brush position, and also perfect commutation if carbon brushes are used. In this respect I fully agree with Mr. Hoult that carbon brushes are possible and work very well if the commutator is ventilated. Perhaps the ventilation scheme adopted by Messrs. Siemens Bros. may have the disadvantage that it is difficult to keep the small channels inside the commutator bars clean. I think they are liable to be obstructed by dust and dirt. The radial-type commutator of the Westinghouse Company, as developed by Mr. Miles Walker, has natural surface ventilation, and is simpler in this respect. Dr. Rosenberg.

As to carbon brushes, I agree with Mr. Hoult that a good mechanical construction is more important than the composition of the brushes with layers that give a high resistance in the direction of movement of the commutator, although the latter will be preferable from a scientific point of view. There are more mysteries in commutation than can be explained by Ohm's law.

As to balancing, the angle between the mark and the position of the required balance weight will be 90° if the speed is just the critical one. The balancing business is very much shortened if arrangements are made to have resonance at a low speed; then the marks are also more distinct, and starting and stopping is quickly done. Resonance can be obtained at low speed by supporting the bearings by means of springs.

Mr. W. PARKER: Mr. Hoult has dealt very fully with the relative advantages and disadvantages of metal and carbon brushes, and comes to the conclusion that carbon brushes will eventually supersede metal brushes. With this statement I entirely agree, and I should like to add one or two reasons in support of this conclusion which Mr. Hoult has not mentioned in his paper. Mr. Parker.

If one goes back to the early stages of the electrical industry, one finds that metal brushes were used exclusively on direct-current machines. I refer more particularly to that period when bipolar machines were at the heights of their popularity. At this period direct-current machines were principally used for lighting where the load is fairly steady and the brushes could be moved as the load came on. Nevertheless, the metal brushes on the old bipolar machines

Mr. Parker.

required a considerable amount of attention in order to keep the machine running sparklessly, and this in spite of the fact that they all ran at very low speeds.

With the advent of traction larger units were called for. They were also required to run with a fixed position of brushes with a very fluctuating load, and to give considerable overloads. At this stage it became obvious that metal brushes unaided were no longer capable of coping with the new conditions. In order to overcome the difficulty designers added a carbon tip to the metal brush in order to take up the sparking. While this produced an improvement, it soon became obvious that this device was not sufficiently drastic to enable the designer to keep pace with the ever-increasing size of unit which the industry demanded. Something new was absolutely essential, and this was discovered in the exclusive use of the carbon brush. The excellent qualities of a carbon brush soon became obvious, and it was so much appreciated that it became universal in spite of the larger and more expensive commutators which this change involved.

The industry still developed, however, and the carbon brush in its turn became incapable, unaided, of coping with the new requirements. Variable-voltage machines for Siemens' Ilgner sets, large reversible rolling mill motors, variable speed machines, etc., were called for which carbon brushes alone were incapable of dealing with. Something new was again required, and this was found in the commutation pole. This is now, as in the case of the carbon brush, becoming universal, as the carbon brush has been strained beyond its capacity in exactly the same manner as its predecessor, the metal brush, had been strained.

We now come to the advent of the turbo-generator—the most difficult case the designer has yet had to deal with. From the historical point of view carbon brushes and commutation poles promptly suggest themselves. The case before is an exceedingly difficult one, and we should therefore use those devices which have been found most useful in the past. But what do we find? We find that instead of carbon brushes, as one would expect, metal brushes were used on the first machines, and are, in fact, still being used.

Now, of course, one would expect to find some very good reason why the carbon brush was, in spite of the history of the direct-current machine, discarded. The reason is that there was an impression abroad at the time that carbon brushes could not be got to run on commutators at this high peripheral speed without excessive vibration. This, at all events, was the reason why the first turbos designed by the firm which I represent were designed to run with metal brushes.

Now observe how history repeats itself. The metal brush is, as one would expect, found to be unsatisfactory, and carbon tips are added, notably by Brown Boveri, in order to improve the commutation, thus showing, by the way, that carbon brushes in the form of tips will run without excessive vibration.

It is now only necessary to build turbos with carbon brushes

exclusively to complete the historic cycle. As a matter of fact this has already been done by the firm which I represent, who have several machines running successfully with carbon brushes. It is also within my knowledge that one of the largest firms in Germany, who have built upwards of 200 turbos with metal brushes, have now gone over entirely to carbon brushes. Mr. Parker.

When the question of constructing direct-current turbos with carbon brushes was seriously taken in hand, it was found that the vibration difficulties could be overcome with a suitable brush holder, and that the real difficulty was the excessive heating of the commutator. In order to overcome this difficulty it is absolutely necessary to cool the commutator artificially, either by channels in the segments, by delivering a stream of air on to the commutator surface, or in some other suitable manner.

With the method of cooling the commutator described in the paper, it is possible to keep the temperature rise on the commutator down to about 40° F. with a density in the brushes of about 45 amperes per square inch, the brushes used being those of Le Carbone "X" or "Z" quality.

Mr. Hoult in his paper gives the impression that the commutator would be considerably longer with carbon brushes. This, however, is not the case, as the following comparison shows: With metal brushes the density is kept down to about 130 amperes per square inch, and the arc of contact to about ⅓ in., hence the current collected per inch axially of commutator is 43 amperes about. Now with carbon brushes it is possible to increase the arc of contact to 1 in. or more, and the current collected per inch axially is therefore 45, hence the commutator when fitted with carbon brushes and artificial cooling is no longer than when fitted with metal brushes.

As regards the wear on the commutator with carbon brushes, I should like to say that after two years' running with carbon brushes on a 400-kw. machine the wear of the commutator did not exceed 2 mm. This machine runs twenty-four hours a day on practically full load, and only shuts down at the week-end.

Mr. W. V. SHAW: There is one point in connection with the sparking of brushes on commutators which has not been mentioned, namely, the vibration. If the armature is not running smoothly sparking occurs due to vibration, and no doubt a great deal of wear on the commutator is produced by sparking. Mr. Shaw.

Mr. G. D. SEATON: The first thing that strikes me about this paper is a matter which has been already referred to. It shows us very completely what a very deep sense of gratitude we ought to feel to Mr. Parsons for all the time, labour, and money he has spent in bringing the turbine into the state in which it is; but for Mr. Parsons' pertinacity, there is no saying where we should have been to-day. Mr. Seaton.

Mr. Pearce has referred to the mechanical construction of turbo-dynamos. As a mechanical man I almost feel appalled when I look at either a dynamo or an alternator, and there is no doubt the electrical man has a lot to learn from mechanical engineering yet.

Mr. Seaton. A little has been said about the cost of brush wear. When Mr. M'Cowen mentioned that his brush wear was 0·003d. per unit, I began to congratulate myself that I lived in Manchester instead of Salford. I turned to my friend Mr. Pearce for his confirmation or otherwise of the figure, and I was staggered to find that his figure is 0·006d., so now we have some slight explanation for our rate of 8s. 2d. in the pound.

Mr. Baille. Mr. J. D. BAILIE: Mr. Pearce referred to the heavy wear on the brushes of the Dickinson Street 1,800-kw. machines continuing after the fitting of the compensating windings. Probably that is so, and it is no doubt due to the fact that the compensation windings are not sufficient—as Mr. Pearce said, the machines are only a little over half-compensated. We have now many machines running fully compensated, and the brush wear is very little indeed. At one time we used binding wire on alternating as well as on direct-current armatures, and though it has been used on some hundreds of machines, failure of the binding wire is almost unheard of. The binding wire is the best piano wire procurable; it is not liable to break, and I think, in the ordinary way, a failure is next door to impossible.

Mr. Hoult infers that heating of the commutators was a cause of prejudice against the use of carbon brushes. It is quite a common thing with the Parsons commutator, when metal brushes are used, for the temperature rise after long tests of, say, six to twelve hours' duration, not to exceed 50° to 60° F., frequently 40° F. only, and I do not think that in a well-designed machine the heating of the commutator is, in itself, quite such a difficulty as is inferred. It is probably caused by the use of the carbon brushes. Incidentally, I may mention that I know of machines giving currents of up to, say, 800 amperes, on which brushes of manganine wire are used with very satisfactory results. I can fully endorse what Mr. Stoney has said in regard to paralleling, for, in the ordinary way, no difficulty whatever is experienced. Mr Hoult refers also to a tendency to chattering when metal brushes are used; I think that it is not very noticeable when the brush-holders are of the gravity type.

Mr. Hoult mentions also the necessity of attending to the brushes and of changing them on long runs. I know many machines that run for three weeks or a month without shutting down, and I have known them run for six months without a single stop; some attention to the brushes certainly is required, but there is no difficulty in this, as, in the event of the bearing surface of the brushes on the commutators becoming too great, they can easily be withdrawn one at a time and reversed in the holders.

Mr. Walker. Mr. MILES WALKER (*Chairman*): There is one point I should like to bring out. It is that manufacturers in future in this matter will not be controlled so much by what they themseves think as by what the users think. However much we may be convinced that the binding wire is strong enough—and I believe it is—however much we are convinced in our minds that a smooth-core armature will stand, I am rather afraid we shall never be able to convince the user. The

same applies to the metal brush. The metal brush may be made to work perfectly. I have seen metal brushes working quite sparklessly, and I think Mr. Stoney is to be congratulated on the enormous skill he has brought to bear on his machines to make commutation so good under very difficult conditions. But the user does not care about that, he does not want a machine which is a very successful attempt at the solution of a difficult problem, all he wants is a machine which he is quite sure his ordinary attendant can look after without any trouble. He does not want any question to arise as to whether the brushes are fed just at the right time; or as to turning the brushes over, he wants a machine like his ordinary solid engine type of machine, where the brushes run from year end to year end without much attention. There are many machines in existence on which carbon brushes have been running for three years, with not more than about ¼ in. wear on the brush, and no perceptible wear about the commutator. The brush seems to polish the copper, and the copper seems to polish the brush, and neither wears the other to any great extent. We have therefore had to deal with the question of how to make a carbon brush run smoothly without chattering, and how to get rid of the heat which is necessarily produced by the high resistance of the carbon brush. Mr. Walker.

Now, the question of the slotted armature must be considered in the same way. The user wants a slotted armature because of its mechanical strength.

Another point that has not been touched upon is the question of volts per segment. The user, I think, in the future will find by experience that machines of high voltage per segment on the commutator are not so satisfactory under difficult conditions of load as machines with a low voltage per segment. A machine may run perfectly well with 30 or 40 volts between the bars, even higher; but long experience has shown with ordinary machines that if the voltage per bar is kept down we are much safer. We can have a machine working at 100 per cent. overload, and jerk the load on and off, and there is a good factor of safety. There again the user will ask for a factor of safety. I certainly think in the future we shall find about 20 volts per bar as a standard for high-voltage machines for traction work, and although it is cheaper from the manufacturers point of view to build them higher, I think the user will get what he asks for.

Mr. Stoney is perfectly right in saying that from a theoretical point of view a very strong compensating winding is much better than a commutating pole, but if you can show that a commutating pole does its work, and you can put the load off and on very suddenly, without trouble from sparking, then, as the commutating-pole machine is the cheaper machine, the user again will buy the cheaper machine.

Now with regard to the question of insulation. It is very important to have a high factor of safety on all machines, particularly on turbo-generators, and one must keep big clearances all through the machine, so that if there is any collection of dust there is no liability to flash over the surface.

Mr. Walker.

Another point, I think, of importance is the question of exposed rings. It may be possible to cover the rings, but it will always be a difficult matter, and if we can get rid of them in some satisfactory way, I think the machine that has no rings will have an advantage. Of course stability in parallel running is a very important point. It is also an important point in a machine to have the frame horizontally split. Where the frame cannot be split the armature has to be pushed in from one end, and to do this either the air-gap must be very big or the binding on the conductor very small. I should certainly advise the user to see that he gets a good factor of safety. It is easy enough to build a machine, say, with a factor of safety of 2 which will run, but a factor of safety of 2 is not good enough. We want a factor of safety of at least 5, and more if it can be got.

Mr. Hoult.

Mr W. HOULT (*in reply*): During the discussion several speakers have shown their appreciation of the work done by Mr. Parsons, and I wish to take this opportunity of mentioning that no one recognises more than I do the great work that Mr. Parsons and Mr. Stoney have done in developing the turbo-generator, both alternating and direct current. There is no doubt that without the skill and perseverance of Messrs. Parsons neither the turbine nor the turbo-generator would be anywhere near the position that they have to-day.

Mr. Stoney inferred that with commutation poles above the brushes had to be set almost miscroscopically, and machines with them had not proved satisfactory. I have always found that there was a considerable range through which the brushes could be moved with sparkless commutation, the only difference in the running of the machine being that with the brushes in advance of the neutral position the shunt regulator switch had to be moved further for the same increase of load owing to the demagnetisation of the main field. In certain cases commutation-pole machines can be built to fulfil all requirements, and I have stood by such a machine (500 k.w. fitted with carbon brushes) while full load has been thrown on, and it was impossible to tell by watching the brushes whether the machine was on load or not. The machine referred to has been in constant service for more than a year, and is frequently overloaded, but has never been known to flash over. Compensating windings could not improve this machine.

Mr. Stoney also referred to machines being absolutely sparkless with the compensating winding. I quite believe that machines can be made to run sparklessly at all loads with fixed brush position with compensating windings, but it requires a very exact calculation. He mentioned as examples the sets supplied to the *Lusitania* and *Mauretania*. I have seen the sets referred to, but was informed by a dynamo attendant that the brushes had to be shifted a little when the load came on.

The machines at the works of the Steel Company of Scotland have not compensating windings as mentioned by Mr. Stoney but have commutation poles only.

Mr. M'Cowen referred to the amount of attention required with the metal brushes in his station and their excessive cost in renewals. I have had machines to run absolutely sparkless with them, but as a brush wears down it of course needs to be fed forward, which alters the bearing surface, so that the back of it only is in contact with the commutator. It usually then takes the attendant all his time to keep the machine satisfactory. Parsons' wire brush, however, beds itself better to the commutator, and adjustment does not much affect the bearing surface. Mr. Hoult.

As regards Mr. Peck's reference to the Siemens ventilated commutator, the amount that can be safely turned down is about ⅜ in., so that it will be very many years before a new commutator would be required. His remark on the balancing refers to the balancing of an armature when it cannot be balanced statically, as, for instance, when it is in its bearings and coupled to the turbine. In this case it is always better to first of all try the first method, for a complete balance may then be obtained in a very short time; but in certain cases, chiefly depending on the build of the shaft, a complete balance is very difficult to obtain. The second method cannot be used unless the old balance weights are first taken out, as the trial weights require to go into any position in the balancing ring. The balancing diagrams are then also simple and clear.

With reference to Dr. Rosenberg's remarks on the possibility of the holes in the commutator getting filled up with dust, I have never known of any trouble arising from this cause, and with an ordinary clean engine-room do not anticipate any.

Mr. Bailie referred to the successful parallel running of Messrs. Parsons' machines fitted with automatic brushgear. I can also say that I have seen such machines working extremely well even on traction loads. With reference to brushes, he mentioned that he had known machines to run for six months without a stop. I have known of machines with metal brushes running for long periods without a stop, but I have not seen any fitted with metal brushes that could run sparklessly on full load for six hours without considerable attention.

Mr. Shaw referred to vibration causing sparking. Of course, if the armature is badly out of balance the brushes jump and sparking can occur. Unless the vibration is very bad, however, the commutation is not usually much affected, the effect being to increase the temperature of the brush and commutator. With short ventilated commutators no trouble whatever is experienced in this direction.

The protection of the steel binding rings was referred to by several speakers. Mr. Stoney was afraid that the wood rings described would shrink, but there has been no trouble on this account, and even if they do shrink there are pieces of mica behind the divisions which make it practically impossible for any dust to get underneath and form short circuits. The wood does not reach to the top of the steel ring as there is no necessity for it—in fact, all that is really required is a depth of about an inch. There are two reasons why the rings should be partially

Mr. Hoult. protected: first, to prevent any metal or carbon dust from bridging across the small mica insulation between the shrink ring and commutator; and secondly, to prevent a sudden flash at the brushes, due to, say, a dead short on the machine reaching the steel ring. Such a flash of itself would never get to the circumference of the ring if the inner part is protected.

INDEX TO VOL. 40.

1907-1908.

EXPLANATION OF ABBREVIATIONS.

[P] signifies a reference to the general title or subject of a Paper.

[*p*] signifies a reference to a subject incidentally introduced into a Paper.

[D] signifies a reference to remarks made in a Discussion upon a Paper, of which the general title or subject is quoted.

[*d*] signifies a reference to remarks incidentally introduced into a discussion on a Paper.

Note.—The lists of speakers in the Discussion upon any Paper will be found in the Table of Contents at the beginning of the volume.

A.

B.

C.

H.

I.

J.

K.

L.

M.

N.

O.

P.

R.

S.

T.

W.

Y.

UNWIN BROTHERS, LIMITED, THE GRESHAM PRESS, WOKING AND LONDON.

No. 189. ISSUED JUNE, 1908. VOL. 40.

JOURNAL

OF THE

INSTITUTION OF ELECTRICAL ENGINEERS,

LATE

THE SOCIETY OF TELEGRAPH-ENGINEERS AND ELECTRICIANS.

FOUNDED 1871. INCORPORATED 1883.

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

PUBLISHED UNDER THE SUPERVISION OF THE EDITING COMMITTEE
and Edited by
GEORGE C. LLOYD, Secretary.

London:
E. AND F. N. SPON, LIMITED, 57, HAYMARKET, S.W.

New York:
SPON AND CHAMBERLAIN, 123, LIBERTY STREET.

PRICE FIVE SHILLINGS

The Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

COUNCIL AND OFFICERS.—1908-1909

President.

COLONEL R. E. CROMPTON, C.B.

President Elect.

W. M. MORDEY (assumes office in Nov., 1908).

Past-Presidents.

SIR WILLIAM H. PREECE, K.C.B., F.R.S.—1880 & 1893.
PROFESSOR G. C. FOSTER, LL.D., D.Sc., F.R.S.—1881.
PROFESSOR W. GRYLLS ADAMS, F.R.S.—1884.
C. E. SPAGNOLETTI.—1885.
SIR WILLIAM CROOKES, F.R.S.—1891.
PROFESSOR W. E. AYRTON, F.R.S.—1892.
ALEXANDER SIEMENS.—1894 & 1904-1905.
COLONEL R. E. CROMPTON, C.B.—1895.
SIR HENRY MANCE, LL.D., C.I.E.—1897.
SIR J. WILSON SWAN, D.Sc., F.R.S.—1898-99.
PROFESSOR SILVANUS P. THOMPSON, D.Sc., F.R.S.—1899-1900.
PROFESSOR JOHN PERRY, LL.D., D.Sc., F.R.S.—1900-1901.
JAMES SWINBURNE, F.R.S.—1902-1903.
ROBERT KAYE GRAY.—1903-1904.
SIR JOHN GAVEY, C.B.—1905-1906.
DR. R. T. GLAZEBROOK, F.R.S.—1906-1907.

Vice-Presidents.

F. GILL.
COL. H. C. L. HOLDEN, R.A., F.R.S.
DR. G. KAPP.
C. P. SPARKS.

Members of Council.

S. EVERSHED.
H. E. HARRISON, B.Sc.
DR. E. HOPKINSON.
J. W. JACOMB-HOOD.
J. E. KINGSBURY.
T. MATHER, F.R.S.
M. O'GORMAN.
MAJOR W. A. J. O'MEARA, C.M.G.
G. W. PARTRIDGE.
W. H. PATCHELL.
W. RUTHERFORD.
PROF. A. SCHWARTZ.
J. F. C. SNELL.
G. STONEY.
A. A. C. SWINTON.

Associate Members of Council.

J. HUNTER GRAY. | H. HUMAN. | J. E. TAYLOR.

Chairmen of Local Sections.

PROF. F. G. BAILY, M.A. (*Glasgow*).
J. P. EDWARDS (*Cape Town*).
M. B. FIELD (*Manchester*).
DR. G. KAPP (*Birmingham*).
PROF. G. D. A. PARR (*Leeds*).
J. PIGG (*Newcastle*).
T. TOMLINSON (*Dublin*).

Hon. Treasurer.—ROBERT HAMMOND.

Hon. Auditors.—H. ALABASTER AND SIDNEY SHARP.

Trustees.

PROFESSOR G. CAREY FOSTER, LL.D., D.Sc., F.R.S.
SIR WM. H. PREECE, K.C.B., F.R.S.
JAMES SWINBURNE, F.R.S.

Hon. Solicitors.

MESSRS. BRISTOWS, COOKE, & CARPMAEL, 1, COPTHALL BUILDINGS, E.C.

Chartered Accountants.

MESSRS. ALLEN BIGGS & Co., 147, LEADENHALL STREET, E.C.

Secretary—GEORGE C. LLOYD.

Asst. Secretary.—P. F. ROWELL. **Chief Clerk.**—R. H. TREE.

Library and Offices: 92, VICTORIA STREET, LONDON, S.W.
Telegrams: "Voltampere, London." Telephone: Victoria, No. 911.

LOCAL SECTIONS OF THE INSTITUTION.

LISTS OF OFFICERS AND COMMITTEES.

LOCAL SECTIONS IN THE UNITED KINGDOM.

BIRMINGHAM.

Chairman: Prof. G. Kapp.
Past Chairmen: W. E. Sumpner, D.Sc.
R. Threlfall, F.R.S.
R. A. Chattock.
Vice-Chairman: R. K. Morcom.
Committee: A. H. Bate.
V. Bornand.
A. R. Everest.
A. L. Forster.
W. E. Groves.
C. W. Hill.
Henry Lea.
Dr. D. K. Morris.
A. Pearson.
M. Railing.
A. M. Taylor.
J. C. Vaudrey.
Hon. Secretary: H. B Matthews,
Winchester House,
Victoria Square, Birmingham.

DUBLIN.

Chairman: T. Tomlinson.
Vice-Chairman: G. F. Pilditch.
Committee: P. T. Blissett.
William Brown
N. Hosgood.
A. T. Kinsey.
A. Moir.
P. S. Sheardown.
W. J. U. Sowter.
A. W. Whieldon.
Hon. Secretary: W. Tatlow,
20, Fleet Street, Dublin.

GLASGOW

Chairman: Prof. F. G. Baily.
Past Chairmen: Lord Kelvin, O.M., F.R.S.
R. Robertson.
J. M. M. Munro.
Vice-Chairman: E. George Tidd.
Committee: C. Day.
W. W. Lackie.
J. Lowson.
W. MacWhirter.
T. B. Murray, B.Sc
F. A. Newington.
J. S. Nicholson.
J. K. Stothert.
Hon. Secretary: J. E. Sayers,
157, West George Street,
Glasgow.

LEEDS.

Chairman: Prof. G. D. A. Parr.
Past Chairmen: G. Wilkinson.
W. Emmott.
A. B. Mountain.
Vice-Chairman: H. E. Yerbury.
Committee: J. D. Bailie.
J. W. Beauchamp.
R. H. Campion.
G. M. Carr.
T. H. Churton.
Prof. E. H. Crapper.
S. W. Cuttriss.
C. J. Hall.
W. Hartnell.
S. D. Schofield.
H. Visger.
H. H. Wright.
Hon. Secretary: H. Dickinson,
1, Whitehall Road, Leeds.

MANCHESTER.

Chairman: M. B. Field.
Past Chairmen: C. D. Taite.
S. L. Pearce.
T. L. Miller.
Vice-Chairman: M. Walker.
Committee: Dr. F. H. Bowman.
S. L. Brunton.
W. Cramp.
A. A. Day.
J. Frith.
Dr. E. Hopkinson.
G. Layton.
J. S. Peck.
P. A. Ramage.
Dr. A. Schuster.
S. J. Watson.
A. P. Wood.
Hon. Secretary: H. W. Wilson,
1, Crosshall Street, Liverpool, &
19, Brazenose Street, Manchester.

NEWCASTLE.

Chairman: James Pigg.
Past Chairmen: G. Stoney.
Dr. W. M. Thornton.
H. L. Riseley.
Vice-Chairman: A. L. E. Drummond.
Committee: A. S. Blackman.
L. E. Buckell.
T. M. Clague.
H. W. Clothier.
W. T. Dalton.
J. H. Holmes.
F. O. Hunt.
W. D. Hunter.
A. H. Law.
R. E. Robson.
F. Tremain.
C. Turnbull.
Hon. Secretary: C. F. Proctor,
44, Simonside Terrace,
Heaton, Newcastle-on-Tyne.

LOCAL SECTIONS ABROAD.

CALCUTTA.

Chairman:

Vice-Chairman: Prof. P. Brühl.
Committee: S. G. L. Eustace.
W. Hodgkinson.
J. W. Meares.
J. Williamson.
Hon. Secretary: M. G. Simpson,
Electrician's Office,
Govt. Telegraph Dept.,
Alipore, Calcutta.

CAPE TOWN.

Chairman: J. P. Edwards.
Vice-Chairman: A. S. Giles.
Committee: John Denham.
E. G. Clifford Jones.
W. F. Long.
Prof. H. Bohle.
Hon. Secretary: C. Proctor Banham,
Table Bay Harbour Works,
Cape Town.

LOCAL HONORARY SECRETARIES AND TREASURERS.

J. BANNEUX, Belgian State Telegraph Dept., Brussels	BELGIUM.
E. G. C. BARTON, Brisbane	QUEENSLAND.
A. S. BAXENDALE, Superintendent of Posts and Telegraphs, Selangor, Straits Settlements	STRAITS SETTLEMENTS AND NETHERLANDS INDIES.
ARTHUR VON BOSCHAN, 1, Kantgasse 3, Vienna XVII.	AUSTRIA-HUNGARY.
A. E. R. COLLETTE, Heemskerckstraat, 30, The Hague	HOLLAND.
ERNEST DANVERS, 475, B. Mitre, Buenos Ayres	ARGENTINA.
J. HUBERT DAVIES, P. O. Box 1386, Johannesburg	TRANSVAAL.
JOHN DENHAM, Electrician's Office, Cape Government Railways, Cape Town	THE CAPE, NATAL, AND RHODESIA.
ICHISUKE FUJIOKA, M.E. 56, Zaimokucho, Azabu, Tokyo	JAPAN.
XAVIER GOSSELIN, 170, Boulevard Magenta, Paris	FRANCE.
J. H. HAMMAR, 6 Vasagatan, Stockholm	SWEDEN.
WILLIAM J. HANCOCK, M.Inst.C.E. Government Electrical Engineer, Perth, Western Australia	WESTERN AUSTRALIA.
JOHN HESKETH, Postmaster General's Dept., Spring Street, Melbourne.	VICTORIA.
ARTHUR JACKSON, British Vice-Consulate, Conde de Aranda 1, Madrid	SPAIN.
J. L. W. V. JENSEN, Frederiksberg Alle' 68, Copenhagen	DENMARK.
H. KINGSFORD, Engineer to the Mexican and Central and South American Telegraphs, Lima, Peru	PERU.
J. K. LOGAN, Superintendent of Electric Lines, Wellington	NEW ZEALAND.
J. Y. NELSON, Telegraph Dept., G.P.O., Sydney	NEW SOUTH WALES
JAMES OLDHAM, Manager, River Plate Telegraph Company, Monte Video	URUGUAY.
Professor R. B. OWENS, McGill University, Montreal	CANADA.
G. SEMENZA, Via Tommaso Grossi 2, Milan	ITALY.
ARNOLD VON SIEMENS, Askanischer Platz 3, Berlin, S.W. 46	GERMANY.
M. G. SIMPSON, Indian Government Telegraphs, Calcutta	INDIA.
C. A. UNBEHAUN, Telegraph Dept., Adelaide	SOUTH AUSTRALIA.
G. G. WARD, Vice-President and General Manager, Commercial Cable Company, Broadway, New York	THE UNITED STATES OF AMERICA.

SECTIONAL COMMITTEES.

Members are reminded that these Committees have been constituted partly for the purpose of affording convenient channels through which they may bring to the knowledge of the Council any questions affecting the several branches of Electrical Engineering that they may think require special consideration.

1.—Traction, Light and Power Distribution.

G. L. Addenbrooke.
T. O. Callender.
W. A. Chamen.
R. A. Chattock.
S. Dobson.
F. E. Gripper.
H. M. Hobart.
C. H. Merz.
W. C. Mountain.
G. H. Nisbett.
W. H. Patchell.
S. L. Pearce.
G. S. Ram.
J. H. Rider.
P. S. Sheardown.
J. F. C. Snell.
C. P. Sparks.
A. D. Williamson.

2.—Telegraphs and Telephones.

A. L. Dearlove.
W. Duddell, F.R.S.
Sir J. Gavey, C.B.
F. Gill.
W. Judd.
J. E. Kingsbury.
P. V. Luke, C.I.E.
W. Noble.
C. J. Phillips.
J. E. Young.

3.—Manufacturing.

E. W. Cowan.
Colonel R. E. Crompton, C.B.
A. C. Eborall.
K. Edgcumbe.
S. Z. de Ferranti.
R. K. Gray.
H. W. Kolle.
M. O'Gorman.
M. Robinson.
W. Rutherford.
H. M. Southgate.
J. J. Steinitz.

4.—Electro-Chemistry and Electro-Metallurgy.

S. O. Cowper-Coles.
H. E. Harrison.
M. Solomon.
C. W. Speirs.
F. S. Spiers.
J. Swinburne, F.R.S.
R. Threlfall, F.R.S.
J. L. F. Vogel.
E. J. Wade.

The President, President-Elect, and immediate Past-President, being *ex-officio* members of all Committees, serve on the above Committees.

MEMBERS OF COMMITTEES WHO ARE NOT SERVING ON THE COUNCIL.

*W. A. CHAMEN.
*R. A. CHATTOCK.
J. CHRISTIE.
*B. DRAKE.
J. E. EDGCOME.
H. M. ELDER.
*Prof. J. A. FLEMING, F.R.S.
*F. E. GRIPPER.
Dr. J. HARKER.
K. HEDGES.
*H. HIRST.
*T. MATHER, F.R.S.
W. R. RAWLINGS.
*Sir D. SALOMONS, Bart.
A. L. TAYLOR.
Prof. E. WILSON.
R. P. WILSON.

* Past Member of Council.

STUDENTS' SECTION.

LONDON.

Chairman: E. W. Moss.

Vice-Chairman: C. E. Abell.

Hon. Secretaries: G. H. Stevens,
266, Barry Road,
Dulwich Park, S.E.

W. Bernard Thompson,
"Ewhurst,"
Hendon, N.W.

GLASGOW.

Chairman: Prof. M. Maclean, M.A., D.Sc.

Vice-Chairmen: E. T. Goslin.
J. S. Nicholson, B.Sc.

Hon. Secretary: J. W. Campbell, B.Sc.,
Technical College,
George Street,
Glasgow.

MANCHESTER.

Chairman: The Chairman of the Manchester Local Section.

Vice-Chairman: John Roberts, Jr.

Hon. Secretary: J. Lawton Lovell,
"Hazel Dene,"
Lynwood Grove,
Sale, Cheshire.

The Institution is not, as a body, responsible for the opinions expressed by individual authors or speakers.

TABLE OF CONTENTS. No. 189.

V

BUYERS' GUIDE.

Being a Classified Index of Names, Addresses, and Specialities of Firms Advertising in this Journal.

(Where no folio is given Advertisement does not appear in this Issue.)

• ENGINEERS •

WHO APPRECIATE LABOUR-SAVING TOOLS, ESPECIALLY IN CASES OF "BREAK-DOWNS" AND REPAIRS, SHOULD SEND FOR OUR CATALOGUE OF ELECTRIC PORTABLE TOOLS, viz.,

DRILLERS, GRINDERS, POLISHERS, &c.

Our Drilling Machines are rated to drill their listed maximum size drilling hole at the rate of 1 in. per minute, *e.g.*, our $\frac{5}{8}$ in. size, weight 17 lbs., list price £13 10s., will drill $\frac{5}{8}$ in. holes in mild steel 1 in. deep per minute, with a current consumption of 180 watts.

We list upwards of 30 different types and sizes, from $\frac{1}{8}$ in. diameter holes—name-plates, &c.—weight 6 lbs., to 4 in. carriage drills, weight 3 cwt.

We also list Three Speed Machines, the various speeds being to suit metals—brass, iron and steel—and size of hole, &c.

We are also makers of Electric Grinders for Lathe Attachments and Commutator Grinding, &c., also a Portable Grinding Machine, the latter machine being invaluable to Tramway Engineers, for grinding or trueing track joints, simply connecting to the live overhead wire and earthing the opposite pole.

The machines are fitted throughout with ball races, also ball thrust, with grease chambers which are sufficiently large to carry grease for a period of several months.

Contractors to the principal Navies, Shipbuilders, Electrical Works, Railways, Bridge Builders, &c.

S. WOLF & CO., 138, SOUTHWARK ST., LONDON, S.E.

Telegrams: "Widerstand, London." A.B.C. Code 5th Ed. Telephone: 5172 Central. **Works Established 1867.**

ENAMELLED AND GALVANISED STEEL CONDUITS AND FITTINGS FOR ELECTRIC WIRING.

NEW LIST

Just Issued,

Comprising Special and Up-to-Date Designs in Fittings for Water-tight Installation.

THE NEW BROTHERTON TUBE CO., LTD.,

Commercial Road, Wolverhampton.

Offices and Stores } LONDON: 66, Victoria St., S.W. MANCHESTER: 9a & 14a, New Brown St. GLASGOW: 3, Cadogan St.

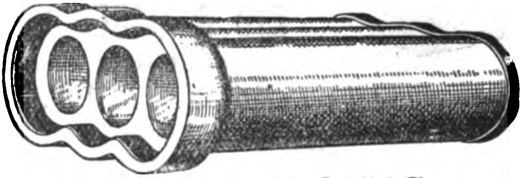
SPECIALITY.

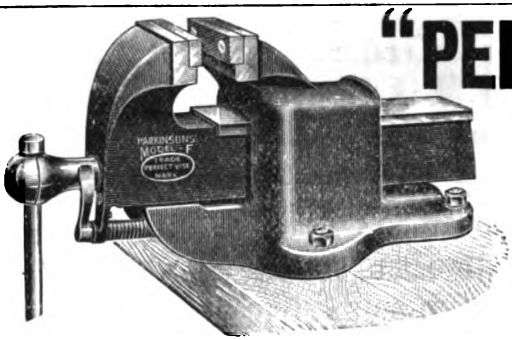
Solid Drawn Conduits in Light and Heavy Gauges, Screwed and Unscrewed.

Over 50 miles of our Heavy Gauge Conduits are installed in Savoy Hotel Extensions.

Motor Starters— PAGE
Ellison, George ....*Birmingham* 24
Oil Cans—
Kaye, J. & Sons, Ltd. ....*Leeds* 24
Oils—
Price's Patent Candle Co. Ltd. [*London, S.W.* 29
Wilcox, W. H. & Co., Ltd. ...*London, S.E.* 11
Packings—
Small & Parkes, Ltd. ....*Manchester* 22
Phosphor Bronze—
Phosphor Bronze Co., Ltd....*London, S.E.* 25
Publications—
Electrical Engineering 3
Pulleys—
Hunt, R. & Co., Ltd...............*Earls Colne* 7
Pulleys (Dynamo)—
Hunt, R. & Co., Ltd...............*Earls Colne* 7
Pumps—
Brackett, F. W. & Co. ....*Colchester* 28
Gwynnes, Ltd.....................*London, E.C.* 5
Pumping Machinery—
Boulton & Paul, Ltd..................*Norwich* 9
Shafting—
Hunt, R. & Co., Ltd...............*Earls Colne* 7
Steam Packing—
Beldam Packing & Rubber Co. [*London, E.C.* 17
Stoneware Conduits—
Albion Clay Co., Ltd. ......*Burton-on-Trent* 20
Storage Batteries—
Electrical Power Storage Co., Ltd. [*London, E.C.*
Hart Accumulator Co., Ltd....*Stratford, E.* 9
Submarine Cables—
Henley's, W. T., Telegraph Works Co., Ltd. ....*London, E.C.* 28
Switches—
Ferranti, Ltd.............*Hollinwood, Lancs.* 1
Telephone Cables—
British Insulated and Helsby Cables, Ltd. [*Prescot* 16
Henley's, W. T., Telegraph Works Co., Ltd. ....*London, E.C.* 28

Telephone Cables (*continued*)— PAGE
Land & Sea Cable Works, Ltd. [*Cologne-Nippes* 2
Telegraph Poles (Wooden)—
Grandidge, James..................*Birkenhead* 24
Grandidge, Richard...............*Birkenhead* 21
Wade, R., Sons & Co., Ltd. ....*Hull* 10
Telegraphs—
Marconi International Marine Communication Co., Ltd................*London, W.C.* 1
Tools—
Parkinson, J. & Son ....*Shipley* 23
Trolley Wire—
Smith, Frederick & Co., Wire Manufacturers, Ltd.........*Salford, Manchester* 12
Troughing (Stoneware Electrical)—
Albion Clay Co., Ltd.......*Burton-on-Trent* 20
Tubes—
Metallic Seamless Tube Co., Ltd. [*Birmingham* 2
New Brotherton Tube Co., Ltd. [*Wolverhampton* 14
Ventilating Fan Makers or Ventilating Engineers—
Electric & Ordnance Accessories Co., Ltd. [*Birmingham* 18
James Keith & Blackman Company, Ltd. [*London, E.C.* 15
Vises—
Parkinson, J. & Son......................*Shipley* 23
Watertight Plugs—
Simmonds Bros., Ltd..........*London, W.C.* 8
Wire Manufacturers—
Smith, Frederick & Co., Wire Manufacturers, Ltd......*Salford, Manchester* 12
Union Cable Co., Ltd............*London, N.* 23
Wireless Telegraphy—
Marconi International Marine Communication Co., Ltd................*London, W.C.* 1
Wooden Telegraph Poles—
Grandidge, James..................*Birkenhead* 24
Grandidge, Richard...............*Birkenhead* 21
Wade, R., Sons & Co., Ltd. ....*Hull* 10





"PERFECT" VISES

SOLD BY
3,000 DEALERS.

Makers—
J. PARKINSON & SON,
SHIPLEY, ENGLAND

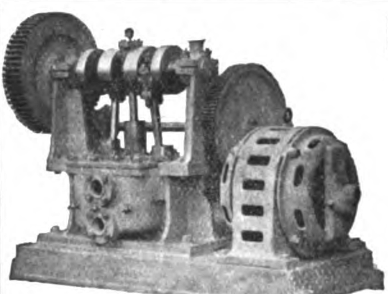
CRYSTALATE MANUFACTURING CO., LTD.

TONBRIDGE, KENT.

Manufacturers of every Description of

INSULATING FITTINGS,
SWITCHBOARD HANDLES,
BUSHES, WASHERS, &c.

Castings Insulated and Special Designs carried out.



F. W. BRACKETT & CO.

HYTHE BRIDGE IRONWORKS,
COLCHESTER.

Makers of ALL KINDS of

PUMPS

UNION CABLE Co., LTD.

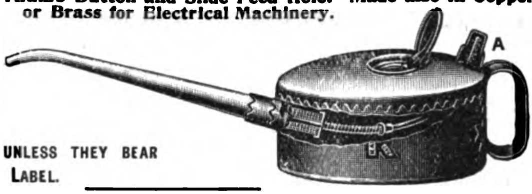
INSULATED
CABLES AND WIRES
Of Every Class.

"Cablewell." ***27, Eagle Wharf Road, N.***

K'S PATENT SERRATED STEEL SEAMLESS OIL CAN AND PATENT SEAMLESS SPOUT,

With New Patent Thumb Button and Slide Feed Hole. Made also in Copper or Brass for Electrical Machinery.

Contractors to H.M. Navy, War Department, Home Office, Indian States Railway, etc.



NONE ARE GENUINE UNLESS THEY BEAR THE *K* LABEL.

JOSEPH KAYE & SONS LTD., Lock Works, LEEDS; And at 93, High Holborn, LONDON.

JAMES GRANDIDGE,

CATHCART WHARF, BIRKENHEAD,

(Also at Wyre Dock, Fleetwood),

TIMBER MERCHANT AND CREOSOTER.

Large Stocks of

TELEGRAPH AND TELEPHONE POLES, SLEEPERS, MINING TIMBER.

Telegraphic Address—"Poles, Birkenhead." **National Telephone—1292 Birkenhead.**

Kindly Address all Letters in full, as they are liable to be misdelivered.

AUTOMATIC "NO VOLTAGE" and OVERLOAD RELEASE

STARTERS

FOR

A.C. MOTORS.

GEORGE ELLISON'S PATENT.

Protecting Polyphase Motors against "NO VOLTAGE" on ALL or on any ONE Phase, and also against OVERLOAD.

GEORGE ELLISON,

Manufacturing Electrical Engineer,

VICTORIA WORKS, 17-18, Warstone Lane, BIRMINGHAM.